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the american journal of sociology

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Embarrassment and the Analysis of Role Requirements[1]

Edward Gross and Gregory P. Stone

ABSTRACT

Since embarrassment incapacitates persons for continued role performance, it can provide an indicator of basic requirements of role performance. Study of one thousand instances of recalled embarrassment revealed three major requirements: identity, poise, and confidence in established identity and poise. The analysis of identity reveals the significance of adjunct roles and reserve and relict identities. Disturbances of poise revolve about the handling of spaces, props, equipment, clothing, and the body. Violations of confidence are prevented by performance norms. Finally, deliberate embarrassment is shown to have major social functions.

Attitudes, in the view of George Herbert Mead, are incipient acts. Meaningful discourse requires that discussants take one another's attitudes—incorporate one another's incipient activities in their conversation. Since all social transactions are marked by meaningful communication, discursive or not, whenever people come together, they bring futures into one another's presence. They are ready, balanced, *poised* for the upcoming discussion. The discussion, of course, remands futures to a momentary present, where they are always somewhat inexactly realized, and relegates them in their altered form to the collective past we call memory. New futures are constantly built up in discussions. Indeed, they must be, else the discussion is over and the transaction is ended. Without a future, there is nothing else to be done, nothing left to say. Every social transaction, therefore, requires that the participants be poised at the outset and that poise be maintained as the transaction unfolds, until there is an accord that each can turn to other things and carry other futures away to other circles.

Poise is not enough. The futures that are presented, imperfectly realized, and re-established must be relevant. Relevance is achieved by establishing the *identities* of those who are caught up in the transaction. Futures or attitudes are anchored in identities. We speak of *role* as consensual attitudes mobilized by an announced and ratified identity. In social transactions, then, persons must announce who they are to enable each one to ready himself with reference to appropriate futures, providing attitudes which others may take or assume. Often announced identities are complementary, establishing the transaction as a social relationship, for many identities presuppose counteridentities. Whether or not this is the case, the maintenance of one's identity assists the maintenance of the other.[2]

[1] Revision of a paper read at the annual meeting of the Midwest Sociological Society, April, 1963.

[2] Alfred R. Lindesmith and Anselm L. Strauss assert that every role presupposes a counter-role. There is a sense in which the assertion is correct, as in Kenneth Burke's "paradox of substance," but it may also be somewhat misleading in sociological

Furthermore, all transactions are transactions through time. It is not enough that identity and poise be established. They must be continuously reaffirmed, maintained, and provisions made for their repair in case of breakdown. Role performers count on this. We attempt here to limn the structure of transactions by examining instances where identities have been misplaced or forgotten, where poise has been lost or destroyed, or where, for any reason, confidence that identities and poise will be maintained has been undermined. We have in mind instances of embarrassment, whether or not deliberately perpetrated.

EMBARRASSMENT AND THE ANALYSIS OF ROLE REQUIREMENTS

Embarrassment exaggerates the core dimensions of social transactions, bringing them to the eye of the observer in an almost naked state. Embarrassment occurs whenever some *central* assumption in a transaction has been *unexpectedly* and unqualifiedly discredited for at least one participant. The result is that he is incapacitated for continued role performance.[3] Moreover, embarrassment is infectious. It may spread out, incapacitating others not previously incapacitated. It is destructive dis-ease. In the wreckage left by embarrasment lie the broken foundations of social transactions. By examining such ruins, the investigator can reconstruct the architecture they represent.

To explore this idea, recollections of embarrassment were expressly solicited from two groups of subjects: (1) approximately 800 students enrolled in introductory sociology courses; and (2) about 80 students enrolled in an evening extension class. Not solicited, but gratefully received, were many examples volunteered by colleagues and friends who had heard of our interest in the subject. Finally we drew upon many recollections of embarrassment we had experienced ourselves. Through these means at least one thousand specimens of embarrassment were secured.

We found that embarrassments frequently occurred in situations requiring continuous and co-ordinated role performance—speeches, ceremonies, processions, or working concerts. In such situations embarrassment is particularly noticeable because it is so devastating. Forgetting one's lines, forgetting the wedding ring, stumbling in a cafeteria line, or handing a colleague the wrong tool, when these things occur without qualification, bring the performance to an obviously premature and unexpected halt. At the same time, manifestations of the embarrassment--blushing, fumbling, stuttering, sweating[4]—coerce awareness of the social damage and the need for immediate repair. In some instances, the damage may be potentially so great that embarrassment cannot be allowed to spread among the role performers. The incapacity may be qualified, totally ignored, or pretended out of existence.[5] For example, a minister, noting the best man's frantic search for an absent wedding ring, whispers to him to ignore it, and all conspire to continue the drama with an imaginary ring. Such rescues are not always

analysis. Specifically, there is a role of cigarette smoker, but the role is not really dependent for its establishment on the counter-role of non-smoker in the sense that the parental role is dependent upon child roles and vice versa. Thus, in some social transactions the establishment and maintenance of one identity may be very helpful for the establishment and maintenance of a counter-identity; in other transactions, this may not be the case at all (see Lindesmith and Strauss, *Social Psychology* [New York: Dryden Press, 1956], pp. 379-80; and Kenneth Burke, *A Grammar of Motives* [New York: Prentice-Hall, 1945], pp. 21-58).

[3] Not all incapacitated persons are always embarrassed or embarrassing, because others have come to expect their *incapacities* and are consequently prepared for them.

[4] Erving Goffman, in "Embarrassment and Social Organization," *American Journal of Sociology*, LXII (November, 1956), 264-71, describes these manifestations vividly.

[5] A more general discussion of this phenomenon, under the rubric civil inattention is provided in Erving Goffman, *Behavior in Public Places* (New York: Free Press of Glencoe, 1963), pp. 83-88 and *passim*.

possible. Hence we suggest that every enduring social relation will provide means of preventing embarrassment, so that the entire transaction will not collapse when embarrassment occurs. A second general observation would take into account that some stages in the life cycle, for example, adolescence in our society, generate more frequent embarrassments than others. These are points to which we shall return.

To get at the content of embarrassment, we classified the instances in categories that remained as close to the specimens as possible. A total of seventy-four such categories were developed, some of which were forced choices between friends, public mistakes, exposure of false front, being caught in a cover story, misnaming, forgetting names, slips of the tongue, body exposure, invasions of others' back regions, uncontrollable laughter, drunkenness in the presence of sobriety (or vice versa), loss of visceral control, and the sudden recognition of wounds or other stigmata. Further inspection of these categories disclosed that most could be included in three general areas: (1) inappropriate identity; (2) loss of poise; (3) disturbance of the assumptions persons make about one another in social transactions.

Since embarrassment always incapacitates persons for role performance (to embarrass is, literally, to bar or stop), a close analysis of the conditions under which it occurs is especially fruitful in the revelation of the requirements *necessary* for role-playing, role-taking, role-making, and role performance in general. These role requirements are thus seen to include the establishment of identity, poise, and valid assumptions about one another among all the parties of a social transaction. We turn now to the analysis of those role requirements.

IDENTITY AND POISE

In every social transaction, selves must be established, defined, and accepted by the parties. Every person in the company of others is, in a sense, obligated to bring his best self forward to meet the selves of others also presumably best fitted to the occasion. When one is "not himself" in the presence of others who expect him to be just that, as in cases where his mood carries him away either by spontaneous seizure (uncontrollable laughter or tears) or by induced seizure (drunkenness), embarrassment ensues. Similarly, when one is "shown up" to other parties to the transaction by the exposure of unacceptable moral qualifications or inappropriate motives, embarrassment sets in all around. However, the concept, self, is a rather gross concept, and we wish to single out two phases that frequently provided focal points for embarrassment—identity and poise.[6]

Identity.— Identity is the substantive dimension of the self.

> Almost all writers using the term imply that identity establishes what and where the person is in social terms. It is not a substitute word for "self." Instead, when one has identity, he is *situated*—that is, cast in the shape of a social object by the acknowledgement of his participation or membership in social relations. One's identity is established when others *place* him as a social object by assigning the same words of identity that he appropriates for himself or *announces*. It is in the coincidence of placements and announcements that identity becomes a meaning of the self.[7]

Moreover, as we have already pointed out, identity stands at the base of role. When inappropriate identities are established or appropriate identities are lost, role performance is impossible.

If identity *locates* the person in social terms, it follows that locations or spaces emerge as symbols of identity, since social relations are spatially distributed. Moreover, as Goffman has remarked,[8] there must be a certain coherence between one's per-

[6] Other dimensions of the self—value and mood—will be taken up in subsequent publications.

[7] Gregory P. Stone, "Appearance and the Self," in Arnold Rose (ed.), *Human Behavior and Social Processes* (Boston: Houghton-Mifflin, 1962), p. 93.

[8] Erving Goffman, *The Presentation of Self in Everyday Life* (New York: Doubleday Anchor Books, 1959), p. 25.

sonal appearance and the setting in which he appears. Otherwise embarrassment may ensue with the resulting incapacitation for role performance. Sexual identity is pervasively established by personal appearance, and a frequent source of embarrassment among our subjects was the presence of one sex in a setting reserved for the other. Both men and women reported inadvertent invasions of spaces set aside for the other sex with consequent embarrassment and humiliation. The implication of such inadvertent invasions is, of course, that one literally does not know where one is, that one literally has no identity in the situation, or that the identity one is putting forward is so absurd as to render the proposed role performance totally irrelevant. Everyone is embarrassed, and such manifestations as, for example, cries and screams, heighten the dis-ease. In such situations, laughter cannot be enjoined to reduce the seriousness of the unexpected collapse of the encounter, and only flight can insure that one will not be buried in the wreckage.

To establish *what* he is in social terms, each person assembles a set of apparent[9] symbols which he carries about as he moves from transaction to transaction. Such symbols include the shaping of the hair, painting of the face, clothing, cards of identity, other contents of wallets and purses, and sundry additional marks and ornaments. The items in the set must cohere, and the set must be complete. Taken together, these apparent symbols have been called *identity documents*,[10] in that they enable others to validate announced identities. Embarrassment often resulted when our subjects made personal appearances with either invalid or incomplete identity documents. It was embarrassing for many, for example, to announce their identities as customers at restaurants or stores, perform the customer role and then, when the crucial validation of this identity was requested—the payoff—to discover that the wallet had been left at home.

Because the social participation of men in American society is relatively more frequently caught up in the central structures, for example, the structure of work, than is the social participation of women who are relatively more immersed in interpersonal relations, the identities put forward by men are often *titles;* by women, often *names*. Except for very unusual titles,[11] such identities are shared, and their presentation has the consequence of bringing people together. Names, on the other hand, mark people off from one another. So it is that a frequent source of embarrassment for women in our society occurs when they appear together in precisely the same dress. Their identity documents are invalidated. The embarrassment may be minimized, however, if the space in which they make their personal appearance is large enough. In one instance, both women met the situation by spending an entire evening on different sides of the ballroom in which their embarrassing confrontation occurred, attempting to secure validation from social circles with minimal intersection, or, at least, where intersection was temporally attenuated. Men, on the other hand, will be embarrassed if their clothing does not resemble the dress of the other men present in public and official encounters. Except for "the old school tie," their neckties seem to serve as numbers on a uniform, marking each man off from every other. Out of uni-

[9] We use the term "appearance" to designate that dimension of a social transaction given over to identifications of the participants. Apparent symbols are those symbols used to communicate such identifications. They are often non-verbal. Appearance seems, to us, a more useful term than Goffman's "front" (*ibid.*), which in everyday speech connotes misrepresentation.

[10] Erving Goffman, *Stigma* (Englewood Cliffs, N.J.: Prentice-Hall, 1963), pp. 59–62. Goffman confines the concept to personal identity, but his own discussion extends it to include matters of social identity.

[11] For example, the title, "honorary citizen of the United States," which was conferred on Winston Churchill, serves the function of a name, since Churchill is the only living recipient of the title. Compare the titles, "professor," "manager," "punch-press operator," and the like.

form, their structural membership cannot be visibly established, and role performance is rendered extremely difficult, if not impossible.[12]

Not only are identities undocumented, they are also misplaced, as in misnaming or forgetting, or other incomplete placements. One relatively frequent source of embarrassment we categorized as "damaging someone's personal representation." This included cases of ethnically colored sneers in the presence of one who, in fact, belonged to the deprecated ethnic group but did not put that identity forward, or behind-the-back slurs about a woman who turned out to be the listener's wife. The victim of such misplacement, however inadvertent, will find it difficult to continue the transaction or to present the relevant identity to the perpetrators of the embarrassment in the future. The awkwardness is reflexive. Those who are responsible for the misplacement will experience the same difficulties and dis-ease.

Other sources of embarrassment anchored in identity suggest a basic characteristic of all human transactions, which, as Strauss puts it, are "carried on in thickly peopled and complexly imaged contexts."[13] One always brings to transactions more identities than are necessary for his role performance. As a consequence, two or more roles are usually performed at once by each participant.[14]

If we designate the relevant roles in transactions as *dominant roles*[15] then we may note that *adjunct roles*—a type of side involvement, as Goffman would have it,[16] or better, a type of side *activity*—are usually performed in parallel with dominant role performance. Specifically, a lecturer may smoke cigarettes or a pipe while carrying out the dominant performance, or one may carry on a heated conversation with a passenger while operating a motor vehicle. Moreover, symbols of *reserve identities* are often carried into social transactions. Ordinarily, they are concealed, as when a court judge wears his golfing clothes beneath his robes. Finally, symbols of abandoned or *relict identities* may persist in set-

[12] The implication of the discussion is that structured activities are uniformed, while interpersonal activities emphasize individuation in dress. Erving Goffman suggests, in correspondence, that what may be reflected here is the company people keep in their transactions. The work of men in our society is ordinarily teamwork, and teams are uniformed, but housework performed by a wife is solitary work and does not require a uniformed appearance, though the "housedress" might be so regarded.

[13] Anselm L. Strauss, *Mirrors and Masks* (Glencoe, Ill.: Free Press, 1959), p. 57.

[14] This observation and the ensuing discussion constitute a contribution to and extension of present perspectives on role conflict. Most discussions conceive of such conflict as internalized contradictory obligations. They do not consider simultaneous multiple-role performances. An exception is Everett C. Hughes' discussion of the Negro physician innocently summoned to attend a prejudiced emergency case in "Dilemmas and Contradictions in Status," *American Journal of Sociology*, L (March, 1945), 353–59.

[15] We have rewritten this discussion to relate to Goffman's classification which came to our attention after we had prepared an earlier version of this article. Goffman distinguishes between what people do in transactions and what the situation calls for. He recognizes that people do many things at once in their encounters and distinguishes those activities that command most of their attention and energies from those which are less demanding of energy and time. Here, the distinction is made between *main* and *side involvements*. On the other hand, situations often call for multiple activities. Those which are central to the situation, Goffman speaks of as *dominant involvements*; others are called *subordinate involvements*. Dominant roles, therefore, are those that are central to the transactional situation—what the participants have come together to do (see Goffman, *Behavior in Public Places*, pp. 43–59).

[16] Adjunct roles are one type of side involvement or activity. We focus on them because we are concerned here with identity difficulties. There are other side activities which are not necessarily adjunct *roles*, namely, sporadic nosepicking, scratching, coughing, sneezing, or stomach growling, which are relevant to matters of embarrassment, but not to the conceptualization of the problem in these terms. Of course, such activities, insofar as they are consistently proposed and anticipated, may become incorporated in the *personal role* (always an adjunct role in official transactions), as in the case of Billy Gilbert, the fabulous sneezer.

tings where they have no relevance for dominant role performances.[17] For example, photographs of the performer as an infant may be thrust into a transaction by a doting mother or wife, or one's newly constituted household may still contain the symbols of a previous marriage.

In these respects, the probability of avoiding embarrassment is a function of at least two factors: (1) the extent to which adjunct roles, reserve identities and relict identities are not incongruent with the dominant role performance;[18] and (2) the allocation of prime attention to the dominant role performance so that less attention is directed toward adjunct role performance, reserve identities, and relict identities. Thus the professor risks embarrassment should the performance of his sex role appear to be the main activity in transactions with female students where the professorial role is dominant—for example, if the student pulls her skirt over her knees with clearly more force than necessary. The judge may not enter the courtroom in a golf cap, nor may the husband dwell on the symbols of a past marriage in the presence of a new wife while entertaining guests in his home. Similarly, should adjunct role performance prove inept, as when the smoking lecturer ignites the contents of a wastebasket or the argumentative driver fails to observe the car in front in time to avert a collision, attention is diverted from the dominant role performance. Even without the golf cap, should the judge's robe be caught so that his golfing attire is suddenly revealed in the courtroom, the transactions of the court will be disturbed. Fetishistic devotion to the symbols of relict identities by bereaved persons is embarrassing even to well-meaning visitors.

However, the matter of avoiding incongruence and allocating attention appropriately among the several identities a performer brings to a transaction verges very closely on matters of poise, as we shall see. Matters of poise converge on the necessity of controlling representations of the self, and identity-symbols are important self-representations.

Personal poise.—Presentation of the self in social transactions extends considerably beyond making the appropriate personal appearance. It includes the presentation of an entire situation. Components of situations, however, are often representations of self, and in this sense self and situation are two sides of the same coin. Personal poise refers to the performer's control over self and situation, and whatever disturbs that control, depriving the transaction, as we have said before, of any relevant future, is incapacitating and consequently embarrassing.

Loss of poise was a major dimension in our scrutiny of embarrassment, and its analysis can do much to shed light on the components of social situations—a necessary task, because the concept, "situation," is quite difficult to specify and operationalize. Working from the outside in, so to speak, we wish to single out five[19] elements of self and situation with reference to which loss of control gave rise to considerable embarrassment.

First, *spaces* must be so arranged and maintained that they are role-enabling. This is sometimes difficult to control, since people appear in spaces that belong to others, over which they exercise no authority and for which they are not responsible. Students, invited to faculty parties where faculty members behave like faculty members, will "tighten up" to the extent that the students' role performance is seriously impeded. To avoid embarrassment, people

[17] This phenomenon provides the main theme and source of horror and mystery in Daphne du Maurier's now classic *Rebecca*.

[18] Adjunct roles, reserve identities, and relict identities need not cohere with the dominant role; they simply must not clash so that the attention of participants in a transaction is not completely diverted from the dominant role performance.

[19] The five components to be discussed—spaces, props, equipment, clothing, and the body—are not offered as an exhaustive list. We have been able to distinguish close to forty such elements.

will go to great lengths to insure their appearance in appropriate places, and to some to be deprived of access to a particular setting is to limit performance drastically.

Spaces are often fixed in location and have boundaries. As such they may partake of the character of territories or domains: a particular person (or persons) is "in command" (it is "his" domain) and most familiar with it, and the territory is in continual danger of being invaded (deliberately or inadvertently). Embarrassments were reported for both of these features of space. Being "in command" and familiar with an area means knowing where the back regions are and having the right of access to them. The host at a party in his own home, however much he may be vanishing,[20] is at least the person in whose territory the gathering takes place. Should a guest spill food on his clothes, he has no choice but to suffer embarrassment for the remainder of the party. The host, by contrast, can retire to his bedroom and change his clothes quickly, often before the momentary loss of poise becomes known. A striking case of the man "in command" of a territory is the person delivering a speech to a fixed audience in a closed room. In being presented to the audience, he may even be told, "The floor is yours." To underline his exclusive domain, the speaker may wait until waiters clear the last table of cups and saucers and the doors are closed. In such a setting, where the audience is not free to leave, the speaker is now in great danger of embarrassing his audience unless his speech is such that the audience is not let down. Should he show lack of poise, the audience will feel embarrassed for him, yet be unable to escape, for that would further embarrass him. Hence they will suffer silently, hoping for a short speech.

In a situation reported to us, the discussant at a professional meeting was able to save the situation. The speaker was a man of national reputation—one of the pillars of his discipline. To everyone's dismay and embarrassment, he proceeded to give a pedestrian address of the caliber of an undergraduate term essay. Everyone hoped the discussant would save them, and he did. His tactic was to make clear to the audience that the identity presented by the speaker was not his real identity. This result he accomplished by reminding the audience of the major contributions of the speaker, by claiming the paper presented must be interpreted as evidence that the speaker was still productive, and that all could expect even more important contributions in the future. When the audience thundered applause, they were not simply expressing their agreement with the discussant's appraisal of the speaker: they were also thanking him for saving them all from embarrassment by putting the speaker back in command of the territory

We have already touched upon problems presented by invasions of spaces, and little more need be said. Persons lose poise when they discover they are in places forbidden to them, for the proscription itself means they have no identity there and hence cannot act. They can do little except withdraw quickly. It is interesting that children are continually invading the territories of others—who can control the course of a sharply hit baseball?—and part of the process of socialization consists of indications of the importance of boundaries. Whether territories are crescive or contrived affects the possibility of invasion. When they are contrived and boundaries are marked, the invader knows he has crossed the boundary and is embarrassed if caught. With crescive territories inadvertent invasions occur, as when a tourist reports discovery of a "quaint" area of the city only to be met with the sly smiles of those who know that the area is the local prostitution region.

Such considerations raise questions concerning both how boundaries are defined and how boundary violations may be pre-

[20] David Riesman, Robert J. Potter, and Jeanne Watson, "The Vanishing Host," *Human Organization*, XIX (Spring, 1960), 17–21.

vented. Walls provide physical limits, but do not necessarily prevent communications from passing through.[21] Hence walls work best when there is also tacit agreement to ignore audible communication on the other side of the wall. Embarrassment frequently occurs when persons on one side of the wall learn that intimate matters have been communicated to persons on the other side. A common protective device is for the captive listeners to become very quiet so that their receipt of the communication will not be discovered by the unsuspecting intimates. When no physical boundaries are present, a group gathered in one section of a room may have developed a common mood which is bounded by a certain space that defines the limits of their engagement to one another. The entry of someone new may be followed by an embarrassed hush. It is not necessary that the group should have been talking about that person. Rather, since moods take time to build up, it will take time for the newcomer to "get with it" and it may not be worth the group's trouble to "fill him in." However unintentionally, he has destroyed a mood that took some effort to build up and he will suffer for it, if only by being stared at or by an obvious change of subject. In some cases, when the mood is partially sustained by alcohol, one can prepare the newcomer immediately for the mood by loud shouts that the group is "three drinks ahead" of him and by thrusting a drink into his hand without delay. So, too, a function of foyers, halls, anterooms, and other buffer zones or decompression chambers around settings is to prepare such newcomers and hence reduce the likelihood of their embarrassing both themselves and those inside.

Spaces, then, include bounded areas within which transactions go on. The boundaries may be more or less sharply defined, that is, walled in or marked off by the distances that separate one encounter from another. Overstepping the bounds is a source of embarrassment, signaling a loss of poise. Consequently, the boundaries are usually controlled and patrolled and come to represent the selves of those who are authorized to cross them.

A second component of self and situation that must be controlled to maintain poise is here designated *props*. Props are arranged around settings in an orderly manner commonly called décor. Ordinarily they are not moved about during a transaction, except as emergencies arise, to facilitate the movement of people about the setting and to protect the props from damage. In some cases, their adherence to settings is guaranteed by law. Wall-to-wall carpeting, mirrors attached to walls, and curtain fixtures, for example, may not be removed from houses, even though ownership of such domestic settings may change hands. The arrangement of less adhesive props within a setting may mark off or suggest (as in the case of "room dividers") smaller subsettings facilitating the division of large assemblies into more intimate circles. Moreover, although props are ordinarily not moved about *during* transactions, they are typically rearranged or replaced between major changes of scene, marking off changes in life situations.[22]

Perhaps just because of their intimate connection with the life situations of those who control them,[23] loss of control over

[21] See Erving Goffman, *Behavior in Public Places*, pp. 151-52.

[22] David Riesman and Howard Roseborough, in a discussion of family careers, indicate the linkage between the rearrangement of props and the rearrangement of life situations. "One of our Kansas City respondents, whose existence had been wrapped up in her daughters' social life, when asked what she did when the daughters married and moved away, said that she slept more—and redecorated the living room. Still another became more active in church work—and redecorated the vestry" ("Careers and Consumer Behavior," in Lincoln Clark [ed.], *Consumer Behavior*, Vol. II: *The Life Cycle and Consumer Behavior* [New York: New York University Press, 1955], p. 14).

[23] Striking examples are provided by Harvey W. Zorbaugh in Ernest W. Burgess (ed.), *The Urban Community* (Chicago: University of Chicago Press, 1926), pp. 103-4; and in Anonymous, *Street-Walker* (New York: Gramercy Publishing Co., 1962), pp. 46-48.

props is a more frequent (though usually milder) source of embarrassment than the violation of boundaries. When one stumbles over his own furniture or slips on his own throw rug, doubt may be cast on the extent to which such props represent, in fact, the self and situation of the person or team members who have arranged them. Gifts of props are frequently embarrassing to the recipients. Thus an artist (or would-be artist) may foist a painting on a friend without recognizing that the painting is contrary to the recipient's aesthetic taste. Moreover, the artist may expect that his work will be given a prominent display commensurate with his investment. A conflict is immediately established between loyalty to the artist-friend and loyalty to the recipient's self. A way out is to include the prop in question only in those situations where the donor is present, but this may become tedious, depending on the frequency and scheduling of visiting. A classic case is the wealthy relative's gift of a self-photograph, which must be dragged out of the closet for display when the relative visits.

Clashing differences in domestic décor will usually terminate or restrict house-to-house visiting. Because of this, many wartime friendships have been abruptly ended shortly after the cessation of hostilities and demobilization. In a common military setting, servicemen would meet and become close friends, sometimes building up life-and-death obligations to one another. They would eagerly anticipate extending their hard-won intimacy into the workaday world of peacetime. Then, when they met in one or the other's home, the glaring incompatibility in décor would silently signal an incompatibility in life situation. Such embarrassing confrontations would be covered over with empty promises and futile vows to meet again, and the former friends would part as embarrassed strangers. If incompatibilities in décor can bring about the estrangement of friends who owe their lives to one another, we can see how props and their arrangement become powerful guaranties of the exclusiveness of social circles and status strata.

Much of our earlier discussion of adjunct roles, reserve identities, and relict identities applies to props. The porcelain dinnerware may always be kept visibly in reserve for special guests, and this very fact may be embarrassing to some dinner guests who are reminded that they are not so special after all, while, for other guests, anything but the everyday props would be embarrassing. Relict props also present a potential for embarrassment, persisting as they do when one's new life-situation has made them obsolete. The table at which a woman used to sit while dining with a former husband is obviously still quite serviceable, but it is probably best to buy another.

Third, every social transaction requires the manipulation of *equipment*. If props are ordinarily stationary during encounters, equipment is typically moved about, handled, or touched.[24] Equipment can range from words to physical objects, and a loss of control over such equipment is a frequent source of embarrassment. Here are included slips of the tongue, sudden dumbness when speech is called for, stalling cars in traffic, dropping bowling balls, spilling food, and tool failures. Equipment appear-

[24] Whether objects in a situation are meant to be moved, manipulated, or taken up provides an important differentiating dimension between equipment on the one hand and props (as well as clothing, to be discussed shortly) on the other. Equipment is meant to be moved, manipulated, or taken up during a social transaction whereas clothing and props are expected to remain unchanged during a social transaction but may be moved, manipulated, or taken up *between* social transactions. To change props, as in burning the portrait of an old girl friend (or to change clothes, as in taking off a necktie), signals a change in the situation. The special case of the strip-tease dancer is no exception, for her act transforms clothes into equipment. The reference above to the "stickiness" of props may now be seen as another way of describing the fact that they are not moved, manipulated, or taken up during transactions, but remain unchanged for the course of the transaction. Clothing is equally sticky but the object to which it sticks differs. Clothing sticks to the body, props stick to the settings.

ances that cast doubt on the adequacy of control are illustrated by the clanking motor, the match burning down to the fingers, tarnished silverware, or rusty work tools. Equipment sometimes extends beyond what is actually handled in the transaction to include the stage props. Indeed, items of equipment in disuse, reserve equipment, often become props—the Cadillac in the driveway or the silver service on the shelf—and there is a point at which the objects used or scheduled for use in a situation are both equipment and props. At one instant, the items of a table setting lie immobile as props; at the next, they are taken up and transformed into equipment. The close linkage of equipment and props may be responsible for the fact that *embarrassment* at times not only *infects* the participants in the transaction but the *objects* as well. For example, at a formal dinner, a speaker was discovered with his fly zipper undone. On being informed of this embarrassing oversight after he was reseated, he proceeded to make the requisite adjustments, unknowingly catching the table cloth in his trousers. When obliged to rise again at the close of the proceedings, he took the stage props with him and of course scattered the dinner tools about the setting in such a way that others were forced to doubt his control. His poise was lost in the situation.

Just as props may be adjunct to the dominant role performance, held in reserve, or relict, so may equipment. Indeed, as we have said, reserve equipment is often an important part of décor.

Fourth, *clothing* must be maintained, controlled, and coherently arranged. Its very appearance must communicate this. Torn clothing, frayed cuffs, stained neckties, and unpolished shoes are felt as embarrassing in situations where they are expected to be untorn, neat, clean, and polished. Clothing is of special importance since, as William James observed,[25] it is as much a part of the self as the body—a part of what he called the "material me." Moreover, since it is so close to the body, it conveys the impression of body maintenance, paradoxically, by concealing body-maintenance activities.[26] Hence, the double wrap—outer clothes and underclothes. Underclothes bear the marks of body maintenance and tonic state, and their unexpected exposure is a frequent source of embarrassment. The broken brassière strap sometimes produces a shift in appearance that few women (or men, for that matter) will fail to perceive as embarrassing.

Fifth, the *body* must always be in a state of readiness to act, and its appearance must make this clear. Hence any evidence of unreadiness or clumsiness is embarrassing. Examples include loss of whole body control (stumbling, trembling, or fainting), loss of visceral control (flatulence, involuntary urination, or drooling), and the communication of other "signs of the animal." The actress who is photographed from her "bad side" loses poise, for it shakes the foundation on which her fame rests. So does the person who is embarrassed about pimples, warts, or missing limbs, as well as those embarrassed in his presence.

Ordinarily, persons will avoid recognizing such stigmata, turn their eyes away, and pretend them out of existence, but on occasion stigmata will obtrude upon the situation causing embarrassment all around. A case in point was a minor flirtation reported by one of our students. Seated in a library a short distance from a beautiful girl, the student began the requisite gestural invitation to a more intimate conversation. The girl turned, smiling, to acknowledge the bid, revealing an amputated left arm. Our student's gestural line was brought to a crashing halt. Embarrassed, he abandoned the role he was building even before the foundation was

[25] William James, *Psychology* (New York: Henry Holt & Co., 1892), pp. 177–78.

[26] A complete exposition of the body-maintenance function of clothing is set forth in an advertisement for Jockey briefs, entitled: "A Frank Discussion: What Wives Should Know about Male Support," *Good Housekeeping*, May, 1963, p. 237.

laid, pretending that his inviting gestures were directed toward some imaginary audience suggested by his reading. Such stigmata publicize body-maintenance activities, and, when they are established in social transactions, interfere with role performances. The pimples on the face of the job applicant cast doubt on his maturity, and, consequently, on his qualifications for any job requiring such maturity.

All this is to say that self and situation must be in a perpetual condition of poise or readiness, adequately maintained, and in good repair. Such maintenance and the keeping of self in a state of good repair obviously require energy and time. While engaged in maintenance or repair, the person is, for that time, unable to play the role. Hence we may expect that persons will, in order to avoid casting doubt on their ability to play a role, deliberately play down or conceal maintenance and repair activity. Speakers know that spontaneity cannot be left to chance but must be prepared for, even rehearsed. Yet, obviously information on the amount of preparation it took to be spontaneous would destroy the audience's belief in the spontaneity. Outer clothes require underclothes, as social life requires an underlife (which is, of course, also social).[27]

MAINTENANCE OF CONFIDENCE

When identities have been validated and persons poised, interaction may begin. Its continuation, however, requires that a scaffolding be erected and that attention be given to preventing this scaffolding from collapsing. The scaffold develops as the relationship becomes stabilized. In time persons come to expect that the way they place the other is the way the other announces himself, and that poise will continue to be maintained. Persons now begin to count on these expectations and to have confidence in them. But at any time they may be violated. It was such violations of confidence that made up the greatest single source of embarrassment in our examples. Perhaps this is only an acknowledgment that the parties to every transaction must always maintain themselves *in role* to permit the requisite role-taking, or that identity-switching ought not be accomplished so abruptly that others are left floundering in the encounter as they grope for the new futures that the new identity implies.

This is all the more important in situations where roles are tightly linked together as in situations involving a division of labor. In one instance, a group of social scientists was presenting a progress report of research to a representative of the client subsidizing the research The principal investigator's presentation was filled out by comments from the other researchers, his professional peers. Negatively critical comments were held to a bare minimum. Suddenly the principal investigator overstepped the bounds. He made a claim that they were well on the road to confirming a hypothesis which, if confirmed, would represent a major contribution. Actually, his colleagues (our informant was one of them) knew that they were very far indeed from confirming the hypothesis They first sought to catch the leader's eye to look for a hidden message. Receiving none, they lowered their eyes to the table, bit their lips, and fell silent. In the presence of the client's representative, they felt they could not "call" their leader for that would be embarrassing, but they did seek him out immediately afterward for an explanation. The leader agreed that they

[27] Consider the fact that the physician often needs time and opportunity to consult medical books and colleagues before he can render an authoritative medical diagnosis. A structural assurance is provided by his having been taught to make diagnoses slowly. Through time thus gained, he takes advantage of informal encounters with colleagues and spare moments between patients when he can consult medical books. A direct revelation of his need for such aids and his rather unsystematic way of getting them would be embarrassing. Yet it is in the patient's best interest that they be kept secret from him, otherwise the patient would be in the position of having to pass judgment on a professional practice when he is, in fact, too involved to render an objective judgment.

were right, but said his claim was politic, that new data might well turn up, and that it was clearly too late to remedy the situation.

Careful examination of this case reveals a more basic reason for the researchers' hesitance to embarrass the leader before the client's representative. If their leader were revealed to be the kind of person who goes beyond the data (or to be a plain liar), serious question could have been raised about the kind of men who willingly work with such a person. Thus they found themselves coerced into unwilling collusion. It was not simply that their jobs depended on continued satisfaction of the client. Rather, they were unwilling to say to themselves and to the client's representative that they were the kind of researchers who would be party to a fraud. To embarrass the leader, then, would have meant embarrassing themselves by casting serious question upon their identities as researchers. Indeed, it was their desire to cling to their identities that led, not long afterward (and after several other similar experiences), to the breakup of the research team.

Just as, in time, an identity may be discredited, so too may poise be upset. Should this occur, each must be able to assume that the other will render assistance if he gets into such control trouble, and each must be secure in the knowledge that the assumption is tenable. Persons will be alert for incipient signs of such trouble—irrelevant attitudes—and attempt to avert the consequences. Goffman has provided many examples in his discussion of dramaturgical loyalty, discipline, and circumspection in the presentation of the self, pointing out protective practices that are employed, such as clearing one's throat before interrupting a conversation, knocking on doors before entering an occupied room, or begging the other's pardon before an intrusion.[28]

The danger that one's confidence in the other's continued identity or his ability to maintain his poise may be destroyed leads to the generation of a set of *performance norms*. These are social protections against embarrassment.[29] If persons adhere to them, the probability of embarrassment is reduced. We discovered two major performance norms.

First, *the standards of role performance almost always allow for flexibility and tolerance*. One is rarely, if ever, totally in role (an exception might be highly ritualized performances, where to acknowledge breaches of expectation is devastatingly embarrassing.[30] To illustrate, we expect one another to give attention to what is going on in our transactions, but the attention we anticipate is always *optimal*, never total. To lock the other person completely in one's glance and refuse to let go is very embarrassing. A rigid attention is coerced eventuating in a loss of poise. One is rapt in the other's future and deprived of control almost like the hypnotist's subject. Similarly, never to give one's attention to the other is role-incapacitating. If one focuses his gaze not on the other's eyes, but on his forehead, let us say, the encounter is visibly disturbed.[31] Norms allowing for flexibility and tolerance permit the parties to social transactions ordinarily to assume that they will not be held to rigid standards of conduct and that temporary lapses will be overlooked. The norm is respected by drinking companions who both understand how it is to have had a drop too much and who can also be counted on not to hold another to everything he says, does, or suggests.

[28] Goffman, *The Presentation of Self in Everyday Life*, pp. 212–33.

[29] Implicit in Georg Simmel, *The Sociology of Georg Simmel*, trans. Kurt H. Wolff (Glencoe, Ill.: Free Press, 1950), p. 308.

[30] See the discussion of "role distance" in Erving Goffman, *Encounters* (Indianapolis, Ind.: Bobbs-Merrill Co., 1961), pp. 105–52.

[31] Here we are speaking of what Edward T. Hall calls the "gaze line." He points out there are cultural variations in this phenomenon. See his "A System for the Notation of Proxemic Behavior," *American Anthropologist*, LXV (October, 1963), 1012–14.

So, too, colleagues are persons who know enough to embarrass one another but can ordinarily be trusted not to do so. The exclusiveness of colleague groups can be seen, therefore, as a collective defense against embarrassment.

The second performance norm was that of *giving the other fellow the benefit of the doubt*. For the transaction to go on at all, one has at least to give the other fellow a *chance* to play the role he seeks to play. Clearly, if everyone went around watching for chances to embarrass others, so many would be incapacitated for role performance that society would collapse. Such considerate behavior is probably characteristic of all human society, because of the dependence of social relations on role performance. A part of socialization, therefore, must deal with the prevention of embarrassment by the teaching of tact. People must learn not only not to embarrass others, but to ignore the lapses that can be embarrassing whenever they occur. In addition, people must learn to *cope* with embarrassment. Consequently, embarrassment will occasionally be deliberately perpetrated to ready people for role incapacitation when it occurs.

DELIBERATE EMBARRASSMENT

Although we have emphasized up to this point instances of embarrassment which arise from wholly unexpected acts and revelations, the unexpected is often deliberately perpetrated. Examples are practical jokes, teasing, initiation into secret societies, puncturing false fronts, and public degradation. Since embarrassment appears to represent social damage that is not at all easily repaired, we might well ask why the condition may be deliberately established. The embarrassed person stands exposed as incapable of continued role performance—a person who cannot be depended upon. In his presence, all must pause and review their assessments and expectations. Whatever they decide, the transaction is halted, and those dependent upon it are deprived of the realization of the futures that they have entrusted to others.

Embarrassments, therefore, always have careers. One person embarrasses others whose hurried attempts to salvage the situation merely call further attention to the embarrassment. A point may be reached where no repair is possible—the embarrassed person breaks into tears, flees, or, in the classic case, commits suicide—not to save face, but because face has been destroyed beyond repair.[22] Other terminations are possible, as we have shown. The embarrassing situation may be transformed by humor—laughed off—to define it as unserious and to invite others to symbolize their solidarity with the embarrassed person by joining in the laughter.[23] Blame may be diverted away from the transaction and placed on others on the outside. The embarrassed one may fall sick. There are numerous outcomes, and, while some are less drastic than others, none is completely devoid of risk. Why is it, then, that embarassment may be deliberately perpetrated? There are at least three reasons or social functions that may be attributed to deliberate embarrassment.

First, since embarrassing situations are inevitable in social life, persons must be schooled to maintain poise when poise is threatened, to maintain the identities they have established in social situations in the face of discreditation, and to sustain the confidence others have built up about matters. Deliberate embarrassment acts to socialize young people with these skills. Consequently, all young children trip one another, push, disarrange one another's clothing and other items of personal appearance. Besides being fun, such play[24]

[22] Goffman, "On Face-Work," *Psychiatry*, XVIII (August, 1955), 213-31.

[23] See Ruth Laub Coser, "Some Social Functions of Laughter: A Study of Humor in a Hospital Setting," *Human Relations*, XII (May, 1959), 171-82.

[24] Careful attention must be given to all phases of children's play, which includes very much more than the anticipatory and fantastic dramas emphasized by George H. Mead.

socializes the child in the maintenance of poise despite direct physical attacks on his "balance." Indeed, young children will spin about, inducing dizziness as they unknowingly test their ability to handle the imbalance in the play that Roger Caillois speaks of as *ilinx* or vertigo.[35] But socialization continues throughout life, and adult men, for example, test who can maintain poise in the face of the other's loss by playing at "drinking the other under the table." The roller coaster and tilt-a-whirl, and less upsetting machines like the merry-go-round and ferris wheel can be interpreted as a technology available to test poise.[36] Almost by definition, every game is a test of poise, but some sports place particular emphasis upon such tests—ski-jumping and gymnastics.[37] Announced identities are also challenged and impugned in play, as in "name-calling," and such teasing often reaches out to call into question everything one seeks to establish about himself in social encounters:

> Shame! Shame! Double shame!
> Everybody knows your name!

The child, of course, learns the institutionalized replies to such tests of identity and self-confidence which throw the challenge back:

> My name, my name is Puddin' Tame.
> Ask me again and I'll tell you the same!

As others have noted, the challenges and responses inherent in such tests of poise, identity, and self-confidence often assume a pattern of interactive insult. The cla case is "playing the dozens."[38]

If one function of deliberate embarr ment is socialization, we would guess t such tests would be concentrated in formative years and in other periods major status passage. Our survey of adı in the evening extension class showed t to be true. When we asked them to rec the time of their lives when they were f quently embarrassed, the adolescent ye were most commonly mentioned. Instan of deliberate embarrassment also includ hazings and the humiliation which acco panied socialization into the armed forc It may well be that every move into . established social world—every major *r de passage*—is facilitated by the delibera perpetration of embarrassing tests of poi identity, and self-knowledge.[39]

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[35] "The Structure and Classification of Games," *Diogenes*, No. 12 (Winter, 1955), pp. 62–75.

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[37] Poise is an essential part of the commercialized tumbling exhibitions we call wrestling. Interviews with professional wrestlers by one of the writers establish that the most feared "opponent" is not at all the most fierce, but the neophyte, upon whose poise the established professional cannot rely.

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performing his role, it can clearly be used to stop someone from playing a role that might discredit a collectivity. Empirical categories include public reprimands, exposure of false fronts, open gossip and cattiness, or embarrassment perpetrated as a retaliation for an earlier embarrassment. In some of these cases, a person is exposed as having no right to play the role he has laid claim to, because the identity in which his role is anchored is invalid. In others, the person is punished by terminating his role performance so that he can no longer enjoy its perquisites.

A third function of deliberate embarrassment is the establishment and maintenance of power. The technique here is rather more subtle than those we have discussed. Specifically, the scene may be laid for embarrassment so that only by following the line established by the one who sets the scene may embarrassment be avoided. In this case, one assures himself that his decision will carry the day by guaranteeing that any alternative will result in irreparable damage to the whole collectivity. Organizational policy changes, for example, may be accomplished by cloaking them in a cover story impregnated with the organizational ideology. To resist the proposed changes, consequently, risks the discreditation of the entire organization. Another example is to be found in "kicking an official upstairs." The decision will be reached in a policy-making discussion where the official in question may be present. In the discussion, emphasis will be given to the official's qualifications for the new post so that the "stage manager" leads a new self forward to replace the old self of the official in question. Discreditation of the new self, particularly in the official's presence, would wreak such damage on the transaction that it must be foregone and the "manager's" decision conceded.[40]

CONCLUSION

In this paper, we have inquired into the conditions necessary for role performance. Embarrassment has been employed as a sensitive indicator of those conditions, for that which embarrasses incapacitates role performance. Our data have led us to describe the conditions for role performance in terms of identity, poise, and sustained confidence in one another. When these become disturbed and discredited, role performance cannot continue. Consequently, provisions for the avoidance or prevention of embarrassment, or quick recovery from embarrassment when it does occur are of key importance to any society or social transaction, and devices to insure the avoidance and minimization of embarrassment will be part of every persisting social relationship. Specifically, tests of identity, poise, and self-knowledge will be institutionalized in every society. Such devices, like all mechanisms of social control, are capable of manipulation and may well be exploited to establish and maintain power in social transactions. Yet, deliberate or not, embarrassment is as general a sociological concept as is role.

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Fertility and Family Planning in Taiwan: A Case Study of the Demographic Transition

Ronald Freedman, John Y. Takeshita, and T. H. Sun[1]

ABSTRACT

Taiwan is at that stage of the demographic transition in which falling mortality exerts pressures on traditional family forms. There is evidence that efforts are being made to reduce fertility by couples in the latter half of the child-bearing period when they have the moderate number of children and sons most of them want. The extent of such family-limitation efforts, the number of children wanted, and the number of children born are shown to be related to a number of modernization indexes.

Profound social changes probably are involved in that stage of the demographic transition in which mortality already has fallen to low levels and fertility has just begun to fall. We have an excellent opportunity to study such a period of change in Taiwan, where mortality is low and fertility has just begun to fall and is likely to decline further.

While many Western countries have passed through this stage, the description of the process and how various population strata are affected by it is largely speculative. This is one of many areas in the social sciences in which many believe that they know the facts but hard evidence is actually very scarce. In the case of Taiwan, we have good sources of data. The official population register for the whole island is unusually good for a society of this type.[2] In addition, we now have for Taichung, Taiwan's provincial capital and fourth largest city, a large body of sample survey data for a probability sample of about 2,500 married women 20–39 years old. These are unique data for a Chinese, and in some respects for any, high-fertility population.

Low mortality and a significant amount of social and economic development probably are preconditions for a significant fall in fertility. If so, these preconditions are realized in Taiwan.[3] Mortality has been low for some time. The great majority of children now survive to become adults so that it is no longer necessary to have a large number of babies to have three or four survive to adulthood. This was recognized as a fact by 75 per cent of the respondents in our survey. It is likely that

[1] The research on which this paper is based was carried out at the Taiwan Population Studies Center and the University of Michigan Population Studies Center. The Taiwan Center is associated with the Maternal and Child Health Institute of the Provincial Health Department of Taiwan. Financial support for the basic research activity has come from the Population Council and the Ford Foundation. We are grateful to many people in Taiwan for their assistance, especially Dr. T. C. Hsu, Commissioner of Health; Director C. L. Chen and Dr. J. Y. Peng of the Maternal and Child Health Institute; S. Y. Soong and Y. F. Liu of the Civil Affairs Department in charge of registration; and Dr. S. C. Hsu of the Joint Commission on Rural Reconstruction. Solomon Chu assisted with computations at the University of Michigan.

[2] There are deficiencies in the registration system, especially with regard to infant deaths and the births associated with them, but we do not believe that these deficiencies seriously affect our conclusions about fertility trends. For a critique of the register at an earlier stage see George Barclay, *A Report on Taiwan's Population to the Joint Commission on Rural Reconstruction* (Princeton, N.J.: Office for Population Research, 1954). We expect to be able to make a fairly definitive evaluation of the registration system in Taichung by a case-by-case comparison of survey data and the register on births, deaths of children, education of husband and wife, composition of the household, and its place of registration.

[3] Cf. R. Freedman, J. Y. Peng, J. Y. Takeshita, and T. H. Sun, "Fertility Trends in Taiwan: Tradition and Change," *Population Studies*, XVI (March, 1963), 219–36.

increasing numbers of living children produce an increasing pressure not only on limited housing but also on all kinds of physical and social arrangements developed in a period when higher mortality restricted family size.

There has been considerable social and economic development in Taiwan since the war, in some areas continuing progress begun under the Japanese. Table 1 shows some of the changes between 1952 and 1961 on a number of significant measures of development and modernization. From these and other measures, it can be shown that Taiwan now is considerably in advance of the average level for developing high-fertility countries. Taiwan is ready for a fertility decline if its relatively high levels of the following indexes are relevant: urbanization, non-agricultural employment, highly productive agriculture oriented to a market, education, literacy, circulation of mass media, internal written communications. Taken together, these various indexes indicate involvement of the population in a system of social interactions transcending traditional local and familial units.

Whatever the cause, it is clear that a decline in fertility is underway. Fertility has decreased all over the island each year since 1958, as has been shown in detail elsewhere.[4] The fertility changes from 1958 to 1962 are as follows:[5]

	Percentage change 1958-62
Total fertility rate	−11
General fertility rate	−5
Age specific birth rates:	
15-19	+5
20-24	+3
25-29	+3
30-34	−16
35-39	−27
40-44	−28
45-49	−28

The pattern of decline by age is especially significant. It shows increasingly marked declines for women at ages over 30 but no significant decline in the younger ages. This is exactly what would be expected if many women wanted a moderate number of living children, had them by age 30, and then tried to limit family size in some way. There is fragmentary evidence for the existence of such a pattern at the beginning of the fertility decline in a number of Western countries. Such a pattern of behavior is confirmed directly in data from a sample survey in Taichung, a city with a population of over 300,000.

The Taichung survey is based on a probability sample of the married women 20-39 years old in Taichung City.[6] Between October and December of 1962 1,367 were interviewed with a long schedule of questions—an interview of more than an hour in length. In January, 1963, a supplementary sample of 1,065 married women with at least two children were given a shorter interview—a selection of questions from the longer interview.[7] Cooperation of the respondents was excellent: 97 per cent of the eligible respondents were interviewed, less than 1 per cent

[4] *Ibid.*

[5] These data are from the population register and were kindly provided by the Department of Civil Affairs, Province of Taiwan.

[6] This survey is the "before" bench-mark measurement for a family-planning action study which is part of the maternal and child health service of the Taiwan Provincial Health Department. After completion of the first survey a program designed to reach large numbers of the women in Taichung was undertaken under controlled conditions. Then the women in the first survey were reinterviewed. One aim of this experimental study was to discover whether the modernization variables found to be related to fertility in the present article also appear important in the response to the experimental program for adoption of family planning.

[7] The two samples were combined by weighting up by two the interviews from the first survey with wives having less than two living children (a rather small minority). The frequencies shown for the total sample are based on this "inflated" sample of 2,713 cases. As we actually interviewed only 2,432 women, frequencies in the total sample should be deflated by approximately 10 per cent. The second set of interviews being shorter than the first some of the data presented are based only on the first sample.

refused interviews, and 2 per cent were absent temporarily.

The present article presents the first report on selected results of this survey. Because the data are unique in many ways and because they have a topical value in view of a possible impending fertility decline in Taiwan, they are presented prior to a multivariate analysis which may clarify a number of issues but which will take some time to complete.

We discuss first what the survey data show about fertility values and family limitation in the child-bearing population as a whole. Then we consider how such fertility values and behavior vary in relation to indexes of modernization.

FERTILITY ATTITUDES AND FAMILY PLANNING

The Taichung survey data show that (1) there is strong consensus that a moderate number of sons and children is desirable; (2) a large proportion of wives has the desired number of living sons and children by their early thirties; (3) the overwhelming majority approves of the idea of family planning; and (4) a significant

TABLE 1

TRENDS IN SOME INDEXES OF SOCIAL DEVELOPMENT FOR TAIWAN (1952–61)

Year	Percentage Living in Five Largest Cities*	Percentage over 12 Literate		Percentage over 12 Completed Primary Education		Percentage over 12 with Education beyond Primary School		Per 1,000 Circulation of Daily Newspapers†	Per Capita Postings of Domestic Mails per Year†	Percentage Male Labor Force over 12 in Nonagricultural Occupations‡
		M	F	M	F	M	F			
1952	18	N.a	N.a.	53§	28§	17	7	28	8	44
1958	20	60	40	60	35	23	10	43	31	49
1961	21	63	44	63	40	26	12	49‖	33#	52

* Unless otherwise noted, all data are from the publications of the Department of Civil Affairs, Province of Taiwan, and are based on the register of population

† Data from United Nations *Statistical Yearbook*

‡ Excludes students, those engaged in "household management," and those listed as having no occupation

§ For 1952 it was necessary to assume that only persons over 12 had completed primary school

‖ For 1959.

\# For 1960.

TABLE 2

NUMBER OF CHILDREN WIFE WANTS AND NUMBER SHE HAS LIVING, FOR WIVES 30–34 AND 35–39 YEARS OLD*

Age	No. Children Wife Wants or Has							
	0	1	2	3	4	5	6 or More	Up to God, Fate, etc.
30–34 (N=741)								
Per cent who want		1	7	23	38	20	9	2
Per cent who have	3	5	8	21	27	21	15	......
35–39 (N=619)								
Per cent who want		1	7	19	40	18	11	4
Per cent who have	3	6	5	13	18	23	32	......

* All data in this and the following tables are from the sample surveys in Taichung described above.

minority does something about limiting the number of their children when their goals for family size are attained.

TABLE 3

PROPORTION OF WIVES (AGE 30 39) WHO HAVE NUMBER OF CHILDREN THEY WANT, OR PREFER FEWER OR MORE, TO NUMBER OF LIVING CHILDREN*

No. of Living Children	Per Cent			N
	Prefers Fewer Children	Has No She Wants	Prefers More Children	
0			100	36
1	.		99	72
2		24	76	91
3	5	49	46	226
4	14	62	24	304
5	47	41	12	292
6 or more	74	21	5	256

* For respondents giving definite numerical response. Excludes those giving answers like "up to God," "up to nature," etc., or no answer.

who want that many. The proportion of wives who want no more children rises sharply with the number of living children, and among those with six living children a majority would prefer fewer (Table 3).

In Taiwan, as in many high-fertility countries, it is regarded as very important to have one and preferably two sons. Ninety-two per cent of the wives in the survey regarded it as important for a family to have a male heir. The modal ideal of this population is a family with three or four children including two sons. But under current mortality and fertility these traditional values about sons are achieved relatively early in life. The proportion with two or more living sons rises from 17 per cent at age 20-24 to 68 per cent at age 30-34 and 74 per cent at age 35-39. Sixty per cent have at least one son at age 20–24, and 92 per cent have at least one son by age 35–39. In short, by the time women are in their early thirties, and often earlier, they have alive the children and the sons they want. Those with larger numbers of sons do not regard themselves as especially fortunate. In fact, the great majority of those with more than three sons are willing to say that they would have preferred fewer (see Table 4). Only 18 per cent of those with two sons and 2 per cent of those with three sons wanted more.

TABLE 4

COMPARISON OF NUMBER OF SONS WIFE CONSIDERS IDEAL FOR HERSELF WITH NUMBER SHE HAS

No. of Living Sons	Ideal No. Less Than Has	No. Has Is Ideal	Ideal No. Greater Than Has	No Definite Ideal No. Given	N
0			94	6	549
1		11	82	7	743
2	2	75	18	5	792
3	39	55	2	4	389
4	73	22	1	4	169
5 or more	83	9	1	7	70

Very few of the wives want very large families or feel that it is all up to fate (see Table 2). To be childless is considered a tragedy, one child definitely is not enough, and few mothers are satisfied with two. But the modal numbers desired are three and four—not six or more. In Tables 2 and 3 there are comparisons of the distributions of numbers of living children and of children wanted. Clearly, by the time women are in their thirties, the proportion who have five or more children alive is much greater than the proportion

The moderate number of sons and children wanted in Taichung now does not necessarily represent a change from fifty

or a hundred years ago. At that time a much larger number of births was needed to provide the smaller number of living children and sons probably regarded as essential then, as now. It may well be that the apparent stress in the culture at that time on unlimited fertility was a way of guaranteeing the essential minimum living number.

trast to many reports that many women in other developing, high-fertility areas would prefer to have fewer than the large number they have borne but relatively few have done anything about it.[8]

Thirty-two per cent of all women had used a family-limiting procedure, that is, sterilization, induced abortion, or contraception. The proportion rises to 47 per cent among women in their thirties with at least three living children. As we shall see later, the proportion is as high as 79 per cent among better-educated couples with at least three children and two living sons.

TABLE 5

PROPORTION WHO HAVE PRACTICED ANY FORM OF FAMILY LIMITATION*

No. Living Children	No. Preferred†	Wife's Age: 20–29		Wife's Age: 30–39	
		Percentage Who Have Done Something To Limit Family Size	No. of Couples	Percentage Who Have Done Something To Limit Family Size	No. of Couples
0	More than 0	4	126	6	36
1	More than 1	9	310	33	72
2	2	42	26	69	22
	More than 2	15	325	18	69
3 . .	Less than 3	33	3	82	11
	3	47	88	59	111
	More than 3	11	178	31	104
4	Less than 4	47	15	80	44
	4	37	95	55	188
	More than 4	19	52	25	72
5	Less than 5	47	30	57	136
	5	43	21	44	121
	More than 5	12	8	17	35
6	Less than 6	22	13	51	189
	6	0	2	23	55
	More than 6	0	1	0	12

* Includes sterilization, induced abortion, and contraception.

† Excludes 84 cases giving answers like "up to God," "up to nature," etc., and 17 giving no answer.

Since so many couples are having the children and sons they want relatively early in marriage it is not too surprising that the great majority (92 per cent) of the wives in Taichung approve of the idea of doing something to limit family size. The proportion of couples who in fact have done something to limit family size increases sharply as the number of children wanted is attained and then exceeded (Table 5). For example, among women in their thirties with three or more children, a majority report some limiting practice if the number of living children they have exceeds the number wanted. This is in contrast to many reports that many women in other developing, high-fertility areas would prefer to have fewer than the large number they have borne but relatively few have done anything about it.[8]

Although 24 per cent of the couples had used contraception at some time, only 17

[8] This pattern is reported in studies in India, Pakistan, Puerto Rico, Jamaica, and elsewhere. In contrast to India and Pakistan, Taiwan has much lower mortality and is more developed in terms of criteria like literacy, urbanization, etc. But it is not clear why it should differ from Puerto Rico, for example.

per cent reported current use at the time of the survey. Contraceptive practice is still in many cases intermittent, relatively ineffective, based on inadequate information, and often initiated only after the couple has more children than are wanted. Otherwise fertility would be lower than it is. Under these circumstances it is not surprising that many couples resort to limiting practices they consider undesirable. Although the overwhelming majority expressed disapproval of abortion and although induced abortions are illegal, 11 per cent of all wives and 15 per cent of those 35–39 reported at least one induced abortion. Eight per cent of all wives and 16 per cent of those 35–39 reported that they or their husbands had been sterilized. Induced abortion apparently is not regarded by many as an adequate single solution to the problem of family limitation, since 82 per cent of those who had had at least one induced abortion either were subsequently sterilized or also made use of contraception. Kingsley Davis has recently supported the thesis that induced abortion is used widely to solve the problem of too many living children during this stage of the demographic transition.[9] As we shall see below those who used this method exclusively are concentrated in the least modernized sectors of the population.

The prevalence of sterilization as a form of family limitation is probably related to the relatively good supply of medical personnel and facilities in Taiwan in general and in Taichung in particular.

A large part of the population is becoming aware of the prevalence of family-limitation practices. Whether or not the perceptions are correct, they probably provide social support for individual action in adopting such practices. In answer to the question: "How many people in Taiwan do you think are doing something to keep from getting pregnant too often or from having more children than they want?" 44 per cent said "many"; 20 per cent said "some"; 2 per cent said "none"; and 21 per cent said that they did not know.

[9] Kingsley Davis, "The Theory of Change and Response in Modern Demographic History," *Population Index*, XXIX (October, 1963), 345–66.

MODERNIZATION AND FERTILTY DIFFERENTIALS

Which groups or strata in Taichung have lowest fertility and lead the way in adopting family limitation? We can show by various other criteria that such modern fertility behavior is most characteristic of population groups in the most modernized sector. But first let us specify that in characterizing groups as having modern fertility behavior we mean that: they want the fewest children; more of those they have survive; they are more likely to do something to limit family size; and, if they resort to induced abortion, they are also likely to use contraception or to follow abortion with sterilization. The net result is lower fertility. We shall refer to this complex of fertility norms and behaviors as "the low-fertility complex."

Which groups are characterized by the low-fertility complex? As an initial summary we can say that it is most characteristic of:

- The better-educated and those who read the mass media
- Those with no farm experience, especially migrants from large cities to Taichung
- Those who own more modern objects of consumption
- Those who are less favorable to traditional Chinese family values
- Those living in nuclear rather than stem or joint families
- Those who work in an impersonal setting as employees of non-relatives
- Those who have received information about family planning from multiple sources

The use of *some* form of family limitation is more consistently related to the indexes of modernization than any *one* of the three major types of limitation considered or the measures of actual or desired fertility. We interpret this to mean that those with more modern characteristics try various methods to limit family

size, but their practice of these methods frequently is either so late or so ineffective that it does not limit fertility to the desired level, although it does have the effect of producing a rather consistent fertility differential. That the practice of family limitation is still ineffective is indicated by the fact that for women age 35–39 the number of children born or living is higher than desired for almost every category of each of the thirteen social and economic characteristics considered. This is true despite the fact that in some of the advanced categories more than 75 per cent use some method, including sterilization for as much as 25 per cent of the wives in some subcategories. Modern family planning, if practiced, is begun late. Seventy-one per cent of those who have used contraception did not begin until after at least four pregnancies, and more than half waited until after five or more pregnancies. It would appear that we have here the demographic stage at which there is a groping for effective ways to achieve the desired family size. Those with the more modern characteristics do succeed in reducing family size to a greater degree than others but still not to the level they desire.

These rather sweeping generalizations summarize a large body of data. In Tables 6 and 7 a series of modernization variables are shown, in relation to a standard set of dependent fertility variables, for wives 35–39 years old in the Taichung survey. By this age almost all the marriages have lasted long enough for differentials to be apparent and for discrepancies between desired and actual fertility to have developed. In general, the relationships shown illustratively in these tables for wives 35–39 years old also appear in less pronounced and consistent form for women at younger ages (20–24, 25–29, 30–34).

EDUCATION AND THE MASS MEDIA

Education and reading of the mass media may be particularly strategic in the development process and in fertility decline.[10] As Table 6 (Panels A and B) indicates, there is a strong relation between the low-fertility complex and either increasing education or frequency of reading newspapers. In Table 7 we show how the practice of family limitation is related to educational level for those who have at least three living children and two living sons—the numbers that would satisfy most couples. Again there is a very strong relationship of education to this part of the low-fertility complex. However, it is noteworthy that with the attainment of three children and two sons a substantial minority of even those with no education try to limit family size.

The use of each of the three major family limitation methods increases with education, but the increase is especially marked for contraception and sterilization—and this is true for most of the modernization indexes considered. Although many of the better-educated have induced abortions, they are less likely than the less-educated to have restricted their practice of family planning to abortion. As the last column in Table 7 indicates, the proportion of those with an abortion who have never used another method decreases sharply with education. The relationship is similar for most other indexes of modernization. Sterilization is also associated with education and readership (as with most other modernization indexes). This is probably in large part an economic matter. The cost of sterilization makes this method less accessible to lower-status groups.

CONSUMPTION OF MODERN OBJECTS

The respondents were asked whether the household owned any of nine modern objects of consumption (identified in Table 6, Panel C). The number of such objects owned may be treated as a rough index of the involvement of the household in a market economy transcending the local and familial. It is also a rough index of the attainment of a modern standard of living. This index is more closely and consistently

[10] R. Freedman, "Norms for Family Size in Underdeveloped Areas," *Proceedings of the Royal Society, B*, Vol. 159, 220–45, 1963.

TABLE 6

MEASURES OF ACTUAL AND DESIRED FERTILITY, SURVIVAL RATES FOR CHILREN, PROPORTIONS USING VARIOUS TYPES OF FAMILY LIMITATION, BY VARIOUS INDEXES OF MODERNIZATION FOR WIVES 35-39 YEARS OLD

Indexes of Modernization	No. of Couples	Mean No. of Live Births	Mean No. of Living Children	Survival Rate for Children	Mean No. of Children Wife Wants	Percentage Using: Sterilization	Abortion	Contraception	Any of Three	Percentage of Those with Abortion Never Using Other Methods
A. Wife's education:										
None	238	5.7	4.9	0.86	4.4	10	8	13	26	35
Primary, not graduated	42	5.2	4.7	90	4.2	26	17	19	48	*
Primary, graduated	187	5.3	4.8	90	4.2	18	18	32	51	9
Junior level or senior not graduated	77	4.5	4.1	91	3.6	22	17	42	66	5
Graduate, senior level or more	75	3.6	3.4	94	3.3	21	28	53	76	5
B. Wife's frequency of newspaper reading:										
Cannot read§	265	5.8	5.0	86	4.5	11	8	18	31	24
Never, but can read	71	5.8	5.1	89	4.2	16	17	13	34	33
Occasional	66	5.0	4.6	92	4.1	24	26	33	61	12
Once a week	33	4.7	4.2	89	3.8	21	21	27	48	*
3 or 4 times a week	41	3.6	3.3	92	3.6	12	7	37	49	*
Every day	141	4.2	3.9	93	3.0	21	23	48	71	9
C. No. of modern objects owned ‡										
0 or 1	48	6.4	5.2	82	4.9	0	10	19	25	*
2	72	5.6	4.8	85	4.4	7	10	14	22	*
3	87	5.6	4.7	85	4.2	10	9	18	31	25
4	84	5.3	4.9	93	4.2	20	14	14	36	8
5	95	4.6	4.1	89	4.0	17	14	24	43	15
6	140	4.7	4.4	92	3.9	21	16	39	63	14
7 or more	93	4.7	4.3	92	3.9	25	29	48	75	7
D. Household income:										
Under 1,000 NT	61	5.3	4.6	87	4.3	7	3	15	23	*
1,000-1,499 NT	109	5.2	4.6	89	4.0	16	9	23	40	20
1,500-1,999 NT	52	4.7	4.3	92	3.9	25	25	38	64	0
2,000 or more	80	4.9	4.5	92	4.1	15	13	35	68	0
E. Current farm status:										
Farm owner or tenant	56	6.1	5.4	88	4.6	11	7	16	29	25
Not a farmer	263	4.8	4.3	90	4.0	17	14	32	49	5
F. Couple's rural background:										
Both have rural background	355	5.4	4.8	.88	4.2	14	16	24	40	16
Only wife	71	5.1	4.4	87	3.9	14	13	32	51	*
Only husband	70	4.5	4.1	93	3.6	21	11	37	58	*
Neither	108	4.8	4.3	90	4.1	22	16	30	53	6
G. Wife's community background:										
Taichung all life:										
No rural background	17	4.4	3.9	90	4.2	16	5	21	47	*
Some rural background	66	5.8	5.0	86	4.4	13	13	21	35	11
Migrant to Taichung, not rural:										
From city	34	3.5	3.4	96	3.3	26	10	36	62	*
From small town	33	5.6	5.0	0.90	4.3	14	14	29	43	*

* Base less than 10

† Cannot read either Chinese or Japanese

‡ The objects are: bicycle, radio with record player, radio, electric fan, sewing machine, electric iron, clock or watch, electric cooking pan, motorcycle.

TABLE 6—*Continued*

Indexes of Modernization	No. of Couples	Mean No. of Live Births	Mean No. of Living Children	Survival Rate for Children	Mean No. of Children Wife Wants	Percentage Using: Sterilization	Abortion	Contraception	Any of three	Percentage of Those with Abortion Never Using Other Methods
Migrant to Taichung, at least some rural background, covering most life:										
Neither before nor after marriage	52	4.1	3.7	0.89	3.6	11	13	40	53	*
Before marriage only	76	5.1	4.6	.91	4.1	17	16	28	47	8
Both before and after marriage	32	6.1	5.6	.91	4.6	3	3	12	15	*
H. Type of family:										
Joint	27	5.2	4.8	.93	4.4	7	.	15	22	*
Stem	117	5.4	4.8	.88	4.3	19	13	20	40	14
Nuclear	475	5.1	4.5	.89	4.0	16	16	30	49	15
I. Wife's attitude toward living with children and support from them:										
Most traditional (1)	385	5.5	4.8	.87	4.1	12	11	23	37	21
(2)	89	4.7	4.3	.92	4.0	21	18	28	53	19
(3)	76	4.5	4.1	.92	3.5	26	13	33	50	0
(4)	27	4.1	3.9	.95	3.4	18	41	44	70	18
Least traditional (5)	39	4.7	4.4	.94	3.5	20	33	54	74	0
J. Husband's employment status:§										
Farmer	97	6.3	5.5	.87	4.8	9	10	16	28	21
Self-employed (nonprofessional)	153	5.8	5.0	.86	4.4	14	14	21	39	24
Family or relative's business	13	5.2	5.0	.96	4.1	31	8	15	46	*
Employed by others	341	4.6	4.1	.88	3.8	18	16	34	53	11
K. Frequency of discussion of birth control:										
Never	232	5.0	4.4	.88	4.2	6	4	11	18	30
Once in a while	123	5.0	4.5	.90	4.1	32	20	34	68	12
Sometimes	160	5.3	4.6	.87	4.1	16	17	38	56	11
Often	104	5.5	5.0	.91	4.0	20	32	40	65	15
L. Wife's exposure to birth control information:										
None	185	5.2	4.7	.89	4.2	17	9	20	38	12
One source	97	4.8	4.3	.90	4.0	16	14	34	52	7
Two sources	31	4.5	4.0	0.90	3.6	6	39	68	74	0

§ Excludes eight self-employed professionals and six in partnerships.

related to the low-fertility complex than is household income (Table 6, Panel D). Apparently the amount of cash income is less important than how it is spent in placing the family in the modernizing sector. A sterilizing operation may be akin to a modern object of consumption. The proportion sterilized increases monotonically with the number of objects owned and reaches 25 per cent for wives 35–39 years old owning seven or more objects of this kind. As there is no such close relationship with income, it would appear that getting a sterilizing operation depends not only on whether the money is available but on whether there is a modern orientation to consumption. The relationship may be reciprocal. Sterilization may be one way to increase the probability

that money to buy modern objects of consumption will be available.

RURAL BACKGROUND

Like most large cities in the developing countries, Taichung is populated largely by migrants from farms and villages. Even among the young couples sampled in our study, 80 per cent reported that one or both spouses had lived or were currently living on a farm. Taichung includes in its official boundaries a large suburban farm area, and 18 per cent of the husbands in the study were farm owners or tenants (as a principal occupation).

Being a farmer in Taichung is associated with high fertility values and behavior—even though the suburban farmers of Taichung are closely linked to an urban market and institutions (Table 6, Panel E). Further, if both husband and wife have ever lived on a farm, fertility values are also high (Table 6, Panel F). Apparently the wife's farm background is more important than the husband's. Both migration status and farm-urban background affect fertility and family planning (Table 6, Panel G). Low fertility values are most characteristic of migrants to Taichung from other large cities. They are least characteristic of migrants to Taichung if the wife has spent most of her life both before and after marriage on a farm. More farm background or coming from smaller town is associated with higher fertility values within each of three migration classes: those living in Taichung all their lives, migrants to Taichung with no farm background, and migrants with a farm background.

TABLE 7

PROPORTION OF COUPLES WITH THREE OR MORE LIVING CHILDREN AND TWO OR MORE LIVING SONS WHO HAVE USED VARIOUS FORMS OF FAMILY LIMITATION, BY WIFE'S EDUCATION AND BY TYPE OF FAMILY

	No. of Couples	Percentage Using				Percentage of Those with Abortion Also Using Other Methods
		Sterilization	Abortion	Contraception	Any of Three	
Wife's education:						
None	435	8	12	17	28	37
Primary, not graduated	158	16	13	20	39	20
Primary, graduated	450	16	19	32	51	14
Junior level or senior, not graduated	161	19	26	51	71	5
Graduate, senior level or more	85	26	28	52	79	4
Type of family:						
Joint	92	9	7	15	26	*
Stem	339	13	14	23	38	15
Nuclear	857	15	20	33	51	15

* Base less than 10

The effect of rural background may be attributed largely to less education. Educational differences remain large when rural background is controlled, but rural background is of much lesser significance once education is controlled (results in unpublished tables).

FAMILY STRUCTURE

Living in a nuclear family rather than in a stem or joint family is linked to lower fertility values (Table 6, Panel H). Among couples with at least three living children and two sons, those in the nuclear units have made use of some method of family limitation twice as often as those in joint families (Table 7). These facts [illegible]

ticularly significant since changes in family structure undoubtedly are an important cause and consequence of any major fertility decline.

Nuclear families in the younger ages are not so distinctively modern in fertility values when compared to stem or joint families. Probably this is due to the fact that the nuclear units do not always represent the most modern sector of the population in these younger ages. Many young couples begin their married life with the parents of the husband, and then those who are most modern in their attitudes are likely to begin a separate household later when they can. There is in fact a rather sharp increase in the proportion living in a nuclear unit as the age of the wife increases. Levy, Lang, and others[11] have made a convincing case for the proposition that even long before the war on the mainland the joint family was probably a normative ideal rather than an actual living situation for most Chinese at any one time. The nuclear family was probably most common in the lower-status groups because high mortality made the necessary personnel unavailable for an extended family form and because the poorer groups did not have the resources to support a larger unit. A pilot study in 1961[12] found precisely this situation prevailing in Taichung. The nuclear family was most common in the lowest status and least modern strata, in part (but not entirely) because mortality was highest in these groups. It is, therefore, all the more striking that we still find nuclear families generally to have the most modern fertility values and behaviors. It appears likely that further research will show that living in a nuclear unit may result *either* from traditional structural features of low economic status or in quite a different way from the modernizing aspects of high status. Thus, living in a nuclear family is most likely to represent a desire to live in this way among high-status families and a necessity among those with lower status.

TRADITIONAL FAMILY ATTITUDES

Holding the most traditional Chinese attitudes about certain other aspects of family life is associated with the less modern fertility and family-planning values. Couples are classified in Table 6 (Panel I) in terms of the wife's answer to questions about whether or not she expected to live with her sons when they were grown up or to be supported by them if she did not live with them. A large majority of the wives answered "definitely yes" to both questions. This traditional majority was characterized by the highest desired and actual fertility and the lowest survival rate, by the least use of each type of family limitation, but by the greatest reliance on abortion alone when it was used. Lesser degrees of traditionalism are not clearly differentiated with respect to actual or desired fertility, but the use of family limitation does increase as traditionalism decreases. However, the barrier of traditional attitudes to the use of family limitation should not be exaggerated. After all, even in the group expressing the most traditional attitudes, 37 per cent had tried some form of family limitation by age 35–39.

EMPLOYMENT STATUS

The employment situation of the husband is another basis for classifying the family's degree of dependence on the more modern sector of the economy. Those who work for their relatives or in a family business exemplify the traditional work situation where kinship and economic systems are interwoven. The self-employed not in professional and technical occupations are also usually in the more traditional sector. They are mainly small shopkeepers or operate small businesses in which they rely on the help of family mem-

[11] Olga Lang, *Chinese Family and Society* (New Haven, Conn.: Yale University Press, 1950); Marion Levy, *The Family Revolution in Modern China* (Cambridge, Mass.: Harvard University Press, 1949).

[12] Unpublished seminar paper by Solomon Chu, University of Michigan, Spring, 1963.

bers; in many cases the family lives on the business premises. Those employed by non-relatives are more likely to be working in the modern type of impersonal work relationship.

The data (Table 6, Panel J) confirm expectations that the more modern employment situation is related to the low-fertility complex and the traditional work situation to higher fertility values.

COMMUNICATION ABOUT FAMILY LIMITATION

Those who adopt low fertility values are likely to be in a communication network in which the lower fertility values are supported either directly or indirectly. Higher education and mass media are likely to have such indirect effects. But respondents were also asked about specific direct communications about birth control from various sources (newspapers, radio, meetings of organizations). As Table 6 (Panel K) indicates, those who reported one or two such sources of information were much more likely to have low fertility values than those reporting no source of information. Again, there is probably a reciprocal relationship. Those who have multiple sources of information may be led to use family planning; those who have used family planning are likely to seek out additional sources of information.

SUMMARY

Taiwan is a society in that stage of the demographic transition in which mortality has fallen to low levels and remained there long enough for higher survival rates to exert pressure on traditional arrangements based on high mortality There are signs of a variety of developmental changes which may lead to lower fertility, and fertility has begun to fall in the older ages, as might be expected if many women have the children they want before the end of the child-bearing period and begin to try to limit family size. A sample survey in the city of Taichung provides evidence consistent with the hypothesis that favorable attitudes to family limitation and attempts to practice it develop in the latter half of the child-bearing period when low mortality permits the survival of the moderate number of children and sons formerly requiring a larger number of births. We do not know whether this represents a change in the desired number of children or simply a different way of reaching the same values.

The survey data show clearly that those in the more modern sectors of the population, with characteristics transcending the traditional local familial and community setting, use family-limitation methods first and most effectively; they want and have smaller families, more of their children survive, and they turn more often to methods other than abortion.

Apparently family limitation becomes so crucial to many couples that there is a fumbling experimentation with a variety of methods, including contraception abortion, and sterilization—even if these are not considered good things to do by the population itself.

TAIWAN AND UNIVERSITY OF MICHIGAN
POPULATION STUDIES CENTERS

Informal Group Participation and Residential Patterns[1]

Aida K. Tomeh

ABSTRACT

The present study deals with informal group participation as related to settlement patterns in a metropolitan community. This analysis describes a precision-matched sample developed from a total sample of 2,401 cases. Basic conclusions are that place of residence tends to differentiate the population relative to informal group participation, and that differences in informal contact are a function of both population characteristics and residence location rather than of the former variable alone, as previous studies seem to suggest. The pattern for such interaction is generally low in the city and high in the suburbs. Exceptions to this generalization are located in those groups that do not fit the general pattern shown by the population of the zone in which they are living; a theoretical implication is that being in a minority affects one's interaction rate.

INTRODUCTION

The growth of the urban community has raised numerous issues among students of social organization. Many of these issues follow from the nature of the new pattern of land use and population redistribution which has resulted from the outward movement of the urban population. One of the important effects of this flow to the suburbs concerns the participation of residents in community activities. The present study deals with one dimension of such participation, namely, the nature and extent of informal social interaction as it is related to residential patterns in metropolitan Detroit.

With the exception of a very few studies,[2] investigations of formal and informal group participation in an urban setting have been limited to the analysis of the total population of a given community without reference to subareas.[3] Important as such studies are, they neglect suburbanization and its effect on informal participation. Such an analysis will enable us to point out the extent to which informal social relationships are influenced by residence location. At the same time, this research should bear on the assumption often found in the work of some early ecologists[4] that spatial distributions are expressions of the social structure (i.e., space constitutes a dimension on which such relationships can be measured).

Four groups have been chosen in this research to represent foci of informal par-

[1] Revision of a paper read at the annual meeting of the Michigan Sociological Association, November, 1961. Data are taken from the author's doctoral dissertation, Department of Sociology, University of Michigan, 1961. The writer is indebted to Professors Harry Sharp, David Goldberg, and Werner Landecker for a number of suggestions and valuable criticisms in connection with this paper.

[2] Amos Hawley and Basil Zimmer, "The Significance of Membership in Associations," *American Journal of Sociology*, LXV (September, 1959), 196–201; Sylvia Fava, "Contrasts in Neighboring: New York City and a Suburban Community," in William Dobriner (ed.), *The Suburban Community* (New York: G. P. Putnam's Sons, 1958), pp. 122–31; Walter Martin, *The Rural Urban Fringe: A Study of Adjustment to Residence Location* (Eugene: University of Oregon, 1953).

[3] Morris Axelrod, "Urban Structure and Social Participation," *American Sociological Review*, XXI (February, 1956), 13–18; Charles R. Wright and Herbert H. Hyman, "Voluntary Association Memberships of American Adults: Evidence from Natural Sample Surveys," *American Sociological Review*, XXIII (June, 1958), 284–94; Harry Sharp, "Migration and Social Participation in the Detroit Area" (unpublished Ph.D. dissertation, University of Michigan, 1954); Basil Zimmer, "Participation of Migrants in Urban Structures," *American Sociological Review*, XX (April, 1955), 218–24.

[4] For an early discussion of this point see Robert Park, "The Urban Community as a Spatial Pattern and a Moral Order," in Ernest Burgess (ed.), *The Urban Community* (Chicago: University of Chicago Press, 1925), pp. 1–18.

ticipation: (1) neighbors, (2) relatives (related persons other than those who may be living in the same household as the respondent), (3) co-workers, and (4) other friends (a residual category consisting of acquaintances other than neighbors, relatives, or co-workers). The extent of participation in such groups, moreover, is often assumed to be an important index of social integration.[5]

Residential distribution is defined in terms of three settlement patterns: (1) the inner city, located within six miles of the center of the business district of the city of Detroit; (2) the outer city zone, which is more than six miles from the center but inside the Detroit city limits; and (3) the suburban area which is the 1950 tracted part of greater Detroit lying outside the city limits.

If differences exist in the rates of informal social participation among different zones of an urbanized area, they may be suggestive of some probable effects of continued population redistribution in the community upon the social organization of the areas.

[5] Theoretically, the concern with social integration is not new in social science. Many sociologists have concerned themselves with the processes of social action that contribute to the maintenance and persistence of the social system. See Émile Durkheim, *Division of Labor* (Glencoe, Ill: Free Press, 1947); Frederick Tonnies, *Fundamental Concepts of Sociology: Gemeinschaft and Gesellschaft*, trans. Charles Loomis (New York: American Book Co., 1940); Charles Cooley, *Social Organization* (New York: Charles Scribner & Sons, 1909); Amos Hawley, *Human Ecology: A Study of Community Structure* (New York: Ronald Press, 1950). Empirically, the function of the primary group has been investigated in urban structures, military units, industrial corporations, etc. See Harry Sharp and Morris Axelrod, "Mutual Aid among Relatives in an Urban Population," in Ronald Freedman *et al.*, *Principles of Sociology* (New York: Henry Holt & Co., 1956), pp. 433-39; Edward Shils and Morris Janowitz, "Cohesion and Disintegration in the Wehrmacht in World War II," *Public Opinion Quarterly*, XII (Summer, 1948), 280-315; Elton Mayo, *The Social Problem of an Industrial Civilization* (Boston: Graduate School of Business Administration, Harvard University, 1945).

The effect of urbanization on informal association is of special interest. Thus, if informal participation is indicative of social integration, and if suburbanites have a higher rate of participation than residents of the inner city, then a relative increase of suburban population would suggest an increase in social integration.

Our purpose in this study is to distinguish *population effect* from *area effect*; that is, most studies contrasting degrees of participation in the suburb and the city have not adjusted for differences in population characteristics which contribute to differences in rates of informal participation. Ideally, if after accounting for the effect of population we still find differences, then these must be associated with residential location itself.

The basic hypothesis of this study is, indeed, that informal participation is more characteristic of the suburb than of the city and that this situation does not reflect selective migration but rather the shift from one type of residential area to another.

Several authors have argued that characteristics peculiar to the suburbs lead to a high level of informal participation. Martin,[6] for example, claims that homogeneity of the population is one of the more important outcomes of the selective migration to and from the suburbs which has important implications for the pattern of informal social relationships. According to Martin, suburban homogeneity has influence on the pattern of neighborhood associations, although he does not distinguish between types of informal groups. Kish[7] has demonstrated for a number of variables that communities immediately adjacent to large metropolitan cities are highly differentiated among themselves but

[6] Walter Martin, "The Structuring of Social Relationships Engendered by Suburban Residence," *American Sociological Review*, XXI (August, 1956), 446-53.

[7] Leslie Kish, "Differentiation in Metropolitan Areas," *American Sociological Review*, XIX (August, 1954), 388-98.

highly homogeneous internally. This internal homogeneity in some suburbs is heightened where there is careful screening of applicants according to cultural, economic, and other background factors. In such highly selective residential districts, the homogeneity of the area tends to maximize interaction on a neighborhood basis.[8] In comparison, the common belief, undocumented by research, is that the lack of common interests which characterizes inner zones tends to limit interaction among residents on the primary level, a characteristic which is likely to be missing in the suburbs.

Others have argued that the population characteristics of suburbs result in high informal participation rates. Suburbs have been described as representing a middle-class community. Consequently, middle-class patterns of interaction can be expected to predominate. These are said to be conducive to participation in community activities.[9]

Impressionistic views lead us to believe that suburban areas have a greater potential for enhancing informal interaction among the residents by the very nature of their location. For example, inner-city zones tend to be quite commercial and industrial. This pattern of land use, moreover, has appeared in numerous residential districts that lie within the limits of the inner city. In comparison, suburban areas lack such conditions which tend to hinder informal participation.

[8] Theodore Caplow and Robert Forman, "Neighborhood Interaction in a Homogeneous Community," *American Sociological Review*, XV (June, 1950), 357–66; Leon Festinger *et al.*, *Social Pressures in Informal Groups* (New York: Harper & Bros., 1950); William H. Whyte, Jr., "The Transients: How the New Suburbia Socializes," *Fortune*, XLVIII (August, 1953), 120 ff.

[9] Leonard Reissman, "Class, Leisure and Social Participation," *American Sociological Review*, XIX (February, 1954), 76–84; Bennett Berger, *Working-Class Suburb: A Study of Auto-Workers in Suburbia* (Berkeley: University of California Press, 1960), chap. i.

DATA SOURCES

The area covered in this survey includes the cities of Detroit, Hamtramck, and Highland Park, as well as the congested urbanized places adjacent to the central city.[10] The data were collected by the Detroit Area Study of the University of Michigan.[11] Information from three surveys is utilized in the present study. The first survey was carried out in 1951–52, the second in 1956–57, and the third in 1958–59. Since identical information for the variables under study was obtained, the data from these surveys were combined to yield a larger N.[12]

The sample for all three surveys represents a cross-section of all adults in the Detroit area living in private households. The samples for all years were selected on a probability basis;[13] that is, every adult in a private dwelling in the research area had a known chance of falling into the sample.

Interviewing was done by graduate students at the University of Michigan and professional interviewers employed by the Survey Research Center. For all of the three samples, interviews were successfully completed at approximately 87–89 per cent

[10] The sample covered approximately 87 per cent of the population of Wayne, Macomb, and Oakland counties. These counties comprise the Standard Metropolitan Statistical Area as defined by the U.S. Bureau of the Census.

[11] For a description of the Detroit Area Study see Ronald Freedman, "The Detroit Area Study: A Training and Research Laboratory in the Community," *American Journal of Sociology*, XXIX (July, 1953), 30–33.

[12] A point may be raised as to the possible yearly variation in the foci of the three studies. We should, therefore, emphasize the fact that data reported here for the combined sample are quite similar to the relationships which appear when each year is analyzed separately. The relevant tables for this analysis are available on request.

[13] The sampling section of the Survey Research Center at the University of Michigan provided materials and directed work on the sample design. A detailed description of the sample design of each year may be obtained from the Survey Research Center, University of Michigan.

of the addresses selected. No substitution of addresses or of respondents was permitted.

FINDINGS

The findings reported in this paper pertain to the analysis of a precision-matched sample which is only one phase of the larger study. Matched samples for the inner, outer, and suburban areas were developed from the total sample of 2,401 cases. Since residents of the three residential areas are matched on a variety of characteristics, the number of cases is the same in each of the area samples.

Respondents were matched on seven characteristics: age, marital status, sex, race, education, religious affiliation, and migrant status. Respondents were not matched on income and occupation. It was felt that education would be a sufficient indicator of socioeconomic status. The three measures of socioeconomic status are themselves correlated, of course, and prior research has indicated that education is a better predictor of informal participation than the other two. The choice of education was deliberately made in order to yield the most effective test of the substantive hypothesis. Each of the seven variables was dichotomized or trichotomoized in the matching procedure. Extra matches[14] in a given zone were eliminated with the use of a table of random numbers so that the means and frequency distributions of all seven variables were identical in each zone.

When population characteristics are held constant in this manner, residential differences in informal participation with relatives are very slight (Table 1). Outer-city and suburban residents exceed the inner-city population in their contacts with kin by only four percentage points.

Contacts with co-workers also show small differences by place of residence. Fifty-nine per cent of the residents in the suburbs visit occasionally with their co-workers, as compared to 54 per cent in the inner city. Thus, as far as co-worker associations are concerned, residing in the suburbs or elsewhere in the city is of little consequence for the matched groups considered here.

TABLE 1

FREQUENCY OF CONTACT, BY TYPE OF INFORMAL GROUP AND PLACE OF RESIDENCE

(Matched Sample)

	Place of Residence (Per Cent)			
	Inner	Outer	Suburb	Total
Contact with relatives:				
Often*	57	61	61	60
Sometimes	29	28	30	29
Never	10	10	8	9
Not ascertained	4	1	1	2
N	(267)	(267)	(267)	(801)
Contact with neighbors:				
Often	39	32	40	37
Sometimes	21	30	34	28
Never	36†	36‡	26	33
Not ascertained	4	2		2
N	(267)	(267)	(267)	(801)
Contact with co-workers:				
Often	19	19	20	19
Sometimes	35	37	39	37
Never	42	43	38	41
Not ascertained	4	1	3	3
N	(267)	(267)	(267)	(801)
Contact with friends:				
Often	39†	44	47	43
Sometimes	42	46	39	42
Never	15	9	13	13
Not ascertained	4	1	1	2
N	(267)	(267)	(267)	(801)

* "Often" refers to a few times a month or more frequently; "sometimes" refers to once a month or less often.

† Percentage difference between inner city and suburb significant at the 05 level.

‡ Percentage difference between outer city and suburb significant at the 05 level.

With respect to neighborhood participation, however, a larger difference by residence is found. For example, while 36 per cent of the inner-city inhabitants *never* contact their neighbors, only 26 per cent of the suburbanites have no such contacts.

[14] An "extra match" applies to situations where there are more cases of a given combination of characteristics in one zone than in another. For example, if there are seven cases in one zone and ten cases in another zone of a given matched category, three of the latter cases are eliminated.

There is evidence here to show that suburban living is associated with increased contacts on the neighborhood level.

The analysis of friendship contacts shows a similar pattern, although the differences in participation are non-linear when compared to the differences in participation with neighbors. Table 1 reveals a gradual increase in contacts with friends as distance from the inner city increases. Thirty-nine per cent of the inner-city population visit with friends "quite often," while 44 per cent of the outer city and 47 per cent of the suburban residents have this pattern.

TABLE 2

TOTAL INFORMAL PARTICIPATION AND PLACE OF RESIDENCE

(Matched Sample)

Informal Participation	Place of Residence (Per Cent)			
	Inner	Outer	Suburb	Total
High	36*	35†	47	40
Low	55	59	48	54
Not ascertained	9	6	5	6
N	(267)	(267)	(267)	(801)

* Percentage difference between inner city and suburb significant at the .05 level

† Percentage difference between outer city and suburb significant at the .05 level.

In general, the above results show that suburbs are conducive to increased participation with neighbors and friends, although the amount of kinship and co-worker associations does not seem significantly larger.[15]

Table 2 relates a summary measure of informal participation to place of residence. This measure was obtained by summing the responses to each of the questions on relatives, neighbors, co-workers, and friends, each question being coded from "1" to "5" according to frequency of contacts. The final distribution of summary scores was then divided into two categories at a cutting point where the number of cases in each of the two categories was approximately equal.

When total informal participation is related to place of residence, a tendency is again noted for such participation to be restricted in the city's center (Table 2). Only 36 per cent of the inner-city population have a high particpation rate as compared with nearly one-half of the suburbanites.[16] No differences appear in the rate of participation between inner-city and outer-city residents.

The presence of larger differences in total informal participation by place of residence is perhaps more noteworthy than are the slight zonal differences in participation when each type of informal group is considered separately. There is evidence here for the proposition that the process of suburbanization tends to be accompanied by increased informal participation in a general sense and across the specific types of informal groups.

If the respective influence of each control variable is examined in its own right, the following consequences for informal participation are observed: younger persons participate more than older people, married persons more than single persons, well-educated persons more than those with less education, and native Detroiters more than migrants. On the other hand, differences in sex, race, and religion have only a slight effect on informal participation (Table 3).

There is, however, a possibility that a given control factor exerts a different influence on participation in one local area than in another. In order to examine this possibility, the three areas of residence were compared within subdivisions of each of the control variables.

[15] For a description of the measures of sampling error upon which statistical significance is established in this research see *A Social Profile of Detroit: 1956* (Ann Arbor: Detroit Area Study, University of Michigan, 1957), Appendix B.

[16] The procedure followed to obtain an index of over-all participation does not utilize the optimum cutting points revealed in Table 1. This fact should be pointed out as a kind of counter to the non-linear results shown in Table 1.

TABLE 3

Total Informal Participation and Place of Residence by Demographic Characteristics

(Matched Sample)

Demographic Characteristics and Informal Participation	Place of Residence (Per Cent)			
	Inner	Outer	Suburb	Total
Age				
21–34:				
High	52	45	63	54
Low	38	51	32	40
Not ascertained	10	4	5	6
N	(84)	(84)	(84)	(252)
35–49				
High	32	43	44	40
Low	64	49	55	56
Not ascertained	4	8	1	4
N	(75)	(75)	(75)	(225)
50 or older:				
High	27	22*	37	29
Low	61	71	56	63
Not ascertained	12	7	7	8
N	(108)	(108)	(108)	(324)
Marital Status				
Not married:				
High	37	26	28	30
Low	53	63	56	57
Not ascertained	10	11	16	13
N	(43)	(43)	(43)	(129)
Married, no children:				
High	27†	26*	44	33
Low	59	69	54	60
Not ascertained	14	5	2	7
N	(84)	(84)	(84)	(252)
Married, children				
High	41†	44*	55	47
Low	53	52	42	49
Not ascertained	6	4	3	4
N	(140)	(140)	(140)	(420)
Sex				
Males				
High	37	35*	48	40
Low	56	60	48	54
Not ascertained	7	5	4	6
N	(126)	(126)	(126)	(378)
Females				
High	36	35	47	39
Low	53	62	48	53
Not ascertained	11	3	5	8
N	(141)	(141)	(141)	(423)

* Percentage difference between outer city and suburb significant at the .05 level.

† Percentage difference between inner city and suburb significant at the .05 level.

TABLE 3—*Continued*

Demographic Characteristics and Informal Participation	Place of Residence (Per Cent)			
	Inner	Outer	Suburb	Total
Race				
Whites				
High	35†	35*	48	39
Low	56	59	47	54
Not ascertained	9	6	5	7
N	(246)	(246)	(246)	(738)
Negroes:				
High	52	38	38	43
Low	43	53	57	51
Not ascertained	5	9	5	6
N	(21)	(21)	(21)	(63)
Education				
Less than high school.				
High	29	31	38	33
Low	62	61	56	59
Not ascertained	9	8	6	8
N	(172)	(172)	(172)	(516)
High school and over				
High	49†	42*	63	52
Low	42	56	36	44
Not ascertained	9	2	1	4
N	(95)	(95)	(95)	(285)
Religious Affiliation				
Catholics				
High	29†	33*	46	36
Low	63	62	49	58
Not ascertained	8	5	5	6
N	(110)	(110)	(110)	(330)
Non-Catholics				
High	41	37	48	42
Low	49	57	47	51
Not ascertained	10	6	5	7
N	(157)	(157)	(157)	(471)
Migrant Status				
Native Detroiters				
High	43	44	56	48
Low	47	52	41	47
Not ascertained	10	4	3	5
N	(68)	(68)	(68)	(204)
Migrants				
High	34†	32*	44	37
Low	57	61	50	56
Not ascertained	9	7	6	7
N	(199)	(190)	(199)	(597)

By and large, increased participation in the suburbs was found again except in the following two instances: single people and Negroes have higher participation rates in the inner zone than in the suburbs.

These exceptions pertain to categories of persons that are severely underrepresented in the suburbs and thus involve personal characteristics that diverge from those which are prevalent. If persons tend to interact informally with others who are like them, then the minority status of the unmarried person (12 per cent of the total adult suburban population)[17] and of the Negro (5 per cent) constitutes a barrier to their participation. On the other hand, unmarried persons (32 per cent) and Negroes (39 per cent) in the inner city constitute a larger percentage of the population than in the suburbs and therefore have relatively frequent opportunities for interaction with other persons like themselves.

INTERPRETATION

In general, the findings seem to suggest, first, that although participation differences between the inner and outer zones are quite small and inconsistent, participation increases as one moves across Detroit's city limits, and second, that participation is facilitated when relatively many persons of similar characteristics live in the same area.

The matching procedure provides some evidence that place of residence is one of several variables *independently* related to the degree of *total informal participation* because differences in participation by zone persist even when seven population characteristics are controlled.

When a comparison is made between the effect of the independent variable and that of the control variables, one is inclined to conclude that zone of residence is less important than the control variables. This fact is particularly clear in the case of age, marital status, education, and migrant status. These variables are highly related to informal group participation, whereas the relationship between residence zone and informal contacts is relatively small, though consistent.

The data show that suburban residence is often associated with an increase in informal participation. This immediately poses the problem of whether the rich informal sociability of the suburb is fostered by the residence location itself or by the population composition of the area. Evidence presented here points out the importance of both factors. Although population type is a primary factor in structuring the degree of informal participation,[18] zone of residence has a small but independent effect on such activity. The zonal effects on participation in this study are much smaller than those reported in previous research on suburban residence. This would suggest the possibility that a large part of the previously reported differences is attributable to population differences rather than to zonal differences.

It is to be emphasized that the unique features of zones which have a bearing on informal interaction remain to be identified. We do not know what bearing homogeneity, land use, size, physical outlay, etc., have on participation. This study has not concerned itself with the "way of life" which is typical of certain areas. There are, of course, studies that have shown that the

[17] For a distribution of the population composition by place of residence of the original sample see Aida Tomeh, "Informal Group Participation and Settlement Patterns in Metropolitan Detroit" (unpublished Ph.D. dissertation, University of Michigan, 1961), Appendix A.

[18] The problem of population effect on informal participation using the same measures of participation as those in this study has been discussed by a number of sociologists, e.g., Sharp, *op. cit.*; Joel Smith *et al.*, "Local Intimacy in a Middle-sized City," *American Journal of Sociology*, LX (November, 1954), 276–83; Scott Greer, "Urbanism Reconsidered: A Comparative Study of Local Areas in a Metropolis," *American Sociological Review*, XXI (February, 1956), 19–25; Axelrod, *op. cit.*; Wendell Bell and Marion Boat, "Urban Neighborhoods and Informal Social Relations," *American Journal of Sociology*, LXII (January, 1957), 391–98; Eugene Litwak, "Occupational Mobility and Extended Family Cohesion," *American Sociological Review*, XXV (February, 1960), 9–2[illegible]

life style of the suburbs and the spatial relationships among the residential sites permit a high degree of intercommunication among the members. These studies have been conducted on extremely selective suburbs or suburban housing developments.[19]

[19] These reports are based for the most part upon life in Levittown, New York; Park Forest, Illinois; Lakewood, near Los Angeles; and a fashionable suburb of Toronto, Canada. See Harold Wattel, "Levittown: A Suburban Community," in William Dobriner (ed.), *The Suburban Community* (New York: G. P. Putnam's Sons, 1958), p. 299; William H. Whyte, Jr., *The Organization Man* (New York: Doubleday Anchor Books, 1956), pp. 295-434; "The New America: Living atop a Civic Mushroom," *Newsweek*, April 1, 1957, pp 36 ff.; John R. Seeley *et al.*, *Crestwood Heights: A Study of the Culture of Suburban Life* (New York: Basic Books, 1956).

It is worth noting that all exceptions to the tendency of increased suburban participation involve residents who do not fit into the general pattern of the area. This finding calls attention to the importance of taking the structural context into account in sociological analysis.[20] Thus observed differences in behavior can be explained when behavior is analyzed not simply in terms of individual characteristics but also in terms of the structural context in which it occurs.

BOWLING GREEN STATE UNIVERSITY

[20] A similar point is made by Zena Smith Blau, "Structural Constraints on Friendships in Old Age," *American Sociological Review*, XXVI (June, 1961), 429-39.

Social Class and College Graduation: Some Misconceptions Corrected[1]

Bruce K. Eckland

ABSTRACT

From past research on the relationship of class origins and education, social scientists generally agree that, although class membership is a strong determinant of who goes to college, it does not determine who graduates. This study presents evidence to suggest that social class and college graduation are in fact, significantly related, especially among the college entrants who were only average students in high school. Rather than economic or intellective factors, the link between the independent and dependent variables appears to be the "psychocultural dimension" of class. Two major explanations are offered for the equivocal findings of earlier research: inattention to the diverse social composition of student bodies at different institutions and neglect of the prolonged academic careers of the college dropouts who came back.

As occupational achievement becomes increasingly dependent upon level of educational attainment,[2] the general question of "who gets educated" becomes more salient to social mobility and the allocation of human resources. Furthermore, the upsurge of college enrolments both in absolute and relative numbers lends special significance to the question of "who graduates from college." This is particularly true when most studies on college attrition, although not all, find that a very large number of the entering Freshmen fail to attain degrees.[3]

It is a recognized fact that all students do not have equal access to institutions of higher education, since one's location in the class structure is an important determinant of achievement in high school and of whether he goes to college.[4] The passage from school to college, in fact, seems to depend more upon socialization, life experience, and opportunity than upon intellective factors. When the student reaches college, however, non-intellective factors presumably are replaced by intellective ones. High-school grades and measures of scholastic aptitude are generally recognized as the most reliable predictors of academic achievement and graduation from college.[5]

[1] I am indebted to James H. Barnett, Bernard Farber, Joseph Gusfield, Dorothy M. Knoell, and Elmer D. West for their criticisms of an earlier version of this paper. Data collection was supported by the Office of Instructional Research, University of Illinois.

[2] Otis Dudley Duncan and Robert W. Hodge, "Education and Occupational Mobility," *American Journal of Sociology*, LXVIII (May, 1963), 629–44.

[3] A major study using a national sample was conducted by Robert E. Iffert (*Retention and Withdrawal of College Students* [Washington, D.C.: Government Printing Office, 1957]). Two recent reviews of the literature on attrition rates are presented by Dorothy M. Knoell, "Institutional Research on Retention and Withdrawal," in *Research on College Students* (The Western Interstate Commission for Higher Education, Boulder, Colorado, and the Center for Higher Education, Berkeley, California, December, 1960), pp. 41–65, and by John Summerskill, "Dropouts from College," in Nevitt Sanford (ed.), *The American College* (New York: John Wiley & Sons, 1962), pp. 627–57.

[4] Ralph F. Berdie, *After High School—What?* (Minneapolis: University of Minnesota Press, 1954); Dael Wolfle, *America's Resources of Specialized Talent* (New York: Harper & Bros., 1954), pp. 158–69; Bryon S. Hollinshead, Robert J. Havighurst, and Robert R. Rodgers, *Who Should Go to College* (New York: Columbia University Press, 1952); W. Lloyd Warner, Robert J. Havighurst, and Martin Loeb, *Who Shall Be Educated?* (New York: Harper & Bros., 1944), pp. 66–69; and R. Clyde White, *These Will Go to College* (Cleveland: Press of Western Reserve University, 1952), pp. 34–59.

[5] Summerskill, *op. cit.*, pp. 634–35; Joshua A. Fishman, "Social-Psychological Theory for Selecting and Guiding College Students," *American Journal of Sociology*, LXVI (March, 1961), 472–84; and Benjamin S. Bloom and Frank R. Peters, *The Use of Academic Prediction Scales for Coun-*

Only a handful of studies have shown a moderate degree of correlation between social class and college performance, while many more have found no relationship. The obvious inference is that the general relations of these variables are so indefinite that most observers have agreed with Wolfle's conclusion that "the influence of socioeconomic differences disappears almost entirely." After a student gets to college, he "has already overcome most of whatever handicaps his home environment offered; once there, his chances of graduating are much more dependent upon his ability and much less upon his family background than were his chances of getting into college in the first place."[6]

Adding to this view we find frequent statements, such as Havinghurst's, about the "expansion of educational opportunity for able students from working-class and lower-middle-class homes."[7] A rising standard of living, the extension of scholarship and loan programs, and the growth of the junior college and the inexpensive state college or university all would appear to enhance "equality of opportunity" and make college success more dependent upon the coupling of talent and a little bit of effort. It has become what Turner calls a "contest" which "is judged to be fair only if all the players compete on an equal footing."[8]

Despite these claims, there is reason to believe that significant and stable relationships between social class and graduation from college might be demonstrated if two unique features of the educational system were taken into account: (1) the existing *diversity* among colleges and universities in the social composition of their student populations; and (2) the prolonged and sporadic careers of many students who *persist* in college and eventually graduate.

The first feature, which shall be referred to as the "diversity hypothesis," indicates that *the rate of dropout at a college or university varies inversely with the class composition of its student population.* The educational system in the United States displays many differences in the intellective and non-intellective composition of the college-going population and the institutions they attend.[9] The resulting diversity may have obscured the impact of social class upon college performance, for this diversity is not randomly arranged.

To a large extent, *where* a student goes to college is determined by his location in the class structure. Two decades ago Warner and his associates observed the variety of institutions that corresponded roughly to the social differences in college students.[10] More recent observations by Riesman and Jencks,[11] Clark,[12] and White[13] do not depart appreciably from Warner's.

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seling and Selecting College Entrants (New York: Free Press of Glencoe, 1961).

[6] Wolfle, *op. cit.*, pp. 160-63. See also Paul Heist, "The Entering College Student—Background and Characteristics," *Review of Educational Research*, XXX (October, 1960), 291; Seymour Martin Lipset and Reinhard Bendix, *Social Mobility in Industrial Society* (Berkeley: University of California Press, 1959), p. 233; and Summerskill, *op. cit.*, p. 632.

[7] Robert J. Havighurst, "The Impact of Population Change and Working Force on American Education," *Educational Record*, XLI (October, 1960), 348-49. Cf. Burton R. Clark, "The Coming Shape of Higher Education in the United States," *International Journal of Comparative Sociology*, II (September, 1961), 203-11.

[8] However, Turner suggests that in our educational system victory is won by the one who exerts the most effort (i.e., the "most deserving") and not necessarily by the most able (Ralph H. Turner, "Sponsored and Contest Mobility and the School System," *American Sociological Review*, XXV [December, 1960], 857).

[9] T. R. McConnell and Paul Heist, "The Diverse College Student Population," in Sanford (ed.), *op. cit.*, pp. 225-52; and Clark, *op. cit.*

[10] Warner *et al.*, *op. cit.*, pp. 71-72.

[11] David Riesman and Christopher Jencks, "The Viability of the American College," in Sanford (ed.), *op. cit.*, pp. 74-192.

[12] Burton R. Clark, *The Open Door College* (New York: McGraw-Hill Book Co., 1960), p. 54.

[13] *Op. cit.*, p. 48.

The social composition of the student body at many institutions tends to be relatively uniform, particularly at the schools with very low and very high prestige. It is not surprising, then, that most of the separate institutional studies, of which the research on retention and attrition has largely been composed, have found low correlations between social class and performance in college. The studies have not obtained representative samples of the class distribution of the American college-going population. Just as the student population is diverse, so too are the colleges and universities which sift through the diversity to produce varying degrees of social likeness at each institution.[14]

A second explanation for the equivocal findings reported in earlier studies and the one which will be evaluated in this report, involves a time element and a motivational element. Together, they are the principal sources of the "persistence hypothesis": *Graduation from college varies directly with the social-class origin of the college entrant; however, this relationship will be less apparent if we do not allow for the late graduation of the persistent student and if we do not select an index of social class that is associated with a motivational element.*

THE TIME ELEMENT

When is a student's performance in college to be judged? A year after entering, four years after, or still later? In terms of occupational achievement, *graduation* from college probably is far more significant than academic standing in one's Freshman class or than attaining "some college" education. While the poor student who barely makes it through college may not find employers eagerly making him offers, he certainly has more immediate access to the better job openings than the bright student who did well in college but did not graduate. No highly structured apparatus for job placement, such as that which picks through each college graduating class, is available to the college dropout. (In fact, the kind of work that a college dropout is expected to obtain appears ill-defined. Some employers hesitate to hire him at all for fear that he is "overeducated" for most jobs and "undereducated" for many others.)

However, about half the studies that have dealt with the predictive qualities of non-intellective variables, such as social class, have taken withdrawal from college or grade-point averages during the *first year* as the dependent variable. Most others use a *four-year* period, which usually means graduation from the institution of first registration. The general impression that social class has no significant bearing upon a student's performance in college is confirmed by the cumulative results of such studies.

Yet, a low proportion of college entrants actually attain a degree in four years from the institution of first registration. Iffert's nationwide survey reported a rate of less than 40 per cent at all degree-granting institutions.[15] Many of the non-graduates in his figures, however, transferred without an interruption, many were still enrolled at the original institution and about to graduate, and many others were dropouts who would later return to college and graduate. Seldom have attrition studies taken any of these groups into account. Consequently, even the four-year institutional studies that have compared gradu-

[14] Data are not available to measure directly the hypothesized association between class origins and dropout rates among a cross-section of American college students. However, if we accept the assumption that class origins vary by institutions, with private colleges enrolling students of upper- and upper-middle-class origins and public colleges enrolling students of middle- and working-class origins, then the findings of two national surveys provisionally support the diversity hypothesis. Iffert (*op. cit.*, p. 90), who sampled 149 colleges and universities in 1954, found considerably higher rates of graduation in four years from privately controlled institutions. Very similar results were obtained by the U.S. Department of Education in a survey of twenty-five universities in 1937 (see John H. McNeeley, "Students Leaving College before Graduation," *School Life*, XXIII [March, 1938], 258–59).

[15] Iffert, *op. cit.*, p. 16.

ates and withdrawals in relation to selected social characteristics cannot be relied upon to explain or predict *final* graduation rates.

The study to be reported here found that among the male dropouts of a large state university, over 70 per cent returned to college at some time during the following ten years and about 55 per cent of those who came back graduated. In addition, many of the graduates who never dropped out of college lacked sufficient credits to participate in the regular commencement exercises with their classmates. If graduation is the critical measure of college performance, then the delayed graduates and the dropouts who came back certainly cannot be disregarded since their prolonged academic careers may represent a degree of persistence at least equal to that of the four-year graduates which, in turn, may be significantly related to the stratification structure.

THE MOTIVATIONAL ELEMENT

The time element simply suggests the *possibility* of a previously obscured relationship between social class and the late academic careers of college students. It does not tell us *why* we should expect to find the relationship.

On what does persistence in college depend, especially the persistence of the late graduates and the dropouts who come back? It is not necessarily intelligence or grades. Although these are limiting factors, a student can continue for a rather long period even with failing marks by transferring from one college to another. Or, at most state institutions, he may wait a prescribed period after an academic dismissal and then return. Of 352 dropouts in one study who left college for "self-admitted" academic failure, nearly half went back somewhere and graduated.[16]

In spite of the fact that wealth is one dimension of social class, the financial factor apparently is not the significant link. It is not consistent with the "diversity hypothesis" which suggests that there is a college or university to fit almost anyone's budget. If family or public support is not available, one may enrol in a part-time college program, work between terms, take a part- or full-time job, or any combination of these. A recent NORC study, for instance, shows that over half of the 1961 college graduates in the United States had been employed full- or part-time during their senior year.[17]

Rather, the link between class origin and persistence in college seems to involve differences in motivation and value orientations which influence the student's capacity either to take advantage of his intelligence and opportunities or to compensate for the lack of them. This "psychocultural dimension" of stratification, as Rosen[18] calls it, is distinct from the economic and other objective consequences of class membership that affect one's "life-chances." Studies on educational and occupational mobility have pointed to, and some have empiricially demonstrated, the significance of individual personality variables like "educational aspiration"[19] or "achievement motivation."[20] In most of them, motivation is shown not only to affect real achievement, which may seem obvious, but also to be consistently related to social class.

[16] The study to be reported below.

[17] James A. Davis, *Great Expectations* (Chicago: National Opinion Research Center, 1965), I, 14.

[18] Bernard C. Rosen, "The Achievement Syndrome: A Psychocultural Dimension of Social Stratification," *American Sociological Review*, XXI (April, 1956), 203-11.

[19] Robert E. Herriott, "Some Social Determinants of Educational Aspiration," *Harvard Educational Review*, XXXIII (Spring, 1963), 157-77; Joseph A. Kahl, "Educational and Occupational Aspirations of 'Common Man' Boys," *Harvard Educational Review*, XXIII (Summer, 1953), 186-203; and Hollinshead *et al.*, *op. cit.*, pp. 135-65.

[20] Rosen, *op. cit.*; Harry J. Crockett, Jr., "The Achievement Motive and Differential Occupational Mobility in the United States," *American Sociological Review*, XXVII (April, 1962), 191-204; and Bernard C. Rosen, "Family Structure and Achievement Motivation," *American Sociological Review*, XXVI (August, 1961), 574-85.

It is *unlikely* that variations of motivation that may be attributed to social class have been so diluted by the pre-college selection process that they cease to exist *after* students have entered college. Furthermore, it does not seem reasonable that when students go to college they would leave behind all of their divergent attitudes and beliefs that are part of the value systems of different social-class groups. In some degree, these class-derived variations undoubtedly still exist at the college level and partly determine who graduates, for the unusual persistence of both the dropouts who come back and the "four-year-plus" graduates surely represents something more than just an affinity for life on a college campus. We are suggesting, then, that the motivational element is the primary contribution that social class makes to performance in college.

TWENTY-FOUR INSTITUTIONAL STUDIES

The academic performance of college students tends to vary most during the Freshman year. This is particularly the case among many state-supported colleges and universities which are required to maintain relatively unrestricted admissions policies but have neither the facilities to cope adequately with rising enrolments nor the desire to lower their academic standards and competitive position in the educational system. As an alternative or supplement to screening devices that would restrict admissions only to students, for example, who graduated in the upper quarter of their high-school class, the Freshman year is often a weeding-out period characterized by high dropout rates. Iffert's national survey indicates that nearly twice as many students leave college, voluntarily or not, in the first year as in any subsequent year.[21] Undoubtedly, the many investigators who have measured college performance during or at the end of the Freshman year have been justified in doing so because the *early* incidence of academic failure is so very high (and, too, because the research task usually is less complicated than following a Freshman class through four or more years of college).

We have implied that if these studies were separated from those measuring either grade averages or college attendance over a longer period we would discover two different patterns regarding the relation of social class and college performance. Without a complete analysis of methodological procedures and of the context of each, a comparison of earlier investigations should be viewed with caution. However, we wish here primarily to examine the general point that the *time* at which college performance is measured will make a difference in the results obtained.

Table 1 compares the findings of twenty-four studies using performance criteria of one year (sometimes only one college term) and two to four years. The studies that served as sources for the data presented here[22] include all known institu-

[21] Iffert, *op. cit.*

[22] Sources for data in Table 1: *Father's occupation:* Jean H. Baer, "A Study of Student Persistence at the State University of Iowa" (Office of the Registrar, State University of Iowa, 1959); Wilbur L. Bergeron, "An Analysis of the Relationship between Selected Characteristics and Academic Success of Freshmen at the University of Arkansas" (unpublished Ed.D. thesis, University of Arkansas 1953); Leon W. Bonner, "Factors Associated with the Academic Achievement of Freshmen Students at a Southern Agricultural College" (unpublished Ed.D. thesis, Pennsylvania State University, 1956); Albert B. Hood, "Certain Non-intellectual Factors Related to Student Attrition at Cornell University" (unpublished Ph.D. thesis, Cornell University, 1957); James G. Hunt, "A Study of Nonintellectual Factors Related to Academic Achievement among College Seniors at Ball State Teacher's College" (unpublished Ph.D. thesis, Purdue University, 1961); Richard A. Kidwell, "An Investigation of Selected Factors Related to Drop-out in the College of Business and Public Administration, University of Arizona, with Implications for Terminal Business Education" (unpublished Ed.D. thesis, University of Arizona, 1958); Milton W. Lehr, "A Statistical Description of Factors Related to Drop-outs and Non-Drop-outs at Northwestern State College" (unpublished Ed.D. thesis, University of Oklahoma, 1956); J. Kenneth Little, "The Persistence of Academically Tal-

tional research that met the following conditions: (*a*) sampled one or more Freshman classes that matriculated since 1950, (*b*) measured college performance in terms of either attendance or grade-point averages, and (*c*) correlated this measure to father's occupation, parents' education, or an index that combines two or more social-class characteristics. (Income was not listed in the table since very few studies had included it.) Mainly because some studies correlated performance separately with both occupation and education, while others correlated only one or the other of these variables, we find thirty-three entries in the cells of the table derived from twenty-four studies.

The one-year studies, without exception,

TABLE 1

RELATIONSHIP OF COLLEGE PERFORMANCE TO SELECTED SOCIAL-CLASS CHARACTERISTICS FOUND IN TWENTY-FOUR INSTITUTIONAL STUDIES SINCE 1950

Direction of Relationship	College Performance*								
	One Year Studies				Two to Four Year Studies				Grand Total
	Father's Occupation	Parents' Education	Composite Indexes of Social Class	Total	Father's Occupation	Parents' Education	Composite Indexes of Social Class	Total	
Positive	0	0	2	2	5	5	1	11	13
Unrelated	8	5	1	14	4	2	0	6	20
Negative	0	0	0	0	0	0	0	0	0

* College performance refers to any index of grade averages, withdrawal, or graduation

ented Youth in University Studies," *Educational Record*, XL (July, 1959), 237-41; Ben K. Patton, Jr., "A Study of Drop-outs from the Junior Division of Louisiana State University, 1953-1955" (unpublished Ph.D. thesis, Louisiana State University, 1958); Florence L. Phillips, "A Socio-economic Study of College Women" (unpublished Ed.D. thesis, Indiana University, 1958); Carroll M. Pike, Jr., "A Study of the Relationship of Selected Socio-economic Factors to Outcomes of the Program of General Education at Michigan State College" ("unpublished Ed.D. thesis, Michigan State College, 1953); Robert H. Saylor, "The Association between Selected Pre-college Experiences and Subsequent Academic Achievement of Freshmen Who Entered the Pennsylvania State University in September, 1957" (unpublished Ed.D. thesis, Pennsylvania State University, 1959); John M. Slater, "Relationship between College Persistence (Attrition), Father's Occupation and Choice of Curriculum" (unpublished Ph.D. thesis, University of Illinois, 1956); W. L. Slocum, "Social Factors Involved in Academic Mortality," *College and University*, XXXI (Fall, 1956), 53-64; Jon T. Staton, "The Relationship of Selected Factors to Academic Success for Beginning Freshmen" (unpublished Ed.D. thesis, University of Oklahoma, 1962); and Mary S. Zink, "Factors Associated with Underachievement in the Colleges of Arts and Sciences and the College of Agriculture at Cornell University" (unpublished Ph.D. thesis, Cornell University, 1960).

Parents' education: Robert L. Lathrop, "Relation of Biographical Inventory Responses to Curricular Choice and Academic Success" (unpublished Ph.D. thesis, Iowa State College, 1957); Frank E. Marsh, "An Analysis of Failure among University Freshmen" (unpublished Ed.D. thesis, Boston University School of Education, 1959); William L. Mock, "Selected Personality, Intellective and Community Characteristics as Related to Academic Success of University of Georgia Students" (unpublished Ed.D. thesis, University of Georgia, 1961); Helene Wetzler, "Diplomat or Dropout: A Study of Non-intellective Factors in Relation to College Student Attrition" (unpublished M.A. thesis, Cornell University, 1957); and seven studies from the above group which separately correlate parents' education (Baer, Bonner, Hood, Hunt, Saylor, Slocum, and Zink).

Composite indexes of social class: David Feldman, "Social Class and Academic Achievement at

find college performance *un*related to father's occupation or parents' education.[23] Two of three investigators who had employed a composite index, however, found Freshman performance positively correlated with social class. One had applied Hollinshead's Index of Social Position, while the other had developed his own index by combining scores on father's education and occupation.

In contrast, eleven of the seventeen correlations reported in the two-to-four-year studies were positive: five of nine correlating father's occupation, five of seven correlating parents' education, and one composite index. It is important to note, also, that none of the studies reported negative findings, that is, an inverse relationship between a selected characteristic of social class and performance in college. If the positive correlations were the result of chance error, then we would expect an approximately equal number of studies to have reported negative findings. Furthermore, most of the two-to-four-year studies showing inconclusive results were in the predicted direction although not statistically significant.[24]

Although the number of studies employing composite indexes is small, their favorable results in the table suggest that part of the "equivocal" nature of the research on social origins and attrition also is due to indiscriminate measures of social class. The gross occupational categories usually employed, for example, are not especially sensitive to small variations among the backgrounds of a college student population. Class lines tend to be blurred since the origins of a college population are already skewed toward the upper end of the class structure. Thus, the identification of the magnitude of the relationship between social class and college performance may depend upon a broader application of composite indexes which involve some kind of summation of two or more class characteristics.

DATA FROM A STATE UNIVERSITY

The data presented below were gathered in 1962 by the writer as part of a larger study designed to trace the academic and social careers of students who had entered college as Freshmen *ten years earlier*. The original sample consisted of an age cohort of 1,332 males born in 1934 who had enrolled at a midwestern state university in September, 1952, as full-time students without advanced standing. The main body of information was obtained from a mail questionnaire which yielded 1,180 usable returns, representing an 89 per cent response from all subjects in the original

Law School" (unpublished Ph.D. thesis, Stanford University, 1960); Stanley O. Ikenberry, "A Multivariate Analysis of the Relationship of Academic Aptitude, Social Background, Attitudes and Values to Collegiate Persistence" (unpublished Ph.D. thesis, Michigan State University, 1960); Durlyn E. Wade, "Student Success in a State Teacher's College and the Distribution of Social Class Status" (unpublished Ph.D. thesis, New York University, 1954); and Norman F. Washburne, "Socioeconomic Status, Urbanism, and Academic Performance in College," *Journal of Educational Research*, LIII (December, 1959), 130–37.

[23] Most of the one-year studies measure grade-point averages, while the two-to-four-year studies are based on dropout rates; thus, the findings here may simply reflect a difference in the relationship of social class origins to grades and to withdrawal. However, the interpretation set forth above seems equally cogent since it is *because* of poor grades that the majority of students leave college during the Freshman year.

[24] Since the difference between accepting or rejecting the null hypothesis often depends upon the manner in which occupational or educational categories are handled, it is entirely possible that some significant results are concealed that would be immediately evident if the original ungrouped data were available. For example, the State University of Iowa study was moved from "unrelated" (as concluded in the original report) to a "positive" correlation. After performing a χ^2 test with 6 degrees of freedom on a 2 × 7 table, its authors found no significant differences between the occupations of withdrawals' fathers and graduates' fathers. However, when I compared the highest status category of their occupational classification ("large business owners, most professionals, gentlemen farmers, etc.") with the lowest four categories collapsed, the resulting 2 × 2 table showed a difference significant at the 0.05 level.

sample. (Only about half of the non-returns were "refusals," while the remainder were people who could not be located by any of the search procedures we employed.) In addition, academic records were supplied by the university and by 104 colleges and universities to which many of the students had transferred.

The undergraduate population on the university campus perhaps falls in the middle range of the college status system. Although not enrolling proportionate numbers of college-going students from the lower and higher socioeconomic strata, the diversity within the university is sufficient to perform a partial test of the influence of class origins upon the academic careers of students. Table 2 illustrates the broad distribution of the study sample by Duncan's Index of Socioeconomic Status (SES) and father's occupation.

Table 3 shows how successful these students had been in attaining a college degree ten years later.[25] It will be noted that only 28.5 per cent of the entrants graduated from the university in the prescribed four years—a rate somewhat lower than that found by Iffert for publicly supported institutions. However, another 3.9 per cent had transferred, without an interruption, to a different institution and graduated on schedule in June, 1956. Another 17.3 per cent graduated after more than four years continuous attendance, either from the university or from a transfer institution. And finally, 24.5 per cent had dropped out of college but after a delay returned somewhere and graduated or are expected to graduate soon. It is especially the prolonged academic careers of these dropouts and the "four-year-plus" students that we wish to examine in relation to class origins, since together they account for *well over half of all the graduates.*

[25] A discussion of the generalizability of the high rate of graduation among the entrants of a four-year institution and a full description of the prolonged academic careers of these students are presented in Bruce K. Eckland, "College Dropouts Who Came Back," *Harvard Educational Review,* Summer, 1964.

Table 4 compares four separate stages of college performance that increasingly allow for the final graduation of the students with the most prolonged and sporadic academic careers:[26]

TABLE 2

DISTRIBUTION OF MALE FRESHMEN AT STATE UNIVERSITY IN 1952, BY INDEX OF SOCIOECONOMIC STATUS AND FATHER'S OCCUPATION

	No.	Per Cent
Index of socioeconomic status *		
90-96 (high)	33	2.8
80-89	81	6.9
70-79	165	14.0
60-69	201	17.0
50-59	136	11.5
40-49	100	8.5
30-39	153	13.0
20-29	40	3.4
10-19	234	19.8
0-9 (low)	27	2.3
Not ascertained	10	0.8
Total	1,180	100.0
Father's occupation		
Professionals	183	15.5
Proprietors, managers, and officials	356	30.2
Clerks	43	3.6
Salesmen	100	8.5
Craftsmen	208	17.6
Operatives	49	4.2
Service workers	23	1.9
Laborers	32	2.7
Farm owners, managers, and laborers	176	14.9
Not ascertained	10	0.8
Total	1,180	99.9

* Duncan's SES index is applied to the father's usual occupation when the student was growing up; see Albert J. Reiss, Jr., Otis Dudley Duncan, Paul K. Hatt, and Cecil C. North, *Occupations and Social Status* (New York: Free Press of Glencoe, 1961), Appendix B).

[26] In Tables 4, 5, 6, and 7, the 176 students with farm origins and the 10 unascertained occupations have been excluded. Duncan partly explains the difficulty of measuring the status of farm occupations: "Census figures on income do not allow for the income in kind received by many farm workers, or for the fact that the real income of farmers is probably higher in relation to their cash income than is true of urban workers, owing to cost-of-living differentials" (in Albert J. Reiss, Jr., *et al., Occupations and Social Status* [New York: Free Press of Glencoe, 1961], p. 131). Since we

Stage A deals with early persistence by computing the percentage of "stayers," that is, students who attended more than two consecutive semesters at any institution.

Stage B computes graduation rates for all students who attained a degree in four years at any institution, that is, graduated in June, 1956, or earlier.

Stage C computes graduation rates for all students who attained a degree after continuous attendance, that is, never dropped out of college before graduating somewhere.

1. All social-class indexes, except family income and who pays for college, are significantly related to *final* graduation rates (D in Table 4) at the 0.01 level or higher. Rather than a continuous linear relationship with final graduation, both of these economic variables seem to produce slightly higher graduation rates as economic resources increase but only up to the level of Status I, where the final rate of graduation suddenly drops. This curvilinearity is not

TABLE 3

DISTRIBUTION OF 1952 MALE FRESHMEN, BY GRADUATION STATUS AND DEGREE-GRANTING INSTITUTION

	Graduates				Non-Graduates		Total	
	Institution of First Registration		Transfer Institution					
	No.	Per Cent	No.	Per Cent	No.	Per Cent	No	Per Cent
Graduated in class of June, 1956, or before	336	28 5	46	3.9	.....	. . .	382	32 4
All others who graduated after continuous attendance	154	13.1	50	4 2	. .	. .	204	17.3
Dropouts who came back and later graduated	135	11.4	94	8 0	....	. .	229	19 4
Dropouts who came back and are potential graduates*	. .	.....	. .	. .	60	5.1	60	5.1
Dropouts who came back but whose graduation appears unlikely	. .	.	.	. .	128	10 8	128	10.8
Dropouts who never came back	. .	. .			177	15 0	177	15 0
Total	625	53.0	190	16 1	365	30 9	1,180	100 0

* Potential graduates are all students who had been attending college somewhere during the past year (i e , between July 1, 1961, and June 30, 1962) and had over 5 full-time semesters of attendance behind them (actually, 42 of the 60 students who fit this description had attended over 7 semesters).

Stage D computes graduation rates for all students who attain degrees, including the dropouts who came back and who are actual or potential graduates.

The data in Table 4 support the following conclusions:

found in the rates for the stayers and the four-year graduates. Furthermore, the 2×2 statistical tests of difference on family income and who pays for college remain significant throughout the first three stages, that is, including the rates for all students who graduated after continuous attendance. Therefore, the similar rates of *final* graduation between Statuses I and V on the economic variables chiefly reflect the ability of many dropouts who came back to overcome the lack of financial resources and eventually graduate. *All of the remain-*

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have employed Duncan's SES index, we shall follow his example (Duncan and Hodge, *op. cit.*, p. 632) and confine our analysis to students with non-farm origins. Our data, too, indicate that retaining the farm group might seriously "muddy the waters." As many as 160 of the 176 students with Midwestern farm origins received SES scores under 20.

TABLE 4

Early Persistence and Graduation Rates (Cumulative) for 1952 Male Entrants, by Selected Social-Class Characteristics

	Total No.	Per Cent			
		A. Stayers (Returned Sophomore Year)	B. Four-Year Graduates	C. All Who Graduated after Continuous Attendance	D. All Graduates (Including Dropouts Who Later Graduate)
All non-farm	994	78.7	31.3	49.2	74.8
Index of socioeconomic status:*					
I	114	86.0	41.2	63.2	86.8
II	164	82.9	31.1	50.0	78.0
III	437	79.6	32.0	49.7	75.7
IV	178	72.5	27.5	45.5	73.0
V	101	70.3	23.8	36.7	55.4
Significance of difference: I and V		0.01	0.01	0.001	0.001
Father's occupation (grouped):†					
I	183	81.4	39.9	59.0	82.0
II	356	80.9	31.2	50.6	76.4
III	143	80.4	32.9	51.0	72.7
IV	208	72.1	24.5	39.9	71.2
V	104	76.9	27.9	43.3	67.3
Significance of difference: I and V		N.S.	0.05	0.02	0.01
Parents' education:‡					
I	115	82.6	42.6	62.6	84.3
II	144	83.3	26.4	46.5	81.3
III	410	79.0	33.7	49.5	74.6
IV	221	75.6	28.1	48.0	71.0
V	102	73.5	23.5	40.2	65.7
Significance of difference: I and V		N.S.	0.01	0.001	0.01
Parents assumed children would go to college:§					
I	519	84.4	36.6	55.1	79.2
II	391	73.4	26.1	44.0	71.9
III	82	68.3	22.0	36.6	62.2
Significance of difference: I and III		0.001	0.01	0.01	0.001
1952 family income:‖					
I	146	88.4	37.7	53.4	73.3
II	142	81.0	33.1	56.3	81.0
III	207	79.2	30.9	49.3	79.2
IV	299	75.6	32.4	48.8	74.6
V	170	72.9	27.1	41.8	68.8
Significance of difference: I and V		0.001	0.05	0.05	N.S.
Parents pay for college expenses:#					
I	197	80.7	38.6	55.3	67.0
II	216	88.9	36.6	60.6	79.6
III	195	82.1	37.4	57.4	78.5
IV	206	76.2	20.9	36.4	77.7
V	174	64.4	22.4	35.1	70.7
Significance of difference: I and V		0.001	0.001	0.001	N.S.

* See Table 2, n.*; I (80–99), II (70–79), III (40–69), IV (20–39), V (0–19).

† I (professionals), II (proprietors, managers, and officials), III (clerks and salesmen), IV (craftsmen), and V (operatives, service workers, and laborers).

‡ I (8–10), II (6–7), III (3–5), IV (1–2), V (0). The scores combine father's and mother's education by adding the assigned values: (0) eighth grade or less, (1) some high school, (2) high-school graduate, (3) some college, (4) college graduate, and (5) graduate or professional degree.

§ The relevant questionnaire item asked the respondents: "Which of the following best describes the situation in your family when you were growing up?" Answers provided were: (I) It was "naturally assumed" that the children would go to college. (II) Children who wanted to go to college were encouraged to do so by one or both parents, but it wasn't assumed that all would go. (III) It was not assumed that any of the children would go to college.

‖ I ($15,000 and over), II ($10,000–$14,999), III ($7,500–$9,999), IV ($5,000–$7,499), V ($0–$4,999).

Refers to the portion of funds coming from "parents, relatives, or other specific individuals": I (all, or nearly all), II (about three-fourths), III (about one-half), IV (about one-fourth), V (none, or almost none).

ing indexes show a consistent linear relationship to final graduation.

2. The SES index (a score from 0 to 96 which combines census-derived norms on the education and income levels of persons employed in each of some 446 occupations) is a better predictor of college success than the separate indicators—education, income, and occupation (by grouped data). The difference, for example, between the final graduation rates (D) for students in Statuses I and V (SES) was 31.4 per cent. Although the SES is scored from only one item of information (father's occupation), the normative properties it contains make it similar to a composite index. Undoubtedly, the predictive quality of the SES index is also increased by its ability to stratify a skewed college student population while avoiding some of the well-known difficulties of both prestige ratings of occupations and the census major occupation groups.

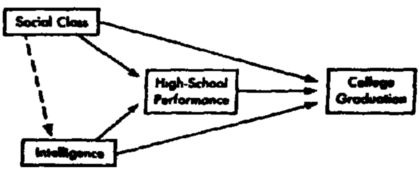


FIG. 1.—Relationship of causal variables purported to determine college graduation.

3. When Statuses I and V are compared on father's occupation (grouped) and parents' education, early persistence (A) varies in the predicted direction but not significantly. In contrast, the four-year graduation rates differ significantly on these two variables. This finding is consistent with the separate analysis above of twenty-four institutional studies (see Table 1), which indicated that one-year measures of college performance, without exception, were not statistically related to the occupation or education of the students' parents. Early dropout, tends, rather, to be more closely associated in Table 4 with low status on the economic variables—an association which may only be temporary.

4. As predicted, the inclusion of four-year-plus graduates (C) and dropouts who later graduate (D) generally increases the predictive ability of all non-economic items. The statuses of these late graduates, on the whole, resemble the class composition of the four-year graduates more than they do that of the permanent dropouts. For instance, the percentages of four-year, four-year-plus, and dropout graduates in Status V (SES) were 7.7, 7.3, and 7.5, respectively, while 18.0 per cent of the non-graduates were located here. To compare collectively both late graduates, who represent over half of all students who attained degrees in this study, and non-graduates against four-year graduates (as most previous research has done) would have partly obscured the true relationship between college graduation and class origin.

A SPURIOUS CORRELATION?

The major challenge to the above propositions perhaps is the possibility of having drawn a spurious correlation with intervening intellective factors. To begin with, in a general population, I.Q. and high-school performance are related to social class. However, the amount of variance in I.Q. that can be explained by class differences is relatively small. The congruity of performance in high school and social class, on the other hand, is more clearly due to social conditions and the amount of variance in performance that can be accounted for by these conditions may be quite high. This means that high-school grade averages are a mixture of probably about equal portions of intellective and non-intellective factors.[27]

Figure 1 illustrates the temporal sequence of the relationship between the above three variables—all of which may affect college graduation. The high predictive quality of high-school rank and aptitude tests was noted in our introduction and is shown below in Table 5 for the 1952 university entrants. Perhaps any correlation found between social class and college graduation is simply a function of the as-

[27] Fishman, *op. cit.*, pp. 477–78.

sociation between social class and the intellective factors which, themselves, correlate so well with college graduation. In addition, it could be argued that the aforementioned "college status system" represents a consequential stratification of colleges not by the class origins of students but rather by their intellective abilities, since the institutions whose students are upper-middle class tend to be those whose admissions policies are most selective. Is the correlation of social class and graduation simply incidental and, thus, the diversity hypothesis relevant only in terms of intellective variability as an explanation for the differential rates of graduation between institutions?

On theoretical grounds I would argue that our propositions are *not* spurious. First, consider the direction and sequence of the relationships outlined in Figure 1. As ascriptive elements, neither social class nor intelligence is antecedent to the other, except that intelligence later may vary according to class when measured by I.Q. or aptitude tests. Therefore, the main question centers around the mixed intellective and social factors that produce the kinds of study habits and academic interests and values that, in turn, determine high-school grades. It may be undesirable, however, to unmix these factors when the whole represents something more than a simple summation of its parts. High-school performance reveals an inseparable complex of intellective and social processes.

Variations in college graduation rates that may be attributed to high-school performance can just as easily be interpreted as variations of social class as they can variations of intelligence. Undoubtedly, it is *because* rank in high school involves social factors that it is a much better predictor of college success than rank in aptitude tests which supposedly measure intelligence.[20] The association of social class and college graduation, then, is far more than incidental in the life-history of the student. Since class is antecedent to high-school performance, and not the reverse, the association of the intervening variable with the independent and dependent variables should add to the importance of class origins rather than detract from it.

Let us concede for the moment that it would still be appropriate to separate these components by partialing out the intellective factors. We would then ask: Regardless of how important class origins are to high-school rank, or even to rank in aptitude tests, do class differences independently determine who graduates from college? The state university data will be employed here again. (A second reduction in the sample from 994 to 580, however, was required in compiling the figures in Tables 5, 6, and 7 due to unavailable high-school rank data for 61 students and ACE test scores for 353 students.)

TABLE 5

FINAL GRADUATION RATES FOR 1952 MALE ENTRANTS, BY HIGH SCHOOL RANK AND COLLEGE APTITUDE TESTS

	No. of Entrants	Per Cent Graduating
High school percentile rank:		
80-99 (high)	244	88.9
60-79	170	74.7
40-59	84	72.6
20-39	56	53.6
0-19 (low)	26	34.6
ACE decile rank		
8-9 (high)	135	88.9
6-7	110	83.6
4-5	140	73.6
2-3	132	66.7
0-1 (low)	63	65.1

Table 6 shows the predicted association of class status and aptitude tests, with higher-status students (SES) receiving higher scores on the American Council of Education exams. The association of class status and high-school percentile rank is *not* as anticipated, for the lower-status students had done slightly better in high school. Unquestionably, this is a function of the type of students who go to a state-supported university where the open doors

[20] Bloom and Peters, *op. cit.*, pp. 8-22.

make it possible, and likely, for upper-status groups to send their children to college even though they have done poorly in high school. Actually, the slight bias toward lower-status students with better high-school records should have increased the chance of accepting the null hypothesis in our previous tests of difference, since these school records of the lower-status students should have favored their eventual graduation from college.

The correlations in Table 7 control for the effects of high-school performance and aptitude tests. We find *in general* that the social-class variables do not correlate independently with college graduation. Among students who ranked in the lower three-quarters of their high-school graduating class, however, there are fairly consistent differences between the graduation rates of low- and high-status groups on all non-economic items. Among upper-quarter high-school students, all of the correlations, except SES in column (3), are in the predicted direction but only one is statistically significant.

This finding—that social class is an important determinant of college graduation for students from the lower rank of their high-school classes but relatively unimportant for those from the higher rank—is not altogether unexpected. Students who did well academically in high school had already displayed a level of skill and motivation which normally would carry them through college to graduation. Since it has been suggested that motivation is the primary link between social class and college performance, it is not likely that class differences will significantly influence graduation rates in a group previously characterized as academic achievers. Performance levels are so high that nearly all graduate, irrespective of class origins.

TABLE 6

PERCENTAGE DISTRIBUTIONS OF 1952 MALE ENTRANTS WHO RANKED HIGH AND LOW IN HIGH-SCHOOL GRADUATING CLASS AND ON COLLEGE APTITUDE TESTS, BY INDEX OF SOCIOECONOMIC STATUS

SES	High-School Percentile Rank				ACE Decile Rank			
	0–74 (Low)	75–99 (High)	Total	N	0–4 (Low)	5–9 (High)	Total	N
I–II	50 9	49.1	100.0	(171)	37.4	62.6	100 0	(171)
III	51.1	48.9	100.0	(237)	48 1	51 9	100.0	(237)
IV–V	45.3	54.7	100.0	(172)	57.0	43 0	100.0	(172)

CONCLUSIONS

We alluded earlier to several major trends in the United States that currently affect who goes to college and where: the increasing selectivity of many institutions, the expansion of educational opportunities, and the growth of mass higher education. Tentatively, how might these trends affect the relationship of social class and college graduation?

Among institutions employing *rigorous selection*, we should expect to find less intellective diversity and more social diversity than in the past. A broadcasting social base at individual colleges would imply somewhat less class diversity between institutions than what we have suggested, and thus it would now be possible to find the predicted correlation between class and graduation at each institution, should it exist. However, there is just as much variation in the use of selective devices by different colleges as there is in the kinds of students who are admitted. There also is the association between social class and the

very intellective criteria by which colleges screen their applicants, a relationship that is deeply rooted in our system of secondary education. Therefore, it is quite unlikely that the trend described here will shortly obliterate the social pyramiding of institutions. The diversity hypothesis is still required to explain the *full* impact of social origins upon college performance.

In spite of the *expansion of educational opportunities* to larger segments of the college-age population, many high-school graduates who do not go to college still profess that it is because they cannot afford to

TABLE 7

FINAL GRADUATION RATES FOR 1952 MALE ENTRANTS, BY SELECTED SOCIAL-CLASS CHARACTERISTICS, CONTROLLING HIGH-SCHOOL RANK AND COLLEGE APTITUDE TESTS

	PERCENTILE RANK IN HIGH-SCHOOL GRADUATING CLASS			
	0-74 (Low)		75-99 (High)	
	ACE Decile Rank 0-4 (Low) (1)	ACE Decile Rank 5-9 (High) (2)	ACE Decile Rank 0-4 (Low) (3)	ACE Decile Rank 5-9 (High) (4)
All non farm	61.1 (167)*	73.1 (119)	78.9 (109)	91.4 (185)
Index of socioeconomic status:				
I-II	77.5 (40)	83.0 (47)	79.2 (24)	95.0 (60)
III	68.1 (69)	71.2 (52)	75.6 (45)	91.5 (71)
IV-V	41.4 (58)	55.0 (20)	82.5 (40)	87.0 (54)
Significance of difference: I-II and IV-V	0.001	0.02	N.S.	N.S.
Father's occupation (grouped):				
I-II	66.2 (77)	82.2 (73)	82.1 (56)	95.8 (96)
III	62.5 (24)	66.7 (15)	72.7 (11)	85.2 (27)
IV-V	54.5 (66)	54.8 (31)	76.2 (42)	87.1 (62)
Significance of difference: I-II and IV-V	N.S.	0.01	N.S.	0.05
Parents' education:				
I-II	72.7 (33)	80.4 (46)	84.0 (25)	94.6 (56)
III	66.7 (66)	68.4 (38)	76.7 (43)	92.8 (69)
IV-V	50.0 (68)	68.6 (35)	78.0 (41)	86.7 (60)
Significance of difference: I-II and IV-V	0.05	N.S.	N.S.	N.S.
Parents assumed children would go to college:				
I	68.4 (79)	80.6 (67)	84.1 (44)	93.3 (105)
II-III	54.0 (87)	63.5 (52)	75.4 (65)	88.8 (80)
Significance of difference: I and II-III	N.S.	0.05	N.S.	N.S.
1952 family income:				
I-II	63.3 (49)	82.5 (40)	87.0 (23)	91.2 (34)
III	71.8 (39)	72.4 (29)	75.0 (16)	91.9 (37)
IV-V	54.7 (75)	66.7 (45)	77.6 (67)	90.8 (109)
Significance of difference: I-II and IV-V	N.S.	N.S.	N.S.	N.S.
Parents pay for college expenses:				
I-II	60.0 (65)	71.7 (60)	90.3 (31)	92.2 (64)
III	65.4 (26)	69.2 (26)	76.2 (21)	97.8 (45)
IV-V	59.5 (74)	78.8 (33)	73.7 (57)	86.7 (75)
Significance of difference: I-II and IV-V	N.S.	N.S.	N.S.	N.S.

* Figures in parentheses indicate the number of subjects in each subgroup upon which the separate rates are based

go,[29] and the same reason is one of those most frequently cited by college dropouts.[30] On the other hand, financial limitations can sometimes be surmounted by those who are highly motivated and may only be a convenient excuse for those who are not.[31] Our findings substantiate the conclusions of other recent studies that economic variables actually do not contribute to a student's level of educational achievement to the extent previously thought.[32] This is not to dismiss the importance of economic factors, even in a society that interprets "equality of opportunity" to mean the right of all to go to college. It certainly does not dismiss the importance of social-class origins, since the dominant effect of class differences appears to be cultural.

Supported by the expansion of educational opportunities and changes in our occupational structure, there is finally the trend toward a system of *mass higher education*. With the rapid growth in the proportion of high-school graduates who go to college, we must anticipate an eventual increase in the relative number of college students with both lower-status origins and only average high-school records.[33] The shifting social base of the college population has already been observed on most campuses.[34] The shifting academic aptitude of the college population, however, is less discernible[35] (for two reasons—(1) the selectivity of some colleges, which so deceives us that we overlook the hastening evolution of the junior college that harbors the backwash of academic talent and (2) a real lag in the expected shift due to a temporary cessation in enrolments just before the full impact of the postwar baby boom strikes the college campuses). Nevertheless, the changes which are clearly anticipated mean that social-class differences will increasingly determine who graduates among the college entrants of the next few decades, since it is apparently among the less qualified students that class origins have the greatest impact. To the extent that higher education develops the same mass conditions as our system of secondary education in which dropout has been so closely associated with social status, a similar process likely will occur in our colleges.

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[29] E.g., see J. Kenneth Little, "The Wisconsin Study of High School Graduates," *Educational Record*, XL (April, 1959), 123–28.

[30] Representative studies that have found financial problems ranked either first or second among self-reported reasons for leaving college include: Iffert, *op. cit.*, p. 91 (males only); Carl A. Peterson, "A Two-Year Study of Causal Factors in Male Student Drop-outs at the University of Pittsburgh, 1955–1957" (unpublished Ph.D. thesis, University of Pittsburgh, 1958); and Thomas Ewing, "Survey of Students Graduating in the Top Quarter of Their High School Class Who Entered the University in the Fall of 1959, and Did Not Return to School for the Following Semester" (unpublished report, Student Counseling Service, University of Illinois, 1959).

[31] About 40 per cent of the university dropouts reported "financial difficulties" as one of the three most important reasons for leaving college. However, nearly half of those reporting such difficulties returned to graduate. Also see Hollinshead, *op. cit.*, pp. 74–76.

[32] Herriott, *op. cit.*, pp. 170–71; John B. Lansing, Thomas Lorimer, and Chikashi Moriguchi, *How People Pay for College* (Ann Arbor: Survey Research Center, University of Michigan, 1960), pp. 128–29; Elizabeth Douvan and Carol Kaye, "Motivational Factors in College Entrance," in Sanford (ed.), *op. cit.*, pp. 199–224; and Martin David, Harvey Brazer, James Morgan, and Wilbur Cohen, *Educational Achievement—Its Causes and Effects* (Ann Arbor: Survey Research Center, University of Michigan, 1961).

[33] Clark, *op. cit.*, p. 208.

[34] Joshua A. Fishman, "Discussion" (of Philip E. Jacob's "Social Change and Student Values"), *Educational Record*, XLI (October, 1960), 342–46.

[35] A Minnesota study shows that today's college students in that state do not differ markedly, either in aptitude-test scores or high-school rank, from students twenty-six years ago (Ralph F. Berdie, Wilbur L. Layton, Theda Hagenah, and Edward O. Swanson, *Who Goes to College* [Minneapolis: University of Minnesota Press, 1962]).

Item Order and Guttman Scales[1]

Donald P. Hayes

ABSTRACT

One deduction from balance theory is that respondents will tend to integrate their answers to a set of attitude items. A second deduction, from classical figure-ground relations, is that the first item of a set of attitude items will, under certain conditions, influence responses to subsequent items. Two experimental studies dealing with these deductions for Guttman scales are reported. The results show that individual responses to certain Guttman scales may be systematically manipulated by item rearrangement. The implications of these findings for interpreting scale patterns and the role of contextual factors in attitude measurement are discussed.

One general hypothesis in cognitive balance theories is that where several highly related pieces of information are presented for judgment, respondents characteristically attempt to integrate their answers.[2] Each new bit of information is fitted into or rejected from a structure formed by previous judgments. The consequence may be a more or less consistent set of responses. Applied to attitudes, where an attitude structure is already well formed, the first of a set of related attitude questionnaire statements serves as the figure against the ground of the previously established attitude structure.[3] Where no attitude previously existed, the first statement of the questionnaire becomes the ground for the second.

BACKGROUND FOR THE STUDY

Among the several attitude-scaling methods, a set of items which fulfils the requirements of a Guttman scale has properties that are quite likely to be affected by these balance tendencies.[4] The scale items must come from a common universe and they must have a clear internal order with minimal error in the placement of items. Because the items are so highly related, respondents may attempt to be consistent in their answers.[5] It is generally the practice to present Guttman scale items without regard to their actual or suspected order. Should no well-established and relevant cognitive structure exist, the first item of the Guttman scale is of special significance since it tends to be a reference point for successive judgments. One inference would be that a different pattern of item indorsement may develop if a scale is first introduced by an item representing one end of the attitude continuum than if it is intro-

[1] The author wishes to thank Walter B. Watson, Ernest A. T. Barth, William R. Catton, and John B. Hudson for helpful comments on earlier drafts of this manuscript.

[2] F. Heider, "Attitudes and Cognitive Organization," *Journal of Psychology*, XXI (1946), 107-12; and his *The Psychology of Interpersonal Relations* (New York: John Wiley & Sons, 1959); L. Festinger, *A Theory of Cognitive Dissonance* (Evanston, Ill.: Row, Peterson & Co., 1957); Milton J. Rosenberg, "An Analysis of Affective Cognitive Consistency," chap. ii, in *Attitude Organization and Change*, ed. M. Rosenberg, C. Hovland, W. McGuire, R. Abelson, and J. Brehm (New Haven, Conn.: Yale University Press, 1960); T. Newcomb, "An Approach to the Study of Communicative Acts," *Psychological Review*, LX (1953), 393-404.

[3] C. Hovland *et al.*, *Order of Presentation in Persuasion* (New Haven, Conn.: Yale University Press, 1960).

[4] S. A. Stouffer, L. Guttman, E. Suchman, P. Lazarsfeld, J. Clausen, and S. Star, *Measurement and Prediction* (Princeton, N.J.: Princeton University Press, 1950).

[5] One wonders whether high reproducibility in a Guttman scale reflects the respondent's capacity for integrating a set of items through balance mechanisms, a characteristic which may be subject to vast individual differences, or independently reflects the inherent consistency of the items.

duced by an item representing the opposite end of the continuum. Should the Guttman scale deal with a highly integrated cognitive structure, the order in which the items appear should not significantly affect the distribution of responses.

A second inference from cognitive balance tendencies for Guttman scales has to do with their degree of reproducibility. Since Guttman items tend to be drawn from a common attitude universe, any arrangement of items that interferes with the strong integrative tendencies should have the effect of reducing the effectiveness of that integration. Some orders of scale items may facilitate, others hamper the integration.

Stating the hypotheses explicitly:

Hypothesis 1. Despite the use of the same scale items, distributions of respondents by scale type will differ when the statements representing the two extremes of the scale are used as the first item.

Hypothesis 2. Any arrangement of scale items that facilitates the integration of responses increases the coefficient of reproducibility.[6]

- *a*) Items of a scale which are separated by irrelevant material should have lower coefficients of reproducibility than scale items which are adjacent to one another.
- *b*) Items arranged in serial order should have higher coefficients of reproducibility than items whose order is disarrayed.

Hypothesis 3. The conditions described in Hypotheses 1 and 2 vary inversely with previous knowledge and structuring of the attitude universe by the respondent.

STUDY DESIGN

Approximately 700 college students at two universities participated in a series of experiments.[7] Each responded to two Guttman scales. The first deals with general manifest anxiety. The second, though not an attitude scale, had the properties of a Guttman scale. It dealt with conventional mathematical sophistication.[8]

In the first series of experiments, scale items were arranged in either serial or disarrayed order, and either adjacent to one

[6] Hypothesis 2 is the general statement from which Hypotheses 2*a* and 2*b* are derived. The empirical tests are of these more specific statements.

[7] In Tables 1 and 2, Population I refers primarily to freshmen and sophomores at the University of Washington (1961–62), and Populations II and III refer to freshmen and sophomores at the University of California (Riverside) (1962–63). Tables 3 and 4 are entirely students from the UCR.

[8] The anxiety-scale statements, arranged in the "best" scale order, were each followed by a line divided by five equally spaced numbers. One end of the line was anchored with the phrase "No nervousness or anxiety," the other end with "Very nervous or anxious." Respondents were instructed to circle the number best representing their own point of view. The responses were dichotomized by splitting the answers at the same point on each of the seven scale items; the first and second numbers were combined against the third through the fifth. It was noted, however, that consistently different minimax solutions characterized male and female respondents; i.e., the order of items and the "best break" were not consistent within the populations and between populations, a fact previously noted in an unpublished manuscript by E. A. T. Barth, sociologist at the University of Washington who first worked with this scale. In nearly all instances, reproducibility was over .90, and there was adequate dispersion of the population by scale types. The anxiety scale consisted of the following items arranged in serial order:

1. Trying on clothing in a store
2. Meeting relatives (not in-laws) for the first time
3. Blind dating
4. Trying to introduce a friend whose name you have suddenly forgotten
5. Being stopped by a traffic officer
6. First day on a new and important job
7. Speaking before a large group.

The mathematics scale items, constructed by Walter Watson, were coded simply right or wrong. The items were ordered in a pattern corresponding to conventional mathematics training in high school. Reproducibility tended to approximate .95 with adequate dispersion. There was no tendency for the minimax solution of mathematics items to differ by sex or university population. Differences in experimental treatments cannot be attributed to allocation of non-scale responses since the scaling

another or separated by a number of irrelevant items. There were thus four forms: items arranged in serial order, adjacent; serial order, separated; disarrayed order, adjacent; and disarrayed order, separated. The forms were distributed at random. Subjects were asked to respond to all items and to treat the items in the order in which they appeared rather than skip around. With some inevitable exceptions, most subjects complied. The anonymous questionnaire was filled out and collected, and the purpose of the experiment revealed.

In the second series of experiments, only the order of the scale items was varied, since by that time the adjacency condition was found to produce negligible effects. Each subject was given the same questionnaire twice, a period of three months intervening. This design provided the opportunity to assess the effects of different item order on the same respondent, rather than using different groups as in the first experiment. Of four groups, one received the serial order form twice, another the disarrayed order twice. The other two received forms which were switched from the first to the second administration, one from serial to disarrayed order, the other from disarrayed to serial order. In all other respects, the experiments were alike.

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was identical in all treatments and populations. The mathematical items were as follows:

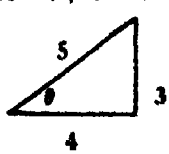
1. Add $+3$ / -8

2. $X - 7 = -4$; $X = ?$

3. $\dfrac{1/2}{1/3} = ?$

4. $X = \dfrac{Y + 10}{5}$; $Y = ?$

5. $X + 4Y = 1$, $2X - Y = -7$; $X = ?$; $Y = ?$

6. 

$\tan \theta = ?$

7. Differentiate X^2.

RESULTS OF THE FIRST EXPERIMENT

Hypothesis 1 deals with the effects of the first item on subsequent judgments. Subjects receiving the serial-order treatments confronted first the least difficult mathematics problem and least anxiety-provoking item. Subjects receiving the disarrayed-order treatments came first upon the next to most difficult mathematics question and the most anxiety-provoking situation of the seven items.

The data in Tables 1 and 2 are consistent with Hypothesis 1. The distribution of anxiety scale types in the two treatments is significantly altered by the rearrangement of items in each of the three independent populations. If this type of scale is used to infer anxiety levels, those receiving the disarrayed form appear to be significantly more anxious than those receiving the serial form. This interpretation is unlikely to be correct, however, since the forms were distributed to the subjects at random. The order in which the items are presented apparently affects the patterning of responses significantly. The mathematics scale was not similarly affected by the ordering of items.

Hypothesis 2, relating cognitive balance tendencies to the coefficient of reproducibility, is tested in two ways—first, by comparing groups whose scale items were adjacent to one another with groups whose items were separated by irrelevant material and, second, by comparing the reproducibility of scale items arranged in serial and disarrayed order.

No differences in the reproducibility criterion that could be attributed to the degree of adjacency of scale items were found in either the mathematics or anxiety scale, a fact contrary to Hypothesis 2b. The degree of cognitive integration was not sig-

nificantly affected when three or four irrelevant items of a similar kind (other attitude items and standard information questions) appear between scale items.

With respect to the effects on reproducibility of serial and disarrayed order of scale items, a pre-test revealed that respondents more frequently recognized the implicit scale and were able to respond to it more easily when the scale was presented in serial order than when the items were disarrayed. For this reason, reproducibility was predicted to be greater in the serial-order treatment. With the mathematics scale, however, no statistically significant differences were found. The total of errors in the three independent samples was, to be sure, proportionately larger in the disarrayed form as predicted, but such a difference could easily have occurred by chance.

Although the results of the mathematics scale are not consistent with Hypothesis 2, those on the anxiety scale are. In each of

TABLE 1

DISTRIBUTION OF SCALE TYPES BY SERIAL OR DISARRAYED ORDER FOR ANXIETY

Anxiety-Scale Type	Population I* (Per Cent)		Population II† (Per Cent)		Population III† (Per Cent)		Combined Populations I, II, III (Per Cent)	
	Serial	Disarrayed	Serial	Disarrayed	Serial	Disarrayed	Serial	Disarrayed
Low level 0–1	36	19	30	16	40	16	35	18
2–3	36	46	49	48	42	44	40	46
4–5	20	22	13	24	10	27	17	23
High level 6–7	8	13	8	12	8	13	8	13
	(199)	(199)	(84)	(83)	(60)	(68)	(343)	(350)
χ^2	15 5		7.0		11 3		31 0	
P	<.01		<.05		<.01		< 01	

* University of Washington.
† University of California (Riverside).

TABLE 2

DISTRIBUTION OF SCALE TYPES BY SERIAL OR DISARRAYED ORDER FOR MATHEMATICS

Mathematical-Scale Types	Population I* (Per Cent)		Population II† (Per Cent)		Population III† (Per Cent)		Combined Populations I, II, III (Per Cent)	
	Serial	Disarrayed	Serial	Disarrayed	Serial	Disarrayed	Serial	Disarrayed
Low sophistication 0–1	18	14	4	5	8	3	13	10
2–3	43	49	29	25	21	28	35	39
4–5	17	14	27	30	24	28	21	21
High sophistication 6–7	22	23	40	40	47	40	31	30
	(204)	(203)	(90)	(89)	(63)	(67)	(357)	(359)
χ^2	3.04		.49		2.97		1.41	
P	>.30		>.90		>.30		<.70	

* University of Washington.
† University of California (Riverside).

the three independent samples, the serial-order presentation produced fewer errors than the disarrayed order. The probability that the combined observed differences could have occurred by chance is less than .01. Both arrangements of items, however, fulfil the reproducibility criteria for adequate Guttman scales.

Hypothesis 3 asserts that the conditions described in the first two hypotheses will not hold if the universe of content, as measured by the scales, has been previously well structured by the respondent. Years of schooling have tended to structure the mathematics scale items whereas the anxiety scale contains items which are not conventionally ordered. The findings in Tables 1, 2, and 3 are consistent with Hypothesis 3 in that the mathematics scale was much less susceptible to the effects of different orders of presentation than the anxiety scale.

TABLE 3

SCALE REPRODUCIBILITY

	Population I*		Population II†		Population III†		Three Samples Combined	
	Serial	Disarrayed	Serial	Disarrayed	Serial	Disarrayed	Serial	Disarrayed
	Anxiety							
No. of errors	97	138	46	66	39	48	182	252
N	194	206	84	83	60	68	338	357
Coefficient of reproduction	933	904	922	.886	910	900	923	899
	Mathematics							
No. of errors	78	71	40	42	16	30	134	143
N	202	203	90	89	63	67	355	359
Coefficient of reproduction	.944	950	.936	933	964	936	946	943

* University of Washington.

† University of California (Riverside).

RESULTS OF THE SECOND EXPERIMENT

The second experiment sought to demonstrate that a subject's level of anxiety, as revealed by the Guttman scale, can be deliberately and predictably manipulated by the use of different forms of the same scale. Subjects were given a questionnaire, with items arranged in serial or disarrayed order, on two occasions separated by three months. The serial arrangement was used both times with one group, the disarrayed arrangement both times with another, a serial and then a disarrayed pattern with a third group, and a disarrayed followed by a serial pattern with a fourth. Both the effect of the arrangement of scale items and the stability of the respondents' scale type over that time period were assessed.

Approximately equal proportions of subjects in the four experimental groups retained the same scale type from the first to the second administration of the questionnaire (see Table 4). For those whose anxiety-scale types changed, however, there was a significant difference in the direction of that shift. The groups that changed the most were Group II (serial to disarrayed order) and Group III (disar-

rayed to serial order). Of the respondents in Group II, 57 per cent shifted to a more anxious level on the scale, while 19 per cent of the Group III respondents shifted in that direction. Forty-three per cent of the Group III subjects shifted to a less anxious scale type, while 10 per cent of the Group II subjects did so. Although the modal scale type in all groups was unchanged from the first to second administration, there were substantial internal distribution changes, their direction depending on the experimental treatment. This finding coincides with the results of the earlier experiment in which groups exposed to the several arrangements of items responded with different distributions of scale types. In both sets of experiments, the effect of the serial presentation leaves the impression of reduced anxiety in comparison to the disarrayed order. How much of a real increase or reduction in anxiety this represents is impossible to ascertain with ordinal data, but care would have to be taken to recognize that the differences in results with these two forms may be nothing more than an artifact of the method of measurement, rather than any fundamental difference in anxiety between two groups.[9]

TABLE 4

RESPONSE STABILITY ON ANXIETY SCALE

Change in Anxiety	Treatment			
	I Serial-Serial (Per Cent)	II Serial-Disarray (Per Cent)	III Disarray-Serial (Per Cent)	IV Disarray-Disarray (Per Cent)
Less anxious	36	10	43	32
As anxious	47	33	38	32
More anxious	17	57	19	36
	(30)	(30)	(37)	(31)

DISCUSSION AND IMPLICATIONS

The second experiment especially makes the point that responses to the general anxiety scale can be manipulated by employing different arrangements of items. The question this fact poses is: *Which of the several resultant scale types is to be taken as representing the respondent's level of anxiety?* The question may be answered by positing a distribution of responses to such scales, the distribution having a point of central tendency. The subject's responses must be treated as an estimate of this point. The fact that these subjects were often represented by two different scale values may then be interpreted in several ways: (1) there was an actual change in the level of anxiety during the intervening three months; (2) the scale lacks reliability; or (3) as Sherif and Hovland have shown, there is an inverse relationship between the intensity of one's latent scale position and the range of scale items one will indorse—thus the results of this experiment might mean that those who shifted their scale type were simply less anxious than those whose scale type remained unchanged.[10]

An alternative explanation of the instability of the respondent's answers will be familiar to anyone who has had extensive interviewing experience. A frequent reaction to a question begins, "Well, it de-

[9] *If* one's objectives in using a Guttman scale are to facilitate the respondents' answers, improve the respondents' chances of guessing the implicit scale, and minimize errors, there is evidence here that serial order has these qualities. Scattering items or keeping them together, at least in this study, did not affect the responses.

[10] M. Sherif and C. Hovland, *Social Judgment* (New Haven, Conn.: Yale University Press, 1961).

pends . . . ," which represents the contingent character of responses on the various contexts for which the attitude or opinion may be appropriate. Herbert Hyman has reviewed a large literature with reference to the effects of different interview contexts on responses.[11] Specifically, Raymond Gordon's study of the manifest views of pro-Russian students in the presence of a mixed audience illustrates one such contextual effect.[12] Although the publicly expressed views of some of the subjects were divergent from the views they expressed to a less hostile audience, their general position was maintained. There appeared to be limits to the range of acceptable positions beyond which more divergent views would not be expressed. Within certain limits, the contexts appear to have controlled the expressions of their manifest views. A report by Goldberg, Sharp, and Freedman on expected family size indicated substantial differences between those questioned about future fertility after thirty minutes of questions related to their religious interests and those who were not asked such questions before responding to future fertility questions.[13] The implication of this literature for the Guttman scale results is that each item in a scale, and especially the first item, serves as a context for subsequent items. Thus the rearrangement of items in the experiments produced different contexts for responding to subsequent items, despite the fact that the items themselves remained unchanged. In short, the effects of item order on the distribution of scale types and reproducibility may be considered, as in Gordon's study, among the many contextual conditions giving rise to response instability.

One conclusion of these comments is that since attitude expression is highly contingent upon the context, the effect of the contingencies is to produce a *range* of expression. This range is thus due to the effects of two components: first, the effects of these different contexts; and second, the common assumption of most attitude instruments (including, though explicitly denied by Guttman, Guttman scales as commonly used in practice) that an individual's manifest expressions of an attitude bear a probabilistic relationship to a specific point on his latent attitude continuum. The probability relationship contributes to the range of manifest expressions of the latent attitude. Attempts to measure attitudes from general expressions will commonly fail to elicit this full potential range without deliberate efforts to vary the contexts.

The ranges of acceptance and rejection of items, as Sherif and Hovland have shown, raise a question concerning what one does with this information. Traditionally, the median response of the indorsed items is used in Thurstone scales on the grounds that this is the best approximation of the respondent's position on his latent attitude continuum. Thus the range is ignored for a measure of central tendency. This practice may be quite satisfactory when obtained from highly partisan subjects since they have narrow ranges of item acceptance. The use of central tendency measures may be less than satisfactory for persons having wide ranges of acceptance, because of their considerable instability. It would be possible for two subjects whose central tendency responses are similar to respond, nonetheless, quite differently to other parts of the same attitude continuum when those parts are within the first one's range of acceptance but outside the second's. Similarly, two persons with rather different central tendencies may nonetheless respond alike to a certain contingency if their ranges of acceptance overlap.[14]

[11] H. Hyman *et al.*, *Interviewing in Social Research* (Chicago: University of Chicago Press, 1954).

[12] "Interaction between Attitude and the Definition of the Situation in Expression of Opinion," *American Sociological Review*, XVII (1952), 50–58.

[13] D. Goldberg, H. Sharp, and F. Freedman, "The Stability and Reliability of Expected Family Size Data," *Milbank Memorial Fund Quarterly*, Vol. XXXVII (October, 1959).

[14] If the full range of manifest attitude responses is retained in addition to measures of central tendency, serious general methodological problems are

CONCLUSION

Evidence presented in this study suggests that contextual factors, in this case the order in which items are presented in a general manifest anxiety scale, can significantly alter the distribution of scale types in a population. Subjects were deliberately made to appear more anxious or less anxious by the simple device of altering item order. This should increase our caution in interpreting the scale types associated with respondents since it raises the question of whether the ordinal measurement properties of the Guttman scale hold at all points on the scale.

A related point concerns the consequences of retaining the entire range of manifest attitude expression rather than using measures of their central tendency. As changes in context contribute to the production of the range, employing central tendency measures sacrifices the effects of context upon attitude expression which may be rather important for purposes of understanding the low correlations of attitude data and selected criterion variables.

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raised. First, there are immense difficulties in applying range-type data to traditional statistical methods based on point estimates of attitude. Since subjects may have narrow or wide ranges and their ranges may be discrete or overlap, they cannot easily be arranged in rank order. All statistical methods making use of central tendency measures, correlation, and analysis of variance would be inapplicable should one seriously hope to use the range of manifest attitude expressions.

The second methodological problem involved in using the full range of manifest attitude responses has to do with finding the limits to the range. A great deal more effort may have to go into ascertaining the range of attitude expression than has hitherto gone into making a point estimate of an attitude. Among the methods which could be used are generally those which permit multiple responses. Thurstone's equal-appearing interval scale allows a range to be assessed. Sherif and Hovland's method is explicitly designed to yield range descriptions of attitudes However, neither of these methods deals directly with the condition emphasized in this paper, namely, the effects of different contexts upon attitude expression and their role in producing the range. One approach would be to provide diverse contexts for the expression of an attitude in order to ascertain its variability. This method in effect attempts to see how contingent the attitude is upon circumstances. The questionnaire and interview are limited in the degree to which realistic contexts can be provided, and so the range limits may always be tentative and indefinite, especially where ordinal data are used.

Interviewing Political Elites in Cross-cultural Comparative Research

William H. Hunt, Wilder W. Crane, and John C. Wahlke

ABSTRACT

The experiences of researchers applying similar survey instruments in the study of legislators in three nation-states are reviewed. Problems of gaining access to respondents, the degree of co-operation encountered, respondents' candor, and interview location are compared. Cultural differences had distinguishable effects upon interviewers' success in handling these problems, but variation in the institutional contexts was of more importance. Prior knowledge of the respondent's institutional surroundings is of considerable aid to the interviewer. If interviewers are carefully selected and trained, cross-cultural comparative research among political elites utilizing standardized and structured survey instruments is both possible and profitable.

Despite increasing use of public opinion research in many corners of the world,[1] there remains a widespread belief that survey techniques are peculiarly appropriate to the American scene, particularly when the object of study is the public officeholder. Beyond the shores of North America, it is said, local habits and mores either incline people to resist interviewing or structure their thought in such a way as to preclude getting reliable data from them, or both. The utility of survey techniques for comparative research among political elites spanning cultural boundaries would therefore be severely limited. A comparison of the authors' individual experiences in three separate but related research projects leads to some **tentative** conclusions about this belief.

All three projects applied survey techniques to the study of institutionalized groups rather than to the study of a sample of a general population. The earliest of these, in which Wahlke was a participant, interviewed 474 of the 504 members who in 1957 constituted the legislatures of California, New Jersey, Ohio, and Tennessee.[2] Hunt's study involved interviews in 1960 with a stratified random sample of seventy-six members of the French National Assembly, drawn from three parties (U.N.R., M.R.P., and S.F.I.O.), and seventy-four party federal secretaries corresponding, where possible, to the departments of the deputies selected.[3] Crane sought interviews during the summer of 1960 with all fifty-six members of the unicameral legislature of Lower Austria.[4]

[1] See, e.g., G. Katona, "Survey Research in Germany," *Public Opinion Quarterly*, XVII (1954), 471-80; W. Phillips Davison (ed.), "Twenty Years of Public Opinion Research," *Public Opinion Quarterly*, Vol. XXI, No. 1 (Spring, 1957), entire issue; and the following articles in *Public Opinion Quarterly*, Vol. XXII (1958): E. C. Wilson, "Problems of Survey Research in Modernizing Areas"; L. and S. H. Rudolph, "Surveys in India: Field Experience in Madras State"; and M. Ralis *et al.*, "Applicability of Survey Techniques in Northern India."

[2] John C. Wahlke, Heinz Eulau, William Buchanan, and Leroy C. Ferguson, *The Legislative System: Explorations in Legislative Behavior* (New York: John Wiley & Sons, 1962). The study was supported by individual grants to the four authors from the Social Science Research Council, which also gave some funds to the group collectively for certain common expenses. In addition, Wahlke was aided by the Institute for Research in the Social Sciences of Vanderbilt University.

[3] The French study, tentatively entitled "Effects of Legislative Roles upon Party Cohesion," was supported by a Research Training Fellowship from the Social Science Research Council. In addition, financial assistance during the analysis stage was received from Funds for Small Grants To Support Research in Public Affairs provided to Vanderbilt University and Wabash College by the Ford Foundation. The present article deals only with the interviews with legislators and excludes those with federal secretaries.

[4] The Austrian study was aided by a grant from Vanderbilt University's Institute for Research in

The American State Legislative Research Project investigated the role concepts of legislators. The French study utilized some questions drawn from this project, but used others too, including some from Gross, Mason, and MacEachern's study of the school superintendency role in the United States.[5] Austrian interviews originally followed a direct translation of the American state legislatures schedule; after interviews were begun, several questions were added to elicit further information on legislators' errand-running functions. The three studies provide an admirable basis for comparison, therefore, since they dealt with a common subject matter, interviewed analogous populations, and utilized instruments which were in two cases practically identical and in the third case substantially parallel.

THE PROBLEM OF ACCESS

The problem of access is often stressed in non-American contexts and in this country as well. Experience in these three projects suggests that degree of access and difficulties encountered were a function of institutional rather than cultural factors.

The initial step toward gaining access in all three cases was a letter describing the project and soliciting the co-operation of each prospective respondent. In Austria, this step alone was an effective means of access in almost all cases. In France, more than three hundred letters were sent to prospective respondents. Yet it still required the utmost ingenuity and effort to track down a portion of those sought and to pin them down to an interview once they were found. Letters and follow-up visits to the Palais Bourbon, where deputies may be paged, proved to be the most essential access tools. Ultimately, the process of gaining access in a few cases involved the interviewer's loitering in the vicinity of the Palais Bourbon in hopes of buttonholing respondents.

Experience among American legislators more nearly resembled that in France than that in Austria, although there was considerable variation among the four American states. In two states, the researchers were introduced by the speaker of the chamber either by the reading of a letter (Ohio house) or directly in person (Tennessee senate and house). The "web" technique was principally used in California, New Jersey, and in the small Ohio senate, the researchers relying upon informal interpersonal contacts between themselves and prospective respondents and among the respondents interviewed and their friends as means of gaining access. In Ohio and Tennessee, it was common practice for interviewers to "work the floor" in search of respondents, since it was there that they were most available.

The vital importance of familiarity with the institutional context of interviewing is suggested by the difference between the simplicity and ease of access to less important Austrian state legislators and the difficulty and complexity of access to busier French national legislators, as well as by the variety of techniques needed to gain access in the six cases (four American, two European). The differences in experience here clearly reflect institutional more than cultural differences. Much of the French and American difficulty, for example, is traceable to simple lack of office and communication facilities for legislators: most deputies in Paris can be reached only by mail addressed to the Palais Bourbon; of the Americans, only California legislators had genuine offices, with secretaries to help fix appointments for them.

Experience in both America and Europe was consistent with Robinson's conclusion about interviewing American congressmen: there is greater resistance to granting an

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the Social Sciences. Reports on this study are Wilder Crane, *The Legislature of Lower Austria* (London: Hansard, 1961) and "The Errand-running Function of Austrian Legislators," *Parliamentary Affairs*, Spring, 1962.

[5] Neal Gross, Ward S. Mason, and Alexander W. MacEachern, *Explorations in Role Analysis: Studies of the School Superintendency Role* (New York: John Wiley & Sons, 1958).

appointment than to giving a satisfactory interview.[6] In a number of cases where American legislators agreed somewhat reluctantly or impatiently to grant a few minutes of their time, they rapidly became so interested in the process of answering the questions that it was difficult to terminate the interview. Similarly in France, the common warning that a respondent could talk for "just a quarter of an hour" was usually forgotten once the interview was under way.

In Austria there were striking and instructive differences in the *initial* co-operativeness of the respondents. Socialists were, without exception, immediately prepared to co-operate in answering questions. Business and worker representatives of the Peoples party were also generally co-operative from the beginning, but the fifteen farmer representatives of the Peoples party were almost all *initially* suspicious, the two refusals being from this faction. Once the formal part of the interview was finished, however, and the interviewer's pen put aside, most of those in this faction insisted on continuing the discussion for some time. The pattern came to be that those who were initially most reluctant were those whom it was most difficult to leave.

Contrary to the Austrian experience, French Socialists were the least accessible of the three party groups, although Socialists and M.R.P. deputies seemed to be the most co-operative once the interviews were under way. American interviewers reported no systematic differences between parties with respect to ease of access. The tentative conclusion in all three cases is that subcultural differences in basic attitudes and outlook make for greater differences in access within each system than broader cultural differences among the various systems of the Western world. In any case, it is important to emphasize that relative ease or difficulty of access was by no means predictive of a respondent's ultimate co-operation or frankness in the course of the interview.

The problem of access was successfully met in all three instances, as shown by Table 1. Only three legislators in California, three in Ohio, and two in Tennessee refused outright to be interviewed; all legislators in New Jersey were interviewed. One each of the California, Ohio, and Tennessee refusers demonstrated hostility to academic research; none of the French or Austrian refusers did. The Austrian speaker explained his refusal to be interviewed as due to his belief that expressing opinions was contrary to his role as an impartial presiding officer. The figure for French non-interviews is based on the *original* sample drawn for French deputies. Substitutes were interviewed in ten of the fourteen cases; subsequent tables in this paper report on seventy-five deputies who were interviewed, including substitutes, and three deputies included in the sample of federal secretaries.[7]

TABLE 1

INTERVIEWS AND FAILURES TO INTERVIEW

	United States (N = 504)		France (N = 76)		Austria (N = 56)	
	No.	Per Cent	No.	Per Cent	No.	Per Cent
Refused outright	8		3		2	
Evaded, without clear refusal	14		7		0	
Died or hospitalized	2		2		1	
Contact failure	6		2		0	
Total not interviewed	30	6	14	18	3	(5)
Total interviewed	474	94	62	82	53	(95)

[6] James A. Robinson, "Survey Interviewing among Members of Congress," *Public Opinion Quarterly*, XXIV (1960), 137. Another recent article on interviewing American congressmen is Charles O. Jones, "Notes on Interviewing Members of the House of Representatives," *Public Opinion Quarterly*, XXIV (1960), 404-6.

[7] Hunt is confident that, had two more months been available for interviewing, the number of deputies interviewed from the original sample would have been increased considerably. Pressed for time, he simply used substitutes to provide his stratified sample of the three parties.

CONDUCT OF INTERVIEWS

INTERVIEW STAFF

The possible distortion of interview results occasioned by particular characteristics of the interview staff is a familiar problem. In America it was thought the problem might arise from differences in status between interviewer and respondent, particularly between student interviewers and legislators. As the professional status of the junior staff was questioned in only a few instances, the American researchers concluded that on the whole both senior and junior interviewers faced the same difficulties, and that the respective advantages of one over the other were offset by corresponding difficulties.

In Europe, the crucial question concerning interview staff was nationality. All French interviewing was done by one American, while in Austria, thirty-five interviews were conducted by an American, fourteen by an American and an Austrian together, and four by an Austrian alone.

American nationality was a definite advantage in gaining access in both European countries. A request from an American aroused curiosity and the feeling that one must be cordial to foreigners, as illustrated by the comment of one French deputy: "If you had not been an American, I would not have received you. We are so busy, but, in my opinion, one should do everything possible to solidify links between Americans and Frenchmen, and particularly I am happy to receive a *young* American." Despite the fact that Crane's Austrian assistant was a junior judge in a Viennese lower court and thus had the advantages of an academic title and judicial non-partisanship, his presence, whether alone or in the company of an American, sometimes created problems by arousing the suspicions of his countrymen.

While American nationality may facilitate interviewers' gaining access in Europe, it is uncertain whether interviewers of different nationalities will obtain similar results. The fact that the interviewer is a foreigner may actuate perceptions that he is an impartial researcher who will protect respondents' anonymity. On the other hand, respondents may slur over esoteric details because they believe the foreigner will not understand them. In both France and Austria some respondents tended to instruct the interviewer about general and obvious aspects of their countries' politics and to waste time by chastising American foreign or racial policies. When Crane's Austrian assistant was present, the time wasted in lectures on elementary Austrian politics was reduced, but respondents' diatribes on American policies, directed at the foreign member of the interviewing team, were not eliminated. With regard to the candor of respondents, it is Hunt's impression that deputies spoke more freely and candidly to him than they would have to a French interviewer. Crane, however, found no clear pattern of differences between interviews that involved an Austrian assistant and those that did not. Some of the most candid interviews were obtained in both circumstances.

Viewed together, experiences in the three projects suggest that more flexibility in choosing interview staffs is possible than is generally believed. Interviewing difficulties vary with the different kinds of status relationships between interviewer and respondent, but the general magnitude of such problems appears to be no greater in Europe than in America. Regardless of locale and of the particular status difference, the proper instruction of interviewers would go far toward eliminating such problems before they arise.

LOCATION OF INTERVIEWS

In all three projects, the place of interview was a function of the institutional structure and practices of the legislature under examination. Table 2 shows the differences among the three countries but does not reveal the corresponding variation among the four American states. Thus, in geographically small New Jersey, where the legislature meets only one day a week,

it was necessary and possible to conduct most interviews in respondents' homes or offices. Ohio's large legislature and greater traveling distances demanded the completion of most interviews within the legislative session and in the legislative chambers. This was feasible due to Ohio's comparatively long session, but Tennessee's short legislative session (eleven weeks) made it necessary to secure about half the interviews by extensive and sometimes difficult travel. The geography of France would have created similar difficulties, but the practices of the Assembly allowed most interviews to be conducted in Paris. The adjournment of the Lower Austrian Legislature in early summer, however, required that Crane travel to about two-thirds of his respondents' homes or offices.

Privacy and freedom from interruption were available in varying degrees at each interview site. With due allowance for extreme variations in particular cases, the consensus of interviewers in both America and Europe is that interviews conducted in homes and offices were more satisfactory than those conducted on the floor or in the lounges of the capitol. There was, in general, little cultural variation in the problems of interview site.

RESPONDENTS' CO-OPERATIVENESS

An initial indication of the generally high level of co-operation in all three projects is the amount of time legislators were willing to devote to the interviews. The average interview required approximately 1½ hours, the shortest lasting ½ hour and the longest about 5 hours. French interviews varied in length between ½ hour and 3½ hours, with a median of 1½ hour, while Austrian interviews ranged from 1 to 3 hours, the average being 1½ hours. With interviews of such length it was sometimes necessary to meet respondents several times in order to obtain complete results. That substantial proportions of French and American legislators were willing to submit to second and even third rounds of intensive interviewing is an additional indication of respondents' co-operation in these two nations.

A more direct measure of respondents' co-operativeness is provided by the interviewers' rating of each respondent, entered on the interview schedule immediately upon completion of the interview. Table 3 shows that remarkably few respondents in either Europe or America were judged unco-operative. The measure is, of course, a highly subjective one. The critical distinction is that between "co-operative" and "not very co-operative," since all interviewers used as their standard here the adequacy of responses for the purposes of

TABLE 2

PLACE OF INTERVIEWS

	Per Cent		
	United States (N=474)	France (N=75)	Austria (N=53)
Capitol building	62	76	32
Respondent's place of business	20	2.5	47
Respondent's home	7	8	17
Hotels, restaurants	9	11	4
Miscellaneous other places, or combinations of the above	2	2.5	0
Total	100	100	100

TABLE 3

INTERVIEWERS' RATINGS OF RESPONDENTS' CO-OPERATIVENESS

	Per Cent		
	United States (N=474)	France (N=75)	Austria (N=53)
Very co-operative	45	64	36
Co-operative	45	23	49
Not very co-operative	8	9	13
Very unco-operative	1	4	2
Not recorded	1	0	0
Total	100	100	100

the project. The three groups are very similar in the proportions which were rated something less than co-operative. The distinction between "co-operative" and "very co-operative" is more difficult to draw. The relatively high proportion of French respondents rated "very co-operative" may reflect the use of thirteen substitutes in the French sample as contrasted with the use in the American and Austrian projects of the total population. However, the researcher in France was generally impressed by the efforts of French legislators to keep appointments despite their busy schedules. Some deputies returned to Paris a day early after the week end, while others did not go home on a week end, in order to co-operate with the interviewer.

TABLE 4

INTERVIEWERS' RATINGS OF RESPONDENTS' FRANKNESS

	Per Cent		
	United States (N=474)	France (N=75)	Austria (N=53)
Very frank	33	59	34
Frank	52	25	55
Not very frank	12	9	11
Very evasive	2	7	0
Not recorded	1	0	0
Total	100	100	100

Although Robinson has concluded that the interview is likely to be "of little therapeutic value" to an American congressman,[8] many respondents in these three projects, particularly the French legislators, seemed to enjoy immensely the opportunity to vent their personal opinions and values. The authors feel that this may partially explain the high degree of co-operation exhibited in many cases.

RESPONDENTS' CANDOR

Interviewers' ratings of respondents' candor constitute perhaps an even more subjective measure than ratings of co-operation, but the results, summarized in Table 4, are interesting. Again one notes the generally similar proportions of respondents adjudged to be something less than frank, and again the French respondents rank somewhat higher than others. It is significant that more French than other respondents called special attention to their desire to speak frankly. One claimed that "if the party were to learn all that I've told you, they'd kick me out the back door." Other respondents thought their own candor was unique: "Now I think that my colleagues probably haven't been completely candid with you. In any case, I'm going to tell you what they wouldn't dare have told you."

The experience in Austria was quite similar, despite the interviewers' expectations of respondent reticence due to Austria's violent political history. Many Austrian legislators described their official roles in the Dollfuss-Schuschnigg dictatorship, others their Nazi affiliations. Many specifically maintained they had talked frankly and demanded repeated assurances that their replies would be treated confidentially. Like French respondents, a number believed they were unique in speaking candidly.

In all three projects, candor was revealed most clearly in the freedom with which respondents named not only colleagues they liked and respected but those they did not. Observation, as well as internal checks between replies of different respondents, suggests that even though they were sometimes poorly informed about some subjects they discussed, most of them tried to speak frankly and openly.

EUROPEAN RESPONDENTS' REACTIONS TO "BEHAVIORAL RESEARCH"

Although both the French and Austrian projects were successful in securing most of the interviews sought, European respondents' reactions to certain types of questions do reflect cultural patterns which pose problems for comparative behavioral re-

[8] *Op. cit.*, p. 133.

search. The most obvious instance of this is the general European suspicion of check-list or scale-item questions. For example, a question used in the American study which asked respondents to indicate their relative interest in international, national, state, and local politics had to be omitted in France because respondents refused to make such rankings. Only 60 per cent of all French respondents answered all five yes-no-type questions composing a scale of affect for interest groups. Almost none of the respondents refused to verbalize, usually at some length, on the *subject matter* of such questions, but a relatively high proportion could not be persuaded to answer in the customary and familiar form used in almost all American surveys. The Austrian experience was practically identical.

In both European countries there was some resistance to the very notion of using precoded questions to obtain data. There were complaints that such devices were "too brutal," that they provided no opportunity for considering "nuances," or that respondents had "personal positions" with regard to politics that would not fit into the categories provided. Many respondents insisted on verbalizing their answers in addition to marking the answer sheets and then, in their excitement, forgot to mark the appropriate columns. Securing answers under such circumstances was not easy, but it is Hunt's conclusion, in disagreement with Lerner, that such methods and questions can be used in France if sufficient attention is given to adjusting them to French conditions.[9] By patient listening and constant probing, the interviewer can succeed in obtaining codable data. The resistance described seems to be more to the "American gimmick" than to the actual process of answering the questions.

A comparison of Austrian and Californian experience with check-list questions provides strong evidence of the degree of cultural difference in this respect. When it became clear that asking these questions inordinately prolonged Austrian interviews, it was decided to leave them (with a stamped return envelope) for respondents to complete at their leisure. The mere thirty-three that were returned in no sense constitute a representative sample.[10] When the identical procedure was adopted in California, however, only 3 of 113 respondents failed to return the questionnaire.

Some of the resistance to the mechanics of check-list and scale-item-type questions can perhaps be traced to mere unfamiliarity with them. Europeans are not so accustomed as Americans to frequent public references to Gallup polls and consumer surveys. Neither are they accustomed in their schools to the use of multiple-choice examinations, which probably help condition Americans to these devices.

But most of the resistance probably runs much deeper than this. Among educated Europeans, it reflects a bias in favor of "humanistic" and against "quantitative" or "scientific" modes of thinking about social and human problems which is by no means foreign to American culture. The explanation offered by European respondents in resisting check-list and similar questions sounds much like many American complaints about modern "social science." Consider the following, written by an Austrian legislator:

> I regret that, after consideration of the questionnaire, I am not in a position to transmit

[9] Daniel Lerner, "Interviewing Frenchmen," *American Journal of Sociology*, LXII (1956), 187-94. Lerner describes how he was compelled to move from a "highly structured questionnaire" to a "minimally directive dialogue" with a "highly participant interviewer." Lerner notes that "this transformation of the schedule reduced the utility of precoding and other mechanical devices for assuring uniform reporting" (p. 194).

[10] The thirty-three respondents who did fill out the check lists were nineteen Socialists and fourteen members of the Peoples party. A further breakdown by faction of those co-operating by filling out the return parallels very closely the rankings of the various groups in terms of access (discussed earlier): Socialists, 76 per cent; Peoples Party Workers League, 62 per cent; Peoples Party Business League, 50 per cent; Peoples Party Farmers League, 33 per cent.

it in the form sought by you. I am certainly a person who enjoys making decisions with clear judgments in political questions. However, I cannot with a clear conscience answer with a short "yes" or "no," for every "yes" has its "however," and every "no" its "if." I would therefore have to give to each of your questions a limiting or expanding commentary. I am of the opinion that one could not begin anything at all with such statistics, which lead very often to false evaluations and mistaken conclusions.

Such attitudes complicate the job of the researcher. They make it extremely difficult to use in Europe certain standard instruments and measures which have been proved valid and reliable in American research. But it can hardly be emphasized too strongly that European resistance is more to the *form* of certain questions than to the basic assumptions of behavioral research. While such attitudes may preclude the use of some rigidly precoded questions, they do not prohibit the use of carefully structured questions which yield more than sufficient data to be coded in rigorously defined categories and subjected to quantitative analysis. The Austrian and French demonstration that it is possible to secure frank and full answers to intensive questioning is undoubtedly more significant than their demonstration of the inutility of particular devices which may often legitimately be labeled "gimmicks" by American researchers as well as by European respondents.

EQUIVALANCE AND COMPARABILITY OF DATA

One problem on which experience in the three projects can offer only the most impressionistic evidence concerns the possible distortion of concepts and ideas in translation from one language to another or the possible uniqueness of concepts and connotations in a given culture. In some apparently very simple cases where genuinely comparable findings were sought, language differences made it difficult to obtain clear conceptual equivalence. For example, it was not possible to find a French equivalent for "may or may not" as a check-list response.[11] Similarly, even native French persons asked to help in translating interview questions could think of no discreet way to ask respondents their religious views. After twenty-four interviews, however, a formula was found which was used successfully in all subsequent interviews and which elicited mail replies from twenty-one of the twenty-four not originally asked the question.[12] In Austria, the most difficult expression to translate was "rules of the game" (referring to unwritten norms of legislative behavior). After using an elaborate description of the concept in the first few interviews, it was found that the Austrian legislators had an obvious expression for it: "*ungeschriebene Gesetze*" ("unwritten laws"). The same expression was used in France ("*règles non-écrites*").

There were, of course, more complex problems than those of simple translation. For example, the French conceptions of "interest groups," "pressure groups," and "lobbies" have quite different connotations from the same American terms. Again, a French "political friend" is a very special kind of friend, with the result that a question dealing with legislators' friendship relations in the chamber was perhaps the most difficult and indiscreet of all questions asked in France. The question was omitted in Austria after the first few interviews.

The Austrian study encountered fewer

[11] The five categories of response to items dealing with deputy role definitions were (1) "Dois absolument" ("Absolutely must"); (2) "Devrais plutôt" ("Preferably should"); (3) "Pourrais ou pourrais ne pas" ("Might or might not"); (4) "Ne devrais plutôt pas" ("Preferably should not"); and (5) "Ne dois absolument pas" ("Absolutely must not").

[12] The question was finally asked as follows: "En ce qui concerne les idées religieuses, est-ce que vous vous considérez catholique, catholique pratiquant, ou autre chose?" ("Regarding religious ideas, do you consider yourself to be a Catholic, a churchgoing Catholic, or something else?")

problems of this kind, but it had some conspicuous failures which reflect the necessity for thorough knowledge of institutions as a precondition for effective behavioral research. Some questions (taken by simple translation from the American interview schedule) were totally irrelevant for Austrian legislators. For example, questions on legislative decision-making, such as how legislators make up their minds how to vote, are meaningless to legislators who have no "free votes" and who act almost entirely as decision-legitimizers. The few questions which served in America to construct a scale of legislators' ideology were all questions on which Austrian legislators were unanimous, suggesting that ideological cleavage, if any, is specific to political systems rather than global in character.

On the whole, the problem of translating concepts from one culture to another, whether at the simple level of transliteration or at a more complex conceptual level, was solved in the light of researchers' experience and understanding of the particular system and culture they were studying. This means that "operational equivalence" was perhaps less common than "conceptual equivalence" in those instances where direct comparability was explicity sought. It is the feeling of the researchers that conceptual equivalence presented a few difficult but not insurmountable problems. One would, of course, anticipate more difficulty on this score where cultural differences are greater than those among the three Western cases involved here.[13]

SUMMARY AND CONCLUSIONS

The mere fact that extensive interviews were completed in three diverse settings with comparable instruments and objectives is in itself worth reporting. Even the American researchers were occasionally faced with the reaction commonly encountered by Hunt and Crane and reported earlier by Lerner: "There was universal doubt about the value and the feasibility of the enterprise."[14] But in each case access was gained and intensive interviews were completed. Conducting the interviews presented no really new problems, either with respect to staff, to location, or to maintaining rapport.

More important, the interviews yielded data of more than satisfactory quality as well as sufficient quantity. The reluctance of European respondents to answer the more "gimmicky" type of question did not extend to answering questions in general, even highly structured ones. In France and Austria, as in America, the interviews rather easily provided data on political socialization and career lines. Few respondents were at all hesitant to tell when they first became interested in politics, what positions they had held, how they became legislators, or to describe clearly their family, social, and educational backgrounds. In all three cases, the interviews provided a wealth of data concerning legislators' perceptions, attitudes, and role concepts, although in a form less easy to analyze, perhaps, than the other data.

An often-overlooked advantage of the systematic interview which impressed researchers in all three projects is its capacity for providing direct, observational knowledge of the kinds of institutional factors which were once the sole concern of political scientists. Where many interviews are conducted at the scene of institutional activity, the interviewer becomes also an intimate and constant observer.

But there is a reverse side to this coin. Experience in all three projects strongly recommends some prior understanding of the institutional environment to be studied. Such knowledge, of course, facilitates comprehension of respondents' replies. But more important still, it enables the interviewer to ask intelligent probing questions and to assume the kind of interviewer role

[13] H. P. Phillips, "Problems of Translation and Meaning in Field Work," *Human Organization*, XVII (1960), 184-92.

[14] Lerner, *op. cit.*, p. 187

that seems most suited to the type of population studied in these three projects. The interview in these cases is most productive when it is a two-way conversation. It is not just that European politicians resist giving simple responses to an inanimate research instrument; American as well as European legislators seem to appreciate an intelligent and informed interviewer whom they can take into their confidence and with whom they can explore questions suggested by, but not included in, the interview schedule.

Nevertheless, those who interviewed in America and Europe are agreed that their most valuable asset was the rigidly structured and uniform interview schedule. Even where a question was inappropriate to a particular state or country, it was found that "no answer" responses were significant from a comparative point of view. Moreover, the structured questionnaire was an important rapport-building device in all three cases. Particularly in Europe it was found that placing personal and biographical questions at the beginning served to relax respondents and involve them in the interview. The fact that possibly indiscreet or inapplicable questions could be read from a standard form was also a valuable asset, since it tended to lead respondents to blame someone other than the interviewer for possible embarrassment and thus to increase their tolerance for the process of being interviewed.

Despite the problems described in this article, therefore, the authors must conclude that systematic, structured interviews are a useful tool for genuinely comparative behavioral research among European as well as American political elites.

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COMMENTARY AND DEBATES

Scapegoating in Baseball

Oscar Grusky's "Managerial Succession and Organizational Effectiveness" (*American Journal of Sociology*, July, 1963) makes use of an ingenious source of data for comparative organizational analysis. By picking major league baseball clubs for his focus, he has available to him twenty organizations which are identical in goals and in a wide variety of other structural characteristics. There is considerable potentiality here, but unfortunately Grusky's analysis offers us no more than the fact that those clubs which have done the worst over the years have changed their field managers most frequently.

For this rather obvious correlation Grusky suggests two possible explanations. He quickly disposes of the common-sense explanation of one-way causality: the manager is fired because the team performs poorly. Despite the fact that this explanation is consistent with the data he presents, he charges it with, among other things, the failure "to stimulate careful empirical test."

Such a charge is doubly puzzling. First of all, it is not at all clear why this particular explanation offers any less of a clear empirical test than the alternative offered. Second, one looks in vain for the careful empirical tests that Grusky's more complicated explanation has produced. At the very least, one might have expected the specification of how one might get data that would allow a choice. Faced with two explanations that apparently handle the available data equally well, we prefer the simpler one, Grusky's invocation of Ernest Nagel notwithstanding.

However, we wish to suggest that the common-sense explanation, Grusky's explanation, and a third one which we will suggest each has different implications and can be tested with little difficulty. A small amount of data toward such a test is offered below. As a prior step, we present the three explanations of the succession-effectiveness relationship and the different predictions which they make.

THREE EXPLANATIONS

1. *The common-sense one-way causality theory.*—This explanation fully accepts the fact that the field manager of a baseball team is a major influence on a team's performance. When the team is doing poorly, he is rightfully held responsible. Consequently, he is fired and replaced with an alternative manager who, it is hoped, will do better. A new manager typically will raise the performance of a team, since he can benefit by avoiding the errors that his predecessor made.

There is no reason, by this theory, to expect any diminution in a team's performance after a new manager has been hired. Instead, deteriorating performance leads to managerial succession which in turn leads to *increased* effectiveness of performance. A team which is fortunate enough to have acquired a good manager will perform effectively over a prolonged period and will have few managerial changes. A team that is not so fortunate in its choice of managers will have poorer performance and a high frequency of managerial succession.

2. *The Grusky two-way causality theory.*—Grusky also assumes that the field manager is a major influence on a team's performance. However, the relationship between effectiveness and succession, he argues, is reciprocal rather than one way. It is certainly true that a team performing

badly will frequently cause the manager to be fired. However, such managerial changes tend to have a number of interrelated and undesirable consequences. To quote Grusky:

> A managerial change inevitably upsets old patterns of behavior. New organizational policies . . . produce changes of great magnitude in the internal structure of the team. Members are forced to adapt not only to the successor's new way of doing things but also to the new informal coalitions that inevitably develop.
>
> Frequent managerial change can produce important dysfunctional consequences within the team by affecting style of supervision and disturbing the informal network of interpersonal relationships. . . . The resulting low primary group stability produces low morale and may thereby contribute to team ineffectiveness. Declining clientele support may encourage a greater decline in team morale and performance. The consequent continued drop in profitability induces pressures for further managerial changes. Such changes in turn produce additional disruptive effects on the organization and the vicious circle continues.

Clearly, a managerial change by the Grusky theory should produce a further deterioration in performance by an already faltering team.

3. *The ritual scapegoating no-way causality theory.*—Unlike the above theories, this one assumes that the effect of the field manager on team performance is relatively unimportant.[1] In the long run, the policies of the general manager and other front-office personnel are far more important. While judicious trades are helpful (here the field manager may be consulted but does not have the main responsibility), the production of talent through a well-organized scouting and farm system is the most important long-run determinant. The field manager, who is concerned with day-to-day tactical decisions, has minimal responsibility for such management functions.

In the short run, the supply of available baseball talent is the most important determinant of performance, that is, the players themselves. The manipulation of this talent by the field manager will make very little difference. The New York Yankees, while Casey Stengel was the manager, won ten pennants in twelve years. Is this the measure of Stengel's skill or do the New York Mets, who are winning no pennants, more accurately reflect it? We suggest that the Yankees would have done as well and the Mets would have (or more accurately, could have) done no worse.

Two qualifications are worth making before one dismisses the field-manager role out of hand. First of all, a truly talented manager such as Gene Mauch of the Phillies may make a long-term contribution through his skilful development of the potentialities of young players. This may not be reflected in immediate change in team performance.

A second qualification acknowledges some over-all effect by field managers but assumes that the *variance* in skill between those who become field managers is so small that managerial skill may be considered a constant. It is worth noting, in this respect, that few "new" managers make their appearance when managerial shifts are made. Instead, there is a pool of *former* managers, frequently employed as coaches by various teams, who are usually called upon when changes are to be made. Most of these coaches were fired from their positions as managers, presumably because they failed to produce winning teams. Such recruiting practices strongly suggest the interchangeability of managers and the improbability of explaining variance in team performance by anything the field manager does.

If the field manager makes little difference, how does one account for the high correlation which Grusky reports between team performance and rate of managerial succession? The answer is quite simple: The firing of the field manager is a classic example of ritual scapegoating. It is a convenient, anxiety-reducing act which the

[1] Grusky suggests this possibility on p. 30, n. 23, but he does not pursue its implications.

participants in the ceremony regard as a way of improving performance, even though (as some participants may themselves admit in less stressful moments) real improvement can come only through long-range organizational decisions.

Those involved have a strong stake in maintaining the myth of managerial responsibility. If the field manager himself denies responsibility for the team's failures, then his claim for responsibility when the team is successful is weakened. As for the front office and the players, it is a happy excuse for what is really their own responsibility. Finally, as Grusky notes, the fans of a poorly performing team can be appeased and their hopes of future success rekindled by this simple ritual act.

Note that if we were simply to compare the performance of the team after the manager's dismissal with the performance before we would surely find some immediate improvement. Such a result is an artifact of the conditions that produce the ritual, namely, a slump in performance. If we compared average rainfall in the month preceding and the month following the performance of the Hopi rain dance, we would find more rain in the period after. The dance is not performed unless there is a drought, so such a comparison would be misleading. Nevertheless, this "slump-ending" effect may help to account for the tenacity of belief in the effectiveness of the ritual.

Our prediction would be that if the slump-ending effect is controlled, there will be no difference in performance under the old manager and the new manager. However, teams which perform poorly over a prolonged period will have more frequent occasion to resort to the act of ritual scapegoating; hence the correlation between team effectiveness and rate of succession among field managers.

A TEST OF THE THEORIES

Managerial successions take place at particular moments in time. Those that occur in the middle of the season are particularly appropriate for analysis for two reasons: (1) mid-season changes tend to maximize disruptive effects and thus should produce a fairer test for Grusky's theory; and (2) there is less opportunity for influences other than the field manager's influence to operate. In the gap between one season and the next, trades, the aging and retirement of present players, and the development of players from the farm system may serve to alter the available talent with which the new manager has to work

If we compare the performance of a team during one season in a period prior to a managerial change and in the period following the change, then we get a different prediction from each of the above theories. (1) By common sense, we should expect an increase in team effectivenesses. (2) By Grusky, we should expect a decrease in team effectiveness due to the disruption of succession. (3) By the scapegoat theory, we should expect no change (after controlling for the slump-ending effect).

We shall present data on twenty-two mid-season managerial changes from 1954 to 1961.[2] Team won-lost record was recorded at four points in time: (1) as of approximately two weeks before dismissal; (2) as of the day of dismissal; (3) as of approximately two weeks after dismissal; and (4) as of the end of the season (Table 1).

There are a number of comparisons of interest. First of all, it is quite clear that dismissals take place in periods of declining performance. Only four of the twenty-two teams had a higher percentage at the time of dismissal than in the previous time period. There is some indication of immediate improvement, as fifteen of the twenty-two teams show better performance in the next two weeks. In only one case, however, does a slumping team recover under the new manager to a point where the team's won-lost percentage is higher two weeks after dismissal than it was two weeks before. Such data seem to document the

[2] We are indebted to Andréa Modigliani for compiling the data reported here.

existence of a slump-ending effect but are equally consistent with all of the theories.

The most relevant test of the theories is the comparison between won-lost record up to two weeks prior to the dismissal of the old manager and won-lost record for the remainder of the season under the new manager. Thus, the two weeks preceding the dismissal of the manager are removed from the performance record to control the slump effect.

In thirteen of the twenty-two cases, the team performs better under the new manager than it had under the old manager up to two weeks prior to his dismissal. This is unfortunately not a very definitive result for choosing between the common-sense and the scapegoating prediction, but it clearly goes against the Grusky prediction. If managerial succession disrupts the primary group relations of ball players, it apparently does not lead to any visible deterioration of performance.

TABLE 1

TEAM PERFORMANCE AFTER CHANGING MANAGER, 1954–61

Comparison	Improvement	Deterioration	N
Two weeks prior versus time of dismissal	4	18	22
Time of dismissal versus two weeks later	15	7	22
Two weeks prior to dismissal versus two weeks after dismissal	1	17	18*
Two weeks before dismissal versus post-dismissal record	13	9	22
Two weeks before dismissal versus record from two weeks after dismissal until end of season	12	10	22

* This comparison refers only to those eighteen teams whose performance was declining at the time of dismissal

Perhaps there is some slight improvement which can be attributed to the ritual itself. The new manager may have no effect through anything he does, but the players' belief in the efficacy of the ritual boosts their morale and brings about a short-run improvement. To test this, we can compare performance up until two weeks before dismissal with performance for the remainder of the season starting two weeks after dismissal. In this comparison, both the periods immediately preceding and immediately following are controlled. Here there is even less evidence of effect: in twelve cases, the team does better under the new manager, in ten cases it does worse.

CONCLUSION

The modest amount of data presented here warrants no firm conclusion. However, Grusky's cavalier dismissal of the simple common-sense explanation seems unwarranted. We do not know if it is as "scientific" as Grusky's more elaborate theory, but it proves the better predictor here. However, it still remains to be established that the field manager has any effect on team performance. Until it is, we prefer the scapegoating explanation of the correlation between effectiveness and rate of managerial succession.

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Reply

Professors Gamson and Scotch present data on twenty-two mid-season managerial changes from 1954 to 1961. All of their comparisons required team won-lost record as of "approximately" two weeks before or after the manager's dismissal. In our replication of their study we found twenty-three cases that qualified when we defined

two weeks to mean exactly fourteen days.[1] We excluded two cases, the 1954 White Sox and the 1958 Cardinals, where a full fourteen days did not follow the dismissal of the manager. In each case the new manager took over eleven days before the season ended. If approximately two weeks were to be defined so as to include these two, the case total would have been twenty-five. We also found evidence of a number of other mid-season managerial changes of considerable relevance to the analysis of the effects of managerial succession.[2]

Gamson and Scotch have indicated that in their opinion the critical test of the three theories is the won-lost record up to two weeks prior to the old manager's dismissal compared with the new manager's won-lost record for the remainder of the season.[3] This comparison allows a two-week control for the slump effect. Table 1 presents a comparison of the Gamson-Scotch study and our replication. In fourteen of twenty-three cases the team shows an improvement. These data and the last comparison in Table 1 seem to me to be most supportive of the common-sense theory and inconsistent with the other two explanations.

However, the theory described in my study did not claim that succession would be equally disruptive under all conditions. In selecting mid-season changes Gamson and Scotch asserted that "mid-season changes tend to maximize disruptive effects and thus produce a fairer test for Grusky's theory." Actually, quite the opposite was the case with the data they presented. As a number of studies have shown, an important factor affecting degree of disruption is

TABLE 1

REPLICATION OF GAMSON-SCOTCH STUDY OF TEAM PERFORMANCE AFTER CHANGING MANAGER, 1954-61

Two Weeks before Dismissal versus Post-dismissal Record	Gamson-Scotch	Replication		
		"Inside"	"Outside"	Total
Improvement	13	7	7	14
Deterioration	9	2	7	9
Total	22	9	14	23

[1] I have learned through sad experience that it is highly desirable to have two coders independently code and tabulate data of this kind. This was accomplished by Daniel Willick and Don Baker, whose help I greatly appreciate. Their results were compared, and disagreements were rechecked and resolved. The sources were the sports sections of the *Los Angeles Times* and the yearly *Baseball Guide and Record Book*, compiled by J. G. Taylor Spink in collaboration with Paul A. Rickart *et al.* (St. Louis: Sporting News Publishing Co.). The use of the facilities of the Helms Athletic Foundation is gratefully acknowledged. I am also grateful to Lindsey Churchill for his advice on the manuscript.

[2] The definition of what constitutes a managerial succession was not without problems in the study. Actually, more than twenty-five successions turned up in the replication. In 1959 the Cleveland Indians fired their manager on September 22 only to rehire him the next day. The manager of the 1960 Phillies quit on April 12, after only one game of the season. The manager of the 1961 Minnesota Twins was relieved temporarily by a coach. He returned as manager only to be dismissed ten days later. We used the date on which he was actually fired and not the date he was temporarily relieved. Also, the 1960 Red Sox had an interim manager, a coach, for five days before a new manager took over; and the 1959 Red Sox had an interim manager, also a coach, for only a few hours. There may very well have been other cases of interim managers that we missed. These cases involving interim managers and the Minnesota Twins' case point up the inadequacies of taking the won-lost record two weeks before dismissal as an index. Anticipation of replacement may and probably does occur before this. Perhaps it would be more advisable to select as base a month before dismissal or, as we decided for Table 2, the preceding season.

[3] The replication revealed numerous other errors. Our findings for the other four comparisons were as follows.

Comparison	Improvement	Deterioration	Total
Two weeks prior vs. time of dismissal	4	19	23
Time of dismissal vs. two weeks later	16	7	23
Two weeks prior to dismissal vs. two weeks after dismissal	6	13	19
Two weeks before dismissal vs. two week post-dismissal record	15	8	23

whether or not the successor is recruited from within the present staff or from outside the organization.[4] Inside successors tend to be less disruptive than outside successors, and mid-season successions in baseball frequently tend to involve inside replacements. In fact, nine of the twenty-three managerial replacements were so close as to have had daily contact with the playing personnel.[5] Eight coaches and one player who became managers of their own teams were designated "inside" successors, and the remaining fourteen were designated "outsiders."

The Gamson-Scotch theory would suggest that since the manager has essentially no effect on team performance it really does not matter whether or not the successor is an insider or an outsider. The common-sense theory presented also did not take this factor into consideration. (It could be argued that inside managers are aware of the predecessor's errors and hence more likely to avoid repeating them.) On the other hand, our theory predicts that inside successions should be less disruptive than outside successions.[6] Let us consider the comparison considered most important by Gamson and Scotch. We find, as shown in Table 1, that the team improved in seven of nine cases involving an inside succession, and deteriorated in seven of fourteen outside successions. These data suggest (1) that type of succession does appear to make a difference, contrary to the Gamson and Scotch theory, (2) that inside succession is associated with team improvement relative to two weeks before dismissal, and (3) that outside succession is associated

TABLE 2

TEAM PERFORMANCE AFTER CHANGING MANAGER, USING OLD MANAGER'S PREVIOUS YEAR'S RECORD AS BASE, 1954–61

Comparison	Improvement			About Same*			Deterioration			Total
	"Inside"	"Outside"	Total	"Inside"	"Outside"	Total	"Inside"	"Outside"	Total	
Old manager's previous year's record vs. new manager's record	7	2	9	1	1	2	0	8	8	19†
Old manager's previous year's record vs. new manager's record from two weeks after dismissal to end of season	5	1	6	2	0	2	1	10	11	19

* Defined as a difference of .005 or less.

† In four cases the old manager was not with the same club the previous year.

[4] See, e.g., A. Gouldner, *Patterns of Industrial Bureaucracy* (Glencoe, Ill.: Free Press, 1954), and R. O. Carlson, *Executive Succession and Organizational Change* (Chicago: Midwest Administrative Center, University of Chicago, 1962).

[5] As indicated in an earlier study, inside-outside is often a relative matter. The nine inside managers were distinguished because they seemed to have the closest association with the players. Many of the remaining fourteen had considerable though less extensive contact. Three were farm club managers for the same team; four were in the team's front office; one was a broadcaster of the team's games; two were coaches on different clubs; and two were managers of other clubs. Only two were not associated with baseball at the time they were hired.

[6] The "our" is not merely editorial. Gouldner, for example, made the same prediction ten years ago (*op. cit.*, p. 72).

with no deterioration in team performance, contrary to our theory.[7]

Although it is certain that some control for the low effectiveness of the team prior to succession is essential, one might question whether or not a two-week control for the slump effect is adequate. It seems unfair to the predecessor to take as a measure of the team's performance under his leadership the won-lost record for the season up until two weeks before he is replaced. The limitations of this technique can be illustrated by the case of the 1959 Cleveland Indians, an instance that qualified for inclusion in the Gamson-Scotch study. On May 2, after losing two of sixteen games played, the manager was fired. Two weeks prior to that date the team had played only six games and lost them all. Now if one inherits a ball club that has a team percentage of .125 there is almost no direction to go but up. And if one compares the Indians' performance two weeks before the dismissal of the manager with the post-dismissal record (Gamson and Scotch call this the critical test of the theories), then the new manager needed to win only a single game for the remainder of the season in order to show improvement. For this reason we thought that another and perhaps fairer test of the theories would be to compare team performance under the old manager in the year previous to his succession with the record of the new manager. (In sixteen of the nineteen cases that qualified, the manager was with the club for the entire year. In the other three cases he was with the club for most of the year, and the won-lost record while he was with the club was used.) Two comparisons were made. The first was identical to the Gamson-Scotch critical test except that it used the predecessor's performance as manager in the previous year as base. The second allowed the successor a two-week period of grace to get used to his new job and may therefore be more appropriate. These comparisons ask the question: Did the team perform better under the new manager than it had in the previous year (with essentially the same player material) under his predecessor? As shown in Table 2, the first comparison reveals that improvement in team performance takes place in about the same number of cases as does deterioration. This supports the Gamson-Scotch argument until one examines the difference between inside and outside successors. Seven of nine teams showing improvement involved replacements by inside managers and eight of eight of those showing deterioration involved replacements by outside managers. Even more compelling evidence for our theory was revealed by the second comparison which gives the players a two-week period to adapt to the new manager and the new man the same amount of time to learn the requirements of his new position before evaluating his performance. We find that deterioration in team performance takes place in almost twice as many cases as does improvement. In only six of nineteen cases did the team do better under the new manager than it had in the previous year under his predecessor. Clearly, this goes against the Gamson-Scotch and the common-sense explanation. Again, the most pronounced findings of the entire study were revealed when inside and outside successors were compared. Five or six managers who showed improvement were inside successors and ten of eleven of those whose

[7] There are some problems with the Gamson-Scotch data that may be noted here: (1) The timing of the managerial succession can have a significant impact on the Gamson-Scotch measure of team performance. A single victory or loss affects the over-all won-lost record greatly at the start of the baseball season and considerably less at the end of the season. To evaluate team performance properly requires some control for this statistical artifact. Seven of the cases that qualified for inclusion in the Gamson-Scotch study would be markedly affected by this factor. Four successions took place at the start of the season, in April and May, and three occurred near the end, that is, in mid-August or later. (2) The use of won-lost record also has some limitations. For example, a minute percentage difference may produce a classification of team performance as improvement or deterioration. It may very well be that team standing is a more useful measure as this index takes into account the relative performance of the team.

teams deteriorated were outsiders. These data support our theory.

Despite this evidence there is merit in the explanation in cultural terms offered by Gamson and Scotch which stresses the myth of managerial responsibility. Their theory and the methods they have devised for testing it are highly provocative. However, it is one thing to assert that such a myth of managerial effectiveness exists and that the manager's reputation is tied closely to the fate of the system which he may not always clearly affect, and quite another matter to claim, as Gamson and Scotch do, that the manager's behavior can have *no* influence at all on organizational effectiveness except in the long run. Such a position unwarrantedly de-emphasizes the potential importance of social organizational factors for effectiveness and, in so doing, fails to explain the findings of numerous studies which reveal an opposite set of conclusions.[8]

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[8] See esp. R. Likert, *New Patterns of Management* (New York: McGraw-Hill Book Company, 1961), and P. Blau and W. R. Scott, *Formal Organizations* (San Francisco: Chandler Publishing Co., 1962). The problem of assessing the relative importance of the manager's contribution to team performance is an important one. Daniel Willick, a research assistant, has devised a rough but ingenious measure for this purpose. He compares each team's final yearly standing with its rank on three "abilities" measures: team batting, team fielding, and pitchers' earned-run averages. He then assumes that teams which are ranked about the same on all four indexes are being managed at a level consistent with their skills. Those teams which have a higher team standing than their rank on the other indexes are considered to be well managed and those with a much lower team standing than rank on the "abilities" indexes are considered poorly managed.

Seeman's "Alienation and Social Learning in a Reformatory"

Two Reactions

Implications of the Findings on Alienation

A recent paper by Melvin Seeman in *AJS*, LVI (November, 1963), 270–84, appears to me relevant to a puzzling question that exists with respect to the American Negro's assimilation into modern urban life. Every immigrant group to American cities has had difficulties of assimilation into urban society, but the difficulties of American Negroes seem far greater than those of any other group. The easy explanation is the prejudice and discrimination among whites erect color barriers which cannot be easily broken down, at least through individual mobility (which has always been the mechanism by which immigrants have achieved a place in American society). Certainly such prejudice and discrimination exist, and certainly they are partly responsible for these difficulties.

Yet there are a number of facts which are not accounted for by this explanation. A single example is the growth of new forms of welfare, such as aid to dependent children, very largely concentrated among Negro mothers. This is a form of welfare which, so far as I am aware, exists in no other society to the degree that it exists in America—and only grew in America with the massive migration of rural southern Negroes into American cities. Another example is provided by the comparison of Puerto Ricans and Negroes, particularly in New York. In many respects, Puerto Ricans have leapfrogged over Negroes who were earlier immigrants to the city.

A more adequate explanation frequently offered points to the lack of a strong family in the rural South and the fragmentation of the Negro family in plantation life. This means, so the explanation goes, that

the principal socialization device in society is largely missing for Negro children. The evidence indicates that this factor is partly responsible for the difficulty of Negro accommodation to urban life, but I would like to propose an additional element suggested by Seeman's paper. I will indicate, first, what I see to be the special condition of the urban American Negro who has recently immigrated from the rural South and then indicate the relevance of Seeman's research.

Perhaps the essential characteristic of the individual's relation to society in industrial urban centers is individualism. In order to survive in a modern city, an individual is thrust sharply on his own resources. The frequent changes of job, residence, and other aspects of daily life, together with the reduction of family and neighborhood supports, require an extreme degree of self-sufficiency. Even knowing how to obtain aid from the confusion of private and public welfare agencies requires a considerable amount of skill.

The social structure of the farm or plantation from which a rural southern Negro emigrated, in sharp contrast, can best be described as modern feudalism. The Negro family was totally incorporated into the farm, and the defining characteristic of his relation to the farm owner was paternalism. Sometimes this paternalism was despotic, sometimes benevolent. But in either case, the Negro's total relation to society was mediated through the farm owner, and he was totally dependent on him. His consumption as well as his production were largely through the farm owner—at periodic intervals he would receive his disbursement of staples, and the only monetary wages over which he had control were for small pleasures and "luxuries." The major element that was alike under the most benevolent and the most despotic owners was this total lack of authority over his destiny and a corresponding lack of responsibility for his daily activities. He had little or no control over his environment. His condition may not have been good, but it was certain, and he need have no concern for his future, for he could not affect it.

This social structure, which is the direct opposite of that in a modern city, I suggest was not wholly paralleled by any from which previous immigrant groups to American cities had come. And it is this social structure which makes Seeman's paper relevant to the condition of the recently urban American Negro. Seeman found that a reformatory inmate's sense of control or power over his environment affected the learning of information relevant to his prison career. Those who had the greatest sense of powerlessness learned least; those who objectively were in a position to affect their career, and those who felt they could do so, learned most. Seeman says, in summarizing his results, "The individual's expectancies for control govern his subsequent attention and acquisition" (p. 283). Seeman's results suggest a general phenomenon: that a man is sensitive to the cues of his environment only when he believes he can have some effect upon it. He will learn only when such learning can benefit him—either in formal learning situations or in the casual cues of his environment.

The point, then, is that in the social structure which has been characteristic of rural southern Negroes learning in areas connected with responsibility for one's destiny was wholly useless. If Seeman's results can be generalized to this situation and to a permanent "set" established in the individual, they are of direct relevance. One could even suggest a further generalization: that such a set toward insensitivity to cues required for self-responsibility requires more than a single generation to die out, being transmitted by those who hold it to their children.

These ideas suggested by Seeman's result could well be tested, for they imply great differentiation in the ability of urban immigrant American Negroes to accommodate to the city. For some Negroes migrate to the city from very different conditions than those described above: from [illegible]

dependent farms, from small towns, from other cities. They also imply great differentiation in the ability of white migrants to cities to accommodate. The white rural hillbilly, whose whole existence has depended upon self-sufficiency and a rather solitary struggle against the elements, should be ideally adapted to urban life, and should accommodate most quickly. The white sharecropper from the paternalistic environment of the black belt should accommodate least quickly.

If such a factor is important, then it helps explain the facts adduced by those who explain differences in adaptation and mobility on the basis of personal force, determination, and strength of will. For, if the above points hold, then a feudal or paternalistic social structure leaves its imprint on the personality, and shows up as purely individual differences in ability to become self-sufficient.

One final point is necessary: if these things are true, then they hold implications for policy matters, particularly in education. They imply that the most important element to learn for survival in urban society is how to take responsibility for one's self. The very individualism of the urban society, its lack of paternalistic authority, should help teach this; but it seems reasonable that explicit educational policy in this direction could aid as well. Seeman's results also indicate how this can be taught —by making the environment one in which the responses are contingent upon the individual's own behavior.

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Criticisms of the Concept of Alienation

Melvin Seeman's article in the November, 1963, issue of *American Journal of Sociology,* entitled "Alienation and Social Learning in a Reformatory," is of particular interest for its broad claims to (1) clarify a widely used concept with a long history, the concept of alienation, (2) support a general social psychological theory of behavior, (3) contribute to the substantive field of criminology, and (4) exemplify the use of a modified experimental methodology in a natural setting. The equally broad deficiencies of this article impose an obligation to examine it carefully both within its own framework and also from an external critical perspective.

Karl Marx and Julian Rotter loom large, and confusingly, in the background of Seeman's research. Seeman, in an earlier paper to which he refers the reader,[1] clearly states that his version of alienation does not deal with the objective conditions of alienation in society, as did that of Marx, but rather with the expectancies of individuals, and that, further, this shift "clearly departs from the Marxian tradition by removing the critical, polemic element in the idea of alienation."[2] At the same time Seeman maintains that his approach is not so "radical a departure from the Marxian legacy as it may appear."[3] But it is doubtful that students of Marx would recognize that legacy, for Marx's complex, changing, and controversial concept of alienation (as dealt with by Lewis Feuer[4] and Daniel Bell)[5] is reduced by Seeman to one word—powerlessness—and then defined in the special terminology of one current variety of learning theory. It would have been better for Seeman not to refer to Marx or any of the historical literature than to do so superficially and then

[1] Melvin Seeman, "On the Meaning of Alienation," *American Sociological Review,* XXIV (1959), 783–91.

[2] *Ibid.*, p. 784.

[3] *Ibid.*, p. 785.

[4] Lewis Feuer, "What Is Alienation? The Career of a Concept," in *Sociology on Trial*, ed. Maurice Stein and Arthur Vidich (Englewood Cliffs, N.J.: Prentice-Hall, 1963), pp. 127–47.

[5] Daniel Bell, "The Debate on Alienation," in *Revisionism: Essays on the History of Marxist Ideas*, ed. Leopold Labedz (New York: Frederick A Praeger, 1962), pp. 195–214.

narrow and distort the meaning to his own purposes.

Seeman points out how close his version of alienation is to Rotter's notion of "internal versus external control of reinforcement." He holds that this congruence "leaves the way open for the development of a closer bond between two *languages* [emphasis added] of analysis—that of learning theory and that of alienation."[6] But he then acknowledges a problem in that "these two constructs, though intimately related, are not generally used to understand the same things."[7] In support of this insight he gives a convincing example, quoting from an experiment by James and Rotter and an essay by Norman Podhoretz.[8] We, too, are convinced and hold that Seeman has shown that his version of alienation has nothing whatsoever but the *name* in common with any historical or substantive use of the term. That he continues to use the *name* "alienation" indicates that *language* has priority over empirical reality in Seeman's way of thinking.

Furthermore, Seeman's use of concepts is less than consistent and leads one to question whether the striving for efficiency has supplanted logical consistency. In this same earlier paper Seeman writes:

> With regard to the question of the generality of powerlessness, I assume that high or low expectancies for the control of outcomes through one's own behavior will (*a*) vary with the behavior involved—*e.g.*, control over academic achievement or grades, as against control over unemployment; and (*b*) will be differentially realistic in different areas (it is one thing to feel powerless with regard to war and quite another, presumably, to feel powerless in making friends). My chief point is that these are matters that can be empirically rather than conceptually solved; we should not, therefore, build either "generality" or "adjustment" into our concept of alienation.[9]

[6] Seeman, *op. cit.*, p. 785.

[7] *Ibid.*

[8] *Ibid.*, n. 8.

[9] *Ibid.*, n. 9.

Now this statement could serve as a major criticism of the later research with which we are concerned here, for in this research the I-E (Internal versus External Control) Scale is used as the measure of alienation, and this scale Seeman describes as follows:

> The test covers a wide range of behaviors (*e.g.*, making friends, war and politics, occupational success, school achievement, *etc.*); hence it is a measure of the individual's generalized expectancies for control of events.[10]

So the I-E test is not a unidimensional scale but measures, if Seeman's earlier statement is correct, a number of dimensions that have a high degree of independence and vary with the type of situation involved. Now to add further confusion, Seeman proposes to use this scale to measure a different type of behavior again, namely, the prison inmate's expectancies for control in reformatory and parole behaviors. If Seeman were consistent with his earlier statement, these behaviors would be regarded as something quite different from feeling powerless with regard to war, making friends, academic achievement, unemployment, etc. But perhaps since the 1959 article it has been decided that what were thought to be situationally related expectancies are really global, enduring, generalized personality characteristics of the kind which Rotter earlier criticized.[11] If Rotter's theory provided an improvement over some other psychological theories, it seemed to be this: Behaviors were to be considered not as direct outcomes of internal characteristics (expectancies, attitudes, traits, personality variables), but rather as a function of specific expectancies in interaction with reward values in spe-

[10] Melvin Seeman, "Alienation and Social Learning in a Reformatory," *American Journal of Sociology*, LXIX (1963), 273.

[11] Julian B. Rotter, *Social Learning and Clinical Psychology* (New York: Prentice-Hall, 1954). For a briefer and clear exposition relevant here see J. B. Rotter, "Some Implications of a Social Learning Theory for the Prediction of Goal-directed Behavior from Testing Procedures," *Psychological Review*, LXVII (1960), 301-16.

cific situations, generalization being an empirical matter in each case and not an assumption. The use of the I-E Scale seems to involve a basic departure from Rotter's theory. In Seeman's thought, apparently, expectancies now can be assumed to be so generalized and internalized that specific situational factors may be ignored to the extent of using a scale which covers a vast range of situations, all substantively different from the one under investigation. Of course, it is possible that we search too deeply in trying to understand this seeming theoretical inconsistency. Seeman may simply adhere to the practical or expedient policy common among behavioral scientists whereby, if separating subjects by means of a scale (however theoretically unsound or logically absurd) yields results —that is, statistically significant correlations or mean differences—then it is considered justified.

Other sources of doubt concerning this study may be placed under the heading of methodological weakness, accompanied sometimes by a lack of sufficient information to allow the reader to evaluate the meaning of the procedures and findings. One must wonder at the scientific rigor of introducing *post hoc* variables after hypotheses have been formulated and data analyzed. Introducing *post hoc* the dichotomous variable Conventional (Square John) versus Unconventional (Real Con), which in effect eliminates fifty-nine of the eight-five subjects, raises the correlations, which were very low even if statistically significant, to a slightly higher level; and indeed they do look more convincing. But who would have thought that arbitrarily introducing *post hoc* variables to improve the results would be part of the rigorous experimental-statistical game?

We also question the use of the social desirability scale. Seeman uses the thirty-three item true-false Marlowe-Crowne "Social Desirability" Scale as a possible control for pressures on the subject to present a certain image of himself. In a footnote in the "Results" section, Seeman says:

> The distribution in Table 1 indicates that there is a correlation between low alienation and high social desirability (the *r* in this sample was − .36), suggesting that the control for social desirability may be useful to maintain. As anticipated, proclaiming one's mastery [high Internal Control score, thus low alienation] tends to go along with a general tendency to present a socially acceptable image.[12]

Then on the next page he says:

> Ignoring the social desirability variable (which does not seem to be a critical control in the data of Table 1), we can obtain the correlations between alienation and each of the three learning scores for the entire sample. The *r* between alienation and parole learning was − .23, a figure that is significant at the 0.05 level.

It does appear that this useful social desirability control was significantly correlated with "low alienation," although the confidence level is not given and there is no satisfactory explanation for ignoring this "useful" control. The effects of the social desirability score seem to have disappeared in Table 1; we can only wonder at the tractability of statistics and consider this as further evidence for the notion that statistics are a highly manipulable and subjective tool. Indeed, as an additional point, the —.36 correlation between I-E score and social desirability score was one of the highest legitimate (that is, non-*post hoc*) correlations obtained in the study, and yet nothing is made of it, while much lower correlations are used to prove hypotheses and far-reaching extrapolations from them. As it is, even with inadequate knowledge, we must suspect that some unknown part of at least every high Internal Control score is confounded by the attempt of the inmate subject to project what he thinks is the image desired of him.

[12] Seeman, *op. cit.*, p. 275.

Another important methodological problem concerns the use of a number of background factors, as summarized in Table 6, with results described as follows:

> The main features of Table 6 are quite clear, not to say dramatic. Age and I.Q. are irrelevant to the learning scores; but continuation past the ninth grade and achievement relative to capacity [that is, difference between each inmate's I.Q. and his score on the Stanford Achievement Test] are quite relevant.[18]

What is so surprising here is Seeman's unequivocal acceptance of these results when there are so many unanswered questions concerning their validity. One of Rotter's contributions to common sense in clinical psychology has been his emphasis on psychological testing as a situation itself within a broader situation; any results obtained with an I.Q., achievement, or personality test may reflect not only what the test is measuring, but also the many situational and background variables impinging on the testing situation. Seeman, however, does not observe the caution of those who recognize psychological testing as an "interaction situation"; he does not even follow the traditional procedure of telling us which "I.Q." test was used so that we might appraise its claims for reliability and validity. Only faith or naïveté could persuade a reader to place any credence in the generalizations supported by such unknown and dubious proceedings.

The final difficulty is one of substantive knowledge concerning the area in which this quasi-experimental research was carried out, the prison. For example, Seeman is satisfied to dichotomize that complex, fluid way of life known as "doing time" into two oversimplified role configurations: Real Con and Square John (or Unconventional versus Conventional). Thus he ignores the existing penological literature which generally acknowledges at least four role configurations, that is, Square John, Right Guy (or Real Con), Con Politician, and Outlaw, or, to put it another way, prosocial, antisocial, pseudosocial, and asocial.

[18] *Ibid.*, p. 281.

Furthermore, Seeman's discrimination between Real Cons and Square Johns on the basis of staff-administered merit earnings is naïve. The institutional conditions which give rise to accommodation between staff and inmates may very well select out for rewards those inmates who can be described as Con Politicians, or pseudosocial types, who know how to *manipulate the inmate social system as well as the staff and rehabilitation apparatus.*

Thus there is no reason to claim that part of the value of this research lies in its taking place in a natural setting: first, because the nature of this setting is known to the author in only a most superficial way, and therefore the effects of the pressures of this setting could not be included as an aspect of the research itself; and second, because every effort is made to make the situation as artificial, laboratory-like and unrelated to the real prison world as possible. As with all psychological experiments and measuring devices, here, too, the question of validity is pertinent: What, if any, relationship does this small, abstracted, artificial sample of behavior have to do with dynamic events in the context and process of life in the real world?

What we are finally left with as the contribution of this study is its support of a general social psychological theory of behavior, the second point in our opening paragraph. As doubtful as it is in methodology, as weak as it is in historical and substantive knowledge and in its assumptions about the nature of alienation and of prison life, it will nevertheless go on file as additional support for a particular scientistic theory of behavior. And yet, it is only too well known that the fate of theories does not rest on their being proven or disproven by abstracted empiricistic data. And so we have the ironic situation that Seeman, who has claimed to "take the polemical element out of the Marxian

concept of alienation," has reported a study which can have no other use than to serve as a polemic for the Rotterian concept of behavior and for a scientistic reductionist sociology which sells its heritage of broad social and historical knowledge and concern for a mess of dubious psychological techniques and jargon.

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Reply

I am afraid that the style adopted by my critics does not serve them well in their review of my work. They do not fulfil the "obligation to examine it carefully"; for such an examination would reveal a very different, and more thoroughgoing, set of problems in it. I will not try to state, much less solve, those problems here; but I trust that greater clarity about what I have and have not done in my several pieces on alienation will encourage a sharper focus on the important issues.

1. There is a great deal of room for intelligent disagreement about the idea of alienation in Marx, and about what has happened to that idea since. My critics are content to advance the flat claim that my version of alienation has "nothing whatsoever but the name" in common with classical sources. Whatever the case that can be made in support of such a view, it cannot be made on the two grounds offered: (*a*) the distinction between objective conditions and individual reactions does not illuminate the matter, since Marx dealt often with both; and (*b*) it is incorrect to assert that Marx's concept of alienation "is reduced by Seeman to one word: powerlessness." I tried to make it clear, in my early theoretical paper, that alienation as "self-estrangement" was one of the central concerns in Marx; that it was embodied most clearly in his discussion of the loss of intrinsic saitsfaction in work; and that I thought this an important enough variety of alienation to attempt an orderly conceptualization of it.

No one knows better than I how approximate that conceptualization was or how elusive the solutions are—e.g., how to systematize the connections among the various brands of alienation or how best to state the idea of intrinsically satisfying work. These, among others, I take to be important problems; hence I welcome the serious efforts that have been made to criticize and modify my version of alienation, whether by those who are fundamentally friendly to it (e.g., Blauner, Zollschan, and Gibeau) or by those relatively unsympathetic to it (e.g., Feuer and Bell).

2. The question of the generality of powerlessness is an important one which demands special clarity as to what is an assumption, what is theoretically argued, and what is empirically determined. On some of this, my critics and I are in agreement: at least, they seem to think favorably about the notion that generality is "an empirical matter in each case and not an assumption."

Precisely so. That was the import of my early remarks (quoted above), and that is the import of the procedure that Rotter, the late Shephard Liverant, and I have followed in developing the powerlessness scale. This scale was originally built so that scores could be obtained for a variety of domains (social recognition, political affairs, etc.), a fact duly reported in the material I cited. What we found, after thorough pretesting and item analysis on several samples, was that there was a greater degree of generality than we had assumed there might be. The final, and shorter, version of the scale (prepared soon after the reformatory study and based in part upon its data) covers a broad range of situations. These procedures, and the publications derived from them, fulfil fairly closely what I meant when I said that generality was an empirical matter; name-

ly, (1) that I would not start by postulating generality, and (2) that I would let the data teach me something about it.

Clearly, the present data do not settle the matter. I have used more limited scales (e.g., in the recent paper on organizations and powerlessness, with Arthur Neal); and we need to find out a great deal more about the conditions and limits of generality. (In my current Swedish data, there is preliminary evidence for two relatively independent domains: "personal" control over one's own life chances and "social" control over political-national affairs.) In short, I still think that it is an open empirical matter.

On the theoretical side, sociologists have stressed the importance of situational determinants in behavior, and one of the attractive elements in Rotter's social-learning theory is, indeed, that it provides a place for the situational feature, thereby dictating caution regarding assumptions of generality. But urging the need to take account of situational determinants is not equivalent to saying that generality will not be tolerated. Any reasonably thorough theory of behavior will have to deal not only with situational specificity but with generality, as Rotter does (in his *Social Learning and Clinical Psychology*) when he describes the development of "higher-order" learning skills (p. 220), the measurement of "generalized expectancies" (p. 173), or the concept of "freedom of movement" (p. 194).

3. My critics find the design artificial and cannot honor the claim that this study makes use of the natural setting of the reformatory. Properly conceived, this is not a mere methodological point, but a broad question that can be read, "What are you up to in this work?" Rightly or not, what I am after in the long run is a social-psychological test of the logic and limits of mass society theory. Among other things, this requires a clearer statement of the theory itself, and the theoretical paper on alienation was part of that statement; but it also requires devising ways of making determinate tests, in various contexts (at work, in prison, in the hospital, in cross-cultural studies), of the structural conditions that encourage various forms of alienation and of the behavioral consequences (in social learning, political participation, etc.) that flow from alienation.

Given these goals, I do not find their argument regarding artificiality very persuasive, either in its logic or its treatment of the evidence. They reason that I use learning material the prisoners have not had access to, hence it must be useless information that these men "in real life function without." Yet we certainly do not bring the same logic to our college teaching. We regularly find it both valid and essential to convey information the students have not had access to and seem to function very well without; and sometimes the students find it relevant to their "life in the real world." As to the evidence, I do not think it unimportant to remember that the learning material is meaningfully related to the prisoner's length of sentence remaining (Table 7), his merit earnings in the prison (Table 2), or his general achievement in relation to capacity outside the prison (Table 6).

4. Much of the remainder of the criticism centers on relatively minor issues, on which the reader's close inspection will probably serve much better than the author's defense. One example will suffice to show what I have in mind. On the use of the social-desirability scale, it is not correct to imply that I (*a*) state that it might be useful to keep an eye on it further, and then (*b*) baldly ignore what I have just said. The social-desirability control is maintained in Table 5 (analyzing the true-false data) and in Table 6 (background variables), as well as in the table (on interest scores) that I refer to in note 14 (p. 280). In brief, where I introduce a new variable, the social-desirability data are taken into account. It is only after showing that this is not a significant variable *in Table 1* (i.e., as it bears on the learning data) that I find it reasonable to analyze these same learning data further without regard for social desirability.

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No one knows better than I how approximate that conceptualization was or how elusive the solutions are—e.g., how to systematize the connections among the various brands of alienation or how best to state the idea of intrinsically satisfying work. These, among others, I take to be important problems; hence I welcome the serious efforts that have been made to criticize and modify my version of alienation, whether by those who are fundamentally friendly to it (e.g., Blauner, Zollschan, and Gibeau) or by those relatively unsympathetic to it (e.g., Feuer and Bell).

2. The question of the generality of powerlessness is an important one which demands special clarity as to what is an assumption, what is theoretically argued, and what is empirically determined. On some of this, my critics and I are in agreement: at least, they seem to think favorably about the notion that generality is "an empirical matter in each case and not an assumption."

Precisely so. That was the import of my early remarks (quoted above), and that is the import of the procedure that Rotter, the late Shephard Liverant, and I have followed in developing the powerlessness scale. This scale was originally built so that scores could be obtained for a variety of domains (social recognition, political affairs, etc.), a fact duly reported in the material I cited. What we found, after thorough pretesting and item analysis on several samples, was that there was a greater degree of generality than we had assumed there might be. The final, and shorter, version of the scale (prepared soon after the reformatory study and based in part upon its data) covers a broad range of situations. These procedures, and the publications derived from them, fulfil fairly closely what I meant when I said that generality was an empirical matter; name-

ly, (1) that I would not start by postulating generality, and (2) that I would let the data teach me something about it.

Clearly, the present data do not settle the matter. I have used more limited scales (e.g., in the recent paper on organizations and powerlessness, with Arthur Neal); and we need to find out a great deal more about the conditions and limits of generality. (In my current Swedish data, there is preliminary evidence for two relatively independent domains: "personal" control over one's own life chances and "social" control over political-national affairs.) In short, I still think that it is an open empirical matter.

On the theoretical side, sociologists have stressed the importance of situational determinants in behavior, and one of the attractive elements in Rotter's social-learning theory is, indeed, that it provides a place for the situational feature, thereby dictating caution regarding assumptions of generality. But urging the need to take account of situational determinants is not equivalent to saying that generality will not be tolerated. Any reasonably thorough theory of behavior will have to deal not only with situational specificity but with generality, as Rotter does (in his *Social Learning and Clinical Psychology*) when he describes the development of "higher-order" learning skills (p. 220), the measurement of "generalized expectancies" (p. 173), or the concept of "freedom of movement" (p. 194).

3. My critics find the design artificial and cannot honor the claim that this study makes use of the natural setting of the reformatory. Properly conceived, this is not a mere methodological point, but a broad question that can be read, "What are you up to in this work?" Rightly or not, what I am after in the long run is a social-psychological test of the logic and limits of mass society theory. Among other things, this requires a clearer statement of the theory itself, and the theoretical paper on alienation was part of that statement; but it also requires devising ways of making determinate tests, in various contexts (at work, in prison, in the hospital, in cross-cultural studies), of the structural conditions that encourage various forms of alienation and of the behavioral consequences (in social learning, political participation, etc.) that flow from alienation.

Given these goals, I do not find their argument regarding artificiality very persuasive, either in its logic or its treatment of the evidence. They reason that I use learning material the prisoners have not had access to, hence it must be useless information that these men "in real life function without." Yet we certainly do not bring the same logic to our college teaching. We regularly find it both valid and essential to convey information the students have not had access to and seem to function very well without; and sometimes the students find it relevant to their "life in the real world." As to the evidence, I do not think it unimportant to remember that the learning material is meaningfully related to the prisoner's length of sentence remaining (Table 7), his merit earnings in the prison (Table 2), or his general achievement in relation to capacity outside the prison (Table 6).

4. Much of the remainder of the criticism centers on relatively minor issues, on which the reader's close inspection will probably serve much better than the author's defense. One example will suffice to show what I have in mind. On the use of the social-desirability scale, it is not correct to imply that I (*a*) state that it might be useful to keep an eye on it further, and then (*b*) baldly ignore what I have just said. The social-desirability control is maintained in Table 5 (analyzing the true-false data) and in Table 6 (background variables), as well as in the table (on interest scores) that I refer to in note 14 (p. 280). In brief, where I introduce a new variable, the social-desirability data are taken into account. It is only after showing that this is not a significant variable in *Table 1* (i.e., as it bears on the learning data) that I find it reasonable to analyze these same learning data further without regard for social desirability.

I might end, as my critics do, on the question of polemics. Theirs is a polemic against "reductionism"; but, as with alienation itself, the vibrato that the word sets up frequently obscures more than it communicates. There are several kinds of reductionism that one can be guilty of; and the least defensible, in my opinion, is the kind that reduces the possibilities for responsible and enlightening intellectual exchange.

MELVIN SEEMAN

UNIVERSITY OF CALIFORNIA
LOS ANGELES

Bensman and Rosenberg, *Mass, Class and Bureaucracy*

There seems to be no exception to the rule that every gift has its price. Gusfield's review of Bensman and Rosenberg's *Mass, Class and Bureaucracy* is a case in point. One rarely finds the traditional dullness of this *Journal* relieved, if not shattered, by a reviewer who, having dared to liken a textbook to a piece of music, adds consistency to daring by scolding the authors for "teaching the woodwinds to blow hard and the percussions to bang away" (*AJS*, LXIX [1964], 425). Novelty is hard to come by these days, and it is unusual to find a sociologist who reviews a sociological work as if it were a musical score.

Having read and used the book itself, however, my appreciation of Gusfield's musicology must be tempered by another reaction to the rest of his performance. Unfortunately, between his woodwind opening and his brass finale, Gusfield has sandwiched a job of hatchet work which is as crude as it is unfair and unwarranted. To save space I will deal with only one of several instances of unfairness. Highly critical of the authors' discussion of social norms and child-rearing, Gusfield writes, "it bears no indication that they ever consulted the complexity of existing studies or Bronfenbrenner's excellent organization of them." This charge appeared fairly serious. To investigate it I turned to the works of other specialists on child-rearing, among whom must be numbered Mr. Gusfield himself, whose discussion of it appears in the large section entitled "Man's Institutions" in Gouldner and Gouldner's text, *Modern Sociology* (New York: Harcourt, Brace & World, 1963). In this entire section, which deals with child-rearing among other subjects, there is not a single reference to Bronfenbrenner. To whom then (among others) does Gusfield refer for opinion on the subject of social class and norms? To whom but Bensman, Gerver, and Rosenberg, whose cited articles or references occupy no less than one-eighth of his entire treatment of the area (8 pages out of 60)! It seems inconsistent that, having enriched his own pages with Bensman, Gerver, and Rosenberg to the neglect of the estimable Bronfenbrenner, Gusfield should take Bensman and Rosenberg to task for showing the same preferences. Despite the fact that this shoe fits Gusfield the author, Gusfield the reviewer has not only declined to wear it but has chosen to throw it at the scholarship of two fellow writers. In his next excursion into the field of musical and/or sociological reviewing, it would be well for Gusfield to distinguish between an ax and a boomerang.

RICHARD R. KORN

CITY COLLEGE AND NEW YORK UNIVERSITY

Reply

I am grateful to Professor Korn for indicating that somebody does read book reviews. I had expected that my review might generate some reaction and am happy to know that intellectual passion is still alive.

A reviewer, in my judgment, has two

tasks: (1) to inform the reader of the nature and content of the book, and (2) to evaluate the worth of the book for the reviewer's audience. His task is complicated by the short amount of space which he has available to him. I recognize that a responsible negative criticism must indicate to its readers grounds for such judgment. In my review of *Mass, Class and Bureaucracy* I tried to do all these things.

My reference to Bronfenbrenner's work was intended as an example of a lack in the treatment of social change and child-rearing in the Bensman and Rosenberg book. It was meant to indicate, within the short confines of a review, a general slipshod treatment throughout the work. That I did not utilize Bronfenbrenner in a piece of my own writing (which touches peripherally on child-rearing) is not an answer to my objections in the review. I was not dealing with social change in that chapter. Further, my reference to Bronfenbrenner was meant merely as one instance in the total characterization of a work. This is no place for me to review my own writing, but I would certainly expect the same criteria that I utilize in my review to be utilized on my own work.

That I made much use of the past works of Bensman and Rosenberg in writing about institutions (not in reference to child-rearing, however) is evidence of my high judgment of their other work. It should also serve to indicate that my review was by no means *ad hominem*.

"Hew to the line, let the chips fall where they may."

JOSEPH R. GUSFIELD

UNIVERSITY OF ILLINOIS
URBANA

Doctor's Degrees in Sociology, 1963

According to reports received by the *Journal* from 48 departments of sociology in the United States and Canada offering graduate instruction, 174 Doctor's degrees were conferred in the calendar year 1963.

BROWN UNIVERSITY

Mary G. Powers, B.S. Teachers College of Connecticut, 1955; M.A. Massachusetts, 1957. "Demographic and Ecological Characteristics of City and Suburban Neighborhoods."

UNIVERSITY OF CALIFORNIA, BERKELEY

Robert Blauner, A.B., M.A. Chicago, 1948, 1950. "Alienation in Work: The Diversity of Industrial Development."

Hailey J. Browning, A.B. Kent State, 1949. "Urbanization in Mexico."

William H. Friedland, A.B., M.A. Wayne State, 1956, 1957. "Institutional Change: A Study of Trade-Union Development in Tanganyika."

Warren O. Hagstrom, A.B., M.A. Minnesota, 1952, 1954. "Social Control in Modern Science."

Stanley M. Honer, A.B., M.A. California (Berkeley), 1941, 1948. "The Theory of Cultural Differences."

Gianfranco Poggi, A.B. Padua, 1956; M.A. California (Berkeley), 1959. "Italian-Catholic Action: A Case Study of a Sponsored Organization."

Samuel J. Surace, A.B., M.A. California (Los Angeles), 1951, 1953. "The Status Evolution of Italian Workers, 1860–1914."

UNIVERSITY OF CALIFORNIA, LOS ANGELES

James T. Duke, A.B., M.A., Utah, 1957, 1958. "Equalitarianism among Emergent Elites in a New Nation."

Dudley Chadwick Gordon, A.B., M.A. California (Los Angeles), 1957, 1962. "Self-Conception and Social-Achievement Behavior."

Witold Krassowski, A.B., M.A. Purdue, 1952, 1954. "Naturalization and Assimilation-Proneness of California Immigrant Populations: A Statistical Study."

Charles C. Moskos, Jr., A.B. Princeton, 1956; M.A. California (Los Angeles), 1961. "Sponsored Nationalism and the Dismantlement of an Empire: A Study of Cohesion, Disintegration, and Elites in the West Indies."

Ivar P. Oxaal, A.B., M.A. Ohio State, 1954, 1959. "West Indian Intellectuals in Power: A Study in the Sociology of Knowledge and Power."

Ronald M. Pavalko, A.B., M.A. California (Los Angeles), 1956, 1961. "The Predental Student: A Study of Occupational Choice and Professional Recruitment."

Eleanora Petersen, B.A. Ohio State, 1940; M.A. California (Los Angeles), 1953. "Some Role Dimensions of Loyalty: A Study of Relationships between Obligations and Privileges Associated with Attitudes of Loyalty toward Parents, Friends, and Country."

Gerald M. Platt, B.A., M.A. Brooklyn, 1955, 1957. "Status Identification, Prejudice, and Selective Memory."

Herman Schwendinger, B.A. City College of New York, 1948; M.A. New York School of Social Welfare, 1956. "The Instrumental Theory of Delinquency: A Tentative Formulation."

Hendrik Willen Van der Merwe, B.A., M.A. Stellenbosch (South Africa), 1956, 1957. "Leadership in a Saskatchewan Community."

CATHOLIC UNIVERSITY OF AMERICA

Sister Mary Ellen Desmond, S.C., A.B. St. Elizabeth, 1950; M.A. Catholic, 1955. "Mortality Trends for the Brandywine Population of Southern Maryland."

Mary Grace Gabig, M.S.N.E., B.S.N. St. Louis, 1942; M.S.N. Catholic, 1948. "A Study of 488 Graduates of Master Programs in Nursing, the Catholic University of America, June, 1943–June, 1957."

Rev. Joseph L. Kerins, C.Ss.R., A.B. Mt. St. Alphonsus Seminary, 1944; M.A. Catholic, 1945. "The Social Thought of Pope Saint Pius X (1903–14)."

Rev. Theodore E. McCarrick, M.A. St. Joseph's Seminary, 1958; M.A. Catholic, 1960. "The Vocation Parish: An Analysis of a Group of High Vocation-Supplying Parishes in the Archdiocese of New York To Determine the Common Characteristics of the Vocation Parish."

UNIVERSITY OF CHICAGO

William Robert Arnold, A.B. Kansas, 1955; A.M. Illinois, 1956. "The Adjustment of Adolescent Males on Parole."

Mercedes B. Concepcion, S.B. Philippines, 1951. "Fertility Differences among Married Women in the Philippines."

Robert Lee Crain, A.B. Louisville, 1956. "Intercity Influence in the Diffusion of Fluoridation."

Kai T. Erikson, A.B. Reed College, 1953; A.M. Chicago, 1955. "Wayward Puritans: A Study of the Sociology of Deviance."

Robert A. Gordon, A.B. City College of New York, 1957; A.M. Chicago, 1962. "Values and Gang Delinquency."

Jan Hajda, A.B. Willamette, 1952; A.M. Chicago, 1957. "An American Paradox: People and Books in a Metropolis."

Herbert Hamilton, A.B. Roosevelt, 1952; A.M. Chicago, 1960. "Social Bases of Opinion in the Cold-War American Army."

Setsuko M. Nishi, A.B., A.M. Washington, 1944. "Japanese-American Achievement in Chicago."

Margaret Peil, S.B. Milwaukee-Downer College, 1951; A.M. Fordham, 1961. "The Use of Child-Rearing Literature by Low-Income Families."

Edna Esther Raphael, S.B. Wayne, 1940; A.M. Northwestern, 1952. "Welfare Activity in the Local Union."

Ross Paul Scherer, A.B. Concordia Seminary, 1943; A.M. Chicago, 1947. "Ministers of the Lutheran Church."

Walter L. Wallace, A.B. Columbia, 1954; A.M. Atlanta, 1955. "Bringing Up Intellectuals."

Raymond Zinser, A.M. Chicago, 1947. "Sectarian Commitment and Withdrawal."

UNIVERSITY OF COLORADO

Harold Charles Meier, B.A., M.A. Colorado, 1951, 1955. "The Oral Communication of Health-Decision Beliefs in a Serial-Reproduction Experiment."

Laurel Richardson Walum, A.B., B.A. in Education, Chicago, 1955, 1956. "Pure Mathematics: A Study in the Sociology of Knowledge."

COLUMBIA UNIVERSITY

Adolfo A. Critto, LL.B. Universitad Nacional de Ecumen, 1955. "The Sacred and the Expedient A Theory in Test of Social Cultural Consistency."

Murray Gendell, A.B. New York, 1950. "Swedish Working Wives: A Study of Determinants and Consequences."

Jerald T. Hage, B.B.A. Wisconsin, 1955. "Organizational Response to Innovation: A Case Study of a Community Hospital."

Richard F. Hamilton, B.A. Chicago, 1950. "The Social Bases of French Working-Politics Class."

Samuel Z. Klausner, B.S. New York, 1948; M.A. Columbia, 1950. "Role Adaptation of Ministers and Psychiatrists in the Religio-psychiatric Movement."

Sam D. Sieber, A.B. New School for Social Research, 1953; M.A. Columbia, 1954. "Union Members, the Public, and Inflation."

Nechama Tec, B.S., M.A. Columbia, 1954, 1955. "Gambling in Sweden: A Sociological Study."

CORNELL UNIVERSITY

Lorraine Norma Gaudreau, B.A. Syracuse, 1951, M.Ed. Smith, 1955. "Stereotypes of the Blind toward the Sighted in Contemporary American Society."

John Boswell Hudson, B.A. Oregon, 1952; M.A. Washington, 1956. "Feminine Roles and Family Norms in a Small City."

DUKE UNIVERSITY

Cyrus Murray Johnson, B.S. Wake Forest College, 1939; M.A. Ohio State, 1940. "Family Patterns and Occupational-Success Orientation."

James Carleton Kimberly, A.B., M.A. Emory, 1950, 1955. "An Experimental Test of a Theory of Status Equilibrium."

Horace Darr Rawls, B.S., M.S. North Carolina State College, 1943, 1946. "Education as a Factor in Social Participation."

EMORY UNIVERSITY

Alpha M. Bond, Jr., A.B. Dartmouth, 1952; M.A. Columbia, 1953. "Mood and Its Relationship to Attitudes toward Self and toward Groups: A Panel Study in Social Gerontology."

UNIVERSITY OF FLORIDA

Jose Fabio Barbosa-Davila, B.A., M.A. São Paulo, 1957, 1960; M.A. School of Sociology and Politics (São Paulo), 1959. "Internal Migration in Brazil—A Sociological Analysis."

FLORIDA STATE UNIVERSITY

George W. Wallis, A.B. Georgia, 1953; M.A. George Peabody, 1954. "Some Social Dimensions of Attitudes toward Civil Defense."

FORDHAM UNIVERSITY

Sister M. Janelle Cahoon, O.S.B., B.A. Mt. St. Scholastica, 1946, M.A. St. Mary's, 1954. "The Dilemmas Faced by Institutionalized Christianity in the United States."

Rev. John Doherty, S.J., B.A., M.A. Sacred Heart Novitiate (P.I.), 1949, 1950. "A Study of Change in the Religious and Social Attitudes of Eighty-five Members of the Christian Family Action Movement in a Suburban Parish."

HARVARD UNIVERSITY

Everett Merle Adams, A.B. Doane (Nebraska), 1945; M.A. Harvard, 1950. "A Functional Interpretation of Social Stratification."

Francesca Micaela Cancian, A.B. Reed College, 1958. "A Method for Studying Family Interaction."

Rev. Jacques Marcel Lazure, A.B. Ottawa, 1945, M.A. Notre Dame, 1958. "The Definition of the Priest-Teacher's Role."

Maren Lockwood, B.Sc. University College London, 1955; M.A. Maryland, 1957. "The Oneida Community: A Study of Organizational Change."

Joseph Zvi Namenwirth, "Candidaats," "Doctoraal" Amsterdam, 1954, 1957. "Bureaucratic Power and European Unification."

Sister Marie Augusta Neal, A.B. Emmanuel, 1942. "Values and Interests in the Process of Social Change."

Stewart Edmond Perry, A.B. Kenyon, 1947. "Social Processes in Psychiatric Research."

Harry Alan Scarr, A.B. Michigan, 1956. "Value-

Orientation Assessment: Developing a Methodology."

Robert Everett Stanfield, A.B. City College of New York, 1957; M.A. Harvard, 1961. "The Family and the Gang in the Aetiology of Delinquency."

UNIVERSITY OF ILLINOIS

Thomas J. Duggan, A.B., M.A. St. Louis, 1957, 1959. "Aldermanic Campaign Techniques and Decision-making Systems in Two Communities."

Philip W. Marden, B.A. Rutgers, 1955; M.A. Illinois, 1960. "Orientations of Registered Nurses toward their Professional Association."

George A. Pownall, A.B., M.A. Illinois Normal, 1952, 1957. "An Analysis of the Role of the Parole Supervision Officer."

John R. Stratton, A.B., M.A. Illinois, 1952, 1959. "The Measurement of Inmate Change during Imprisonment."

Barbara Ruth Williams, B.A. Austin, 1954; M.A. Illinois, 1957. "'American Youth' and 'Teenagers': A Sociological Analysis of Two Images of the Adolescents in American Society."

INDIANA UNIVERSITY

Stuart L. Hills, A.B. Wooster College, 1955; M.A. Indiana, 1958. "Voluntary Associations and Communicative Integration in a Suburban Community."

Joseph Walter Scott, B.A. Central Michigan College, 1957; M.A. Indiana, 1959. "Sources of Change in Community Family and Fertility in Aibonito, Puerto Rico."

IOWA STATE UNIVERSITY

Daryl Hobbs, B.S. Iowa State, 1956; M.S. Iowa State, 1960. "Attitudinal Prediction of Differential Farm-Management Ability."

Larry Kasperbauer, B.S., M.S. Iowa State, 1958, 1959. "Farmers' Images of Radio Stations."

Gerald Klonglan, B.S., M.S. Iowa State, 1958, 1962 "Role of a Free-Sample Offer in the Adoption of a Technological Innovation."

Ronald Powers, B.S., M.S. Iowa State, 1957, 1960. "Delineation of the Social Power Structure in an Iowa County."

STATE UNIVERSITY OF IOWA

Robert Claus, B.A. Iowa State, 1949; M.A. Iowa, 1955. "An Empirical Approach to the Extrinsic-Intrinsic Dimension of Attitudes toward Material Possessions."

Edward E. Harris, B.A. Lincoln, 1954; M.A. Iowa, 1958. "The Influence of Prospective Activities on Family Behavior, Expected Social Mobility, the Potential To Migrate, Motivation, and Value-Orientation."

Winfield W. Salisbury, II, A.B., M.A. California, 1954, 1956. "The Self and Anxiety."

Jerry Simmons, A.B. Iowa, 1959. "The Relationships between Attitude Positions, Alienation, and Personal Disturbance."

David Thomas, B.S. Florida State, 1959; M.A. Iowa, 1960. "An Investigation of the Process by Which Women Become Employed."

UNIVERSITY OF KENTUCKY

Gordon F. DeJong, B.A. Central College (Iowa), 1957; M.A. Kentucky, 1960. "Human Fertility in the Southern Appalachian Region: Some Demographic and Sociological Aspects."

Charles Garth, B.A. Morehouse, 1951; M.A. Atlanta, 1956. "Self-Concept of Negro Students Who Transferred or Did Not Transfer to Formerly All-White High Schools."

Ali A. Paydarfar, Law Degree Teheran, 1956; M.S. in Ed. Kentucky, 1959. "Modernization and Demographic Characteristics of Iranian Provinces and Selected Nations."

LOUISIANA STATE UNIVERSITY

Robert John Dolan, B.A., M.S. Louisiana State, 1949, 1958. "An Analysis of the Role Structure of a Complex Occupation with Special Emphasis on the Value- and Role-Orientations Associated with the County-Agent Situs."

Albeno P. Garbin, A.B. Blackburn College, 1954; M.A. Louisiana State, 1959. "An Empirical Investigation of Occupational Prestige-Trait Relationships: A Multidimensional and Comparative Consideration."

UNIVERSITY OF MARYLAND

Emmert Foster Bittinger, A.B. Bridgewater, 1945; M.A. Maryland, 1951. "Realism and Vocational Expectations."

Robert Lee Derbyshire, B.S., M.A. Maryland, 1954, 1959. "Personal Identity: An Attitude Study of American Negro College Students."

UNIVERSITY OF MICHIGAN

Paul R. Eberts, A.B. Heidelberg, 1953; B.C. Yale, 1956; A.M. Michigan, 1958. "Family Life-Cycle and Community Involvement."

Henry Elsner, A.B., A.M. Michigan, 1952, 1953 "Messianic Scientism: Technocracy, 1919-60."

Albert Friedman, A.B., A.M. Michigan, 1952, 1954. "Traditionalism and the American Creed under Contemporary *Gemeinschaft* and *Gesellschaft* Conditions."

Ronald L. Johnstone, B.A. Concordia Theological Seminary, 1956; A.M. Michigan, 1960. "Militant and Conservative Community Leadership among Negro Clergymen."

Harold N. Organic, A.B., A.M. Michigan, 1942, 1957. "Religious Affiliation and Social Mobility in Contemporary American Society: A National Study."

Hallowell Pope, A.B., A.M. Chicago, 1951, 1957. "Economic Deprivation and Social Integration in a Group of Middle-Class Factory Workers."

MICHIGAN STATE UNIVERSITY

Walter R. Banks, B.A. Rutgers, 1957; M.A. Michigan State, 1959. "A Source of Social Protest: The Predicament of the Status-inconsistent Negro."

Paul H. Besanceney, B.Lit. Xavier, 1947; A.M. St. Louis, 1954; S.T.L. West Baden, 1957. "Factors Associated with Protestant-Catholic Marriages in the Detroit Area: A Problem in Social Control."

Carl Jantzen, B.A. Bethel, 1957; M.A. Michigan State, 1959. "A Study in the Theory and Methodology in Community Time Allocation."

David M. Lewis, B.A., M.A. Maryland, 1954, 1956. "The Acceptance of Work-related Values by Young Rural Japanese."

Henry J. Watts, B.A., M.A. Buffalo, 1957, 1960. "Methodological Problems in the Measurement of Values."

MISSISSIPPI STATE UNIVERSITY

Gerald Globetti, B.A. Louisiana, 1959; M.A. Mississippi State, 1961. "Development and Application of a Level-of-Living Scale for White, Open-Country Families, Both Farm and Non-Farm, in the Southeast."

C. Raymond Sollie, A.B. Millsaps, 1959; M.A. Mississippi State, 1961. "Parental Family Characteristics and the Migration of Young Adults."

John H. Watson, A.B. Morehead State College, 1952; M.A., M.S. Mississippi State, 1958, 1961. "A Study of Social and Occupational Adjustment in Relation to Civilian and Military Identification of United States Air Force Retired Officers."

UNIVERSITY OF MISSOURI

Edward Tomich, B.A., M.A. Northeast Missouri State College, 1954, 1956. "Teacher Cognitions and Behavioral Conformity."

UNIVERSITY OF NEBRASKA

Jerry S. Cloyd, S.B. Harvard, 1948, B.S., M.S. Nebraska, 1951, 1953. "Patterns of Role Behavior in Small-Group Social Systems."

Bartolomeo J. Palisi, B.A., M.A. Brooklyn College, 1956, 1960. "Ethnicity, Family Structure, and Participation in Voluntary Association."

NEW SCHOOL FOR SOCIAL RESEARCH

Emily Alman, B.A. Hunter College, 1945; M.A. New School for Social Research, 1959. "Apperception of Self and Reality."

Norman Matlin, B.A. Yeshiva, 1952; M.A. New School for Social Research, 1955. "A Heuristic Model for Education."

Henry Seligson, B.S. City College of New York, 1942, M.A. Columbia, 1949. "Occupational Interests in School Settings."

NEW YORK UNIVERSITY

Paul Kwang-Yi Chao, B.A. St. Benedict College, 1951, M.A. St. Louis, 1953. "Analysis of Marxist Doctrine on the Family—Testing Its Validity in Soviet Russia and Communist China."

Hyman Enzer, A.B. Union College, 1938; M.A. New York, 1955. "The American First Novelist: A Study of Commitment and the Literary Career."

Norman Goodman, B.A. Brooklyn College, 1955; M.A. New York, 1961. "Communication and the Self-Image."

Dorothea Hubin, A.B., M.A. New York, 1949, 1951. "An Analysis of the Juvenile Conference Committees of New Jersey."

Theodore Kemper, A.B. City College of New York, 1949; M.A. New York, 1960. "The Relationship between the Self-Concept and the Characteristics and Expectations of Significant Others."

Arthur Niederhoffer, B.A., M.A. Brooklyn College, 1937, 1956. "A Study of Police Cynicism."

UNIVERSITY OF NORTH CAROLINA

Charles M. Bonjean, A.B. Drake, 1957; M.A. North Carolina, 1959. "Community Leadership: A Conceptual Refinement and Comparative Analysis."

John Rochester Earle, A.B. Wake Forest College, 1958; M.A. North Carolina, 1961. "Marital Relation and Family Integration."

James Allen Williams, Jr., A.B. North Carolina, 1958; M.A. Cornell, 1961. "Interviewer-Respondent Interaction: A Study of Bias in the Information Interview."

NORTHWESTERN UNIVERSITY

Gilbert M. James, A.B. Greenville, 1955; M.A. Washington (St. Louis), 1957. "Individual Anomia, Societal Anomie, and Some Intervening Variables."

UNIVERSITY OF NOTRE DAME

Gordon J. DiRenzo, B.S., M.S. Notre Dame, 1956, 1957. "A Social-Psychological Analysis of Personality Structures of Members of the Italian Chamber of Deputies."

Joseph Thomas Doran, A.B., M.S. Notre Dame, 1948, 1949. "Family Social Position and School Expectations as Factors Influencing the Peer Group Status of Fifth- and Sixth-Grade Elementary-School Children."

John J. Lennon, A.B. John Carroll, 1948, M.A. Catholic, 1951. "An Analysis of the Family Patterns of Selected Puerto Rican Families in a Selected Area of Chicago, Illinois."

OHIO STATE UNIVERSITY

Joseph Louis Albini, B.A. Pennsylvania State, 1954, M.A. Louisiana State, 1956. "Psychotherapy with Disturbed and Defective Children: An Evaluation of Change in Behavior and Attitudes."

Patrick Tracy Cleaver, A.B. Miami (Ohio), 1956; M.A. Ohio State, 1960. "The Development of an

Index for Estimation Social-Class Levels of City Blocks."

Ernest Paul Donald, B.A. Lucknow, 1936; M.S.W. Ohio State, 1952. "Self-Concept of Sixth-Grade Boys: A Study of Delinquency-Proneness."

John Ross Eshleman, B.A. Manchester College, 1958; M.Sc. Ohio State, 1960. "Mental Health and Marital Integration as Applied to Married Youth."

Norman Joseph Johnson, Jr., B.A. Duquesne, 1958; M.A. Ohio State, 1960. "Toward a Taxonomy of Organizations."

Lawrence Noel Moyer, B.A., M.A. Ohio State, 1959, 1960. "Occupational Variables and the Role of the Sick."

Harry Raymond Potter, B.S. Iowa State, 1957; M.Sc. Ohio State, 1960. "Disabled Workers' Conception of Their Work Limitations and Vocational Potential."

Constantine Safilio Rothschild, M.S. Ohio State, 1959. "The Reaction to Disability in Rehabilitation."

Hans Sebald, B.A. Manchester College, 1958; M.Sc. Ohio State, 1959. "Parent-Peer Control and Masculine-Marital Role Perceptions of Adolescent Boys."

UNIVERSITY OF OREGON

James T. Borhek, M.A. California (Berkeley), 1960. "An Incongruence Theory of Intolerance."

David L. Dodge, B.A. San Diego State, 1958. "Social Stress, Integration, and Chronic-Disease Morbidity."

Martin Meissner, B. Comm. British Columbia, 1958. "Behavioral Adaptions in Industrial Technology."

Richard O'Toole, M.A. Oklahoma, 1959. "Experiments in George Herbert Mead's 'Taking the Role of the Other.'"

Caleb Paulus, M.A. Madras (India), 1940. "The Impact of Urbanization on Fertility in India."

Clyde Pope, M.A. Chicago, 1960. "Occupational Mobility as an Independent Variable."

Curt Tausky, B.A. Portland State, 1959. "Career Anchorage Points of Middle Managers."

UNIVERSITY OF PENNSYLVANIA

Kenneth Edward Burnham, A.B. Berea College, 1940; M.A. Pennsylvania, 1947. "Father Divine: A Case Study of Charismatic Leadership."

Perry Levinson, B.A. Western Maryland College, 1951; M.A. Pennsylvania, 1953. "The Relationship between Sibling Position and Reading Ability."

Martin Oppenheimer, B.S. Temple, 1952; A.M. Columbia, 1953. "The Genesis of the Southern Negro Student Movement (Sit-in Movement): A Study in Contemporary Negro Protest."

UNIVERSITY OF PITTSBURGH

Joseph J. Grau, B.A. Cathedral College, 1947; M.S.A. Fordham, 1953. "Permissive Treatment of Disabled Persons: A Sociological Study."

M. Ruth McIntyre, A.B. Seattle Pacific, 1947; M.A. Pittsburgh, 1948. "The Organizational Nature of an Urban Residential Neighborhood in Transition: Homewood-Brushton, Pittsburgh."

John S. Zawacki, B.S., M.A. Purdue, 1951, 1953. "The System of Unofficial Rules of a Bureaucracy: A Study of Hospitals."

PRINCETON UNIVERSITIY

Bernard Beck, A.B. Brooklyn College, 1958; M.A. Princeton, 1960. "The Real World of the Little Men."

David Chaplin, A.B. Amherst College, 1953; M.A. Princeton, 1957. "The Recruitment of the Peruvian Industrial Labor Force."

Rodney K. Crook, B.A. Leeds, 1958; M.A. Princeton, 1960. "Role Differentiation and Functional Integration: A Structural Model of a Mental Hospital Ward."

Paul J. Hollander, B.A. London School of Economics, 1959; M.A. Illinois, 1960; M.A. Princeton, 1962. "The New Man and His Enemies: A Study of the Stalinist Conception of Good and Evil Personified."

Charles Parker Wolf, A.B. North Carolina, 1955; M.A. Princeton, 1960. "The System Concept in Sociological Analysis."

PURDUE UNIVERSITY

Lois R. Cohen, B.A. Pennsylvania, 1960; M.S. Purdue, 1961. "Internal Migration of Scientists Awarded Doctorates in the United States in 1958."

Margaret Jacobson, B.S. Minnesota, 1947; M.S. Cornell, 1956. "Coping with Heart Disease—A Study of Farm Families."

Gerald Kallas, B.A. St. Olaf College, 1959; M.S. Purdue, 1961. "The Impact of Heart Disease: A Panel Study of Changes in Health Information, Work, Behavior, and Utilization of Medical Services."

Stanley Robin, B.A., M.A. Ohio State, 1955, 1957. "A Comparison of Male-Female Roles in Engineering."

Robert N. Whitehurst, B.A. Butler, 1959; M.S. Purdue, 1961. "Employed Mothers' Influences on Working-Class Family Structure."

UNIVERSITY OF SOUTHERN CALIFORNIA

Genevieve Delta Carr, A.B. Ohio Wesleyan, 1935; A.M. Southern California, 1952. "A Psychosociological Study of Fertile and Infertile Marriages."

Ruth Block Michaelson, A.B. Los Angeles State College, 1956; A.M. Southern California, 1958. "An Analysis of the Changing Focus of Marriage Counseling."

Billyanna Mary Niland, A.B., D.D.S., M.A. South-

ern California, 1940, 1944, 1948. "Social Factors Related to Dentistry as a Career."

Fernando Penalosa, A.B. Denver, 1949; M.A., Ph.D. Chicago, 1950, 1956; M.A. Southern California, 1959. "Class-Consciousness and Social Mobility in a Mexican-American Community."

SOUTHERN ILLINOIS UNIVERSITY

Herschel E. Aseltine, B.A., B.D. McMaster, 1951, 1952; M.A. Chicago, 1955. "Urbanity and Oligopolis: The Dispersed City in Southern Illinois."

SYRACUSE UNIVERSITY

Thomas John Fararo, A.B. City College of New York, 1959. "A Study of Participant Nets."

Abdul-Khalik Mahmoud Zikry, B.S. Cairo University, 1941; M.S. North Carolina State, 1960 "Socio-economic Determinants of Egyptian Fertility."

UNIVERSITY OF TEXAS

Robert B. Hagedorn, A.B. San Francisco State College, 1950; M.A. Washington, 1953. "Reactions to Deviation: A Study in Social Control."

Sanford Irwen Labowitz, A.B. California (Los Angeles), 1960; M.A. Texas, 1961. "Technology and Division of Labor."

VANDERBILT UNIVERSITY

Paul Deutschberger, B.A. City College of New York, 1940; M.A., M.S.W. Columbia, 1941, 1948. "Tasks, Bargains, and Identities in Social Interaction."

WASHINGTON UNIVERSITY (ST. LOUIS)

Mark Jay Abrahamson, A.B. Illinois, 1960. "The Integration of Industrial Scientists."

Elizabeth Daniels Ossorio, B.A. California, 1943; M.A. Washington (St. Louis), 1958. "Clergymen and Community Resources."

James Arthur Wiggins, B.S. Iowa State, 1957; M.A. Washington (St. Louis), 1959. "Interpersonal Interference, Goal-Interlocking, and Aggression."

UNIVERSITY OF WASHINGTON

Jeannette Ruth Folta, B.S. Boston, 1959. "Nurses' Perceptions of Role Requirements and Role Action in Situations Involving Death in Hospitals."

Walter Max Gerson, A.B., M.A. Montana State, 1957, 1958. "Social Structure and Mass Media Socialization."

George Carleton Myers, A.B. Yale, 1953; M.A. Washington, 1960.

Peter Stephan Spitzer, Central Washington, 1957; M.S. Washington, 1960. "Perception and Interpersonal Behavior Following Focal Communication."

WASHINGTON STATE UNIVERSITY

Leland J. Axelson, B.A, M.A. Washington State, 1949, 1958. "A Study of the Marital Adjustment and Role Definitions of Husbands of Working and Non-working Wives."

WAYNE STATE UNIVERSITY

Grant Bogue, B.A., M.A. Michigan State, 1950, 1954. "Segregation and Discrimination in a Small Northern City."

WESTERN RESERVE UNIVERSITY

Alan Crain, B.A. Wooster, 1955; M.A. Western Reserve, 1959. "Mother-Child Interaction and the Behavior of Diabetic and Non-diabetic Children."

YALE UNIVERSITY

John F. O'Rourke, A.B. Massachusetts, 1956. "Field and Laboratory: A Study of Family Groups."

Eugene Royster, A.B. Antioch College, 1953; M.A. Yale, 1957. "Socialization and Interaction: An Experimental Study of Intergroup Behavior."

Robert H. West, A.B, M.A. Yale, 1957, 1959. "Professional Task Performance and Training: A Study of the Admission Note and the Physician."

Doctoral Dissertations Newly Started in 1963

The following list of 176 doctoral dissertations, *newly started* in 1963, in universities and colleges in the United States and Canada is compiled from returns sent by 47 departments of sociology. This list includes dissertations in social work, divinity, and other related fields.

BRANDEIS UNIVERSITY

Paul Campanis, A.B. Harvard, 1958; M.A. Brandeis, 1962. "A Study of a Modern Factory System."

Rogers P. Johnson, A.B. Antioch, 1947; M.A. Columbia, 1960. "The Relationship between Organizational and Personal Politics in an Urban Community."

BROWN UNIVERSITY

Alvin Callott Dorse, A.B., A.M. Cincinnati, 1955, 1961. "Changing Distribution of Population and Economic Activity, United States, 1950–60."

Stephen Lawrence Finner, A.B. Colby College, 1960. "The Impact of Urbanism on Fertility and Family Structure in Eleven Indian Villages."

Calvin Goldscheider, B.A. Yeshiva, 1961; M.A. Brown, 1963. "Jewish Fertility Differentials in the Providence Metropolitan Area."

Richard Warren Osborn, A.B. Flint College, University of Michigan, 1959. "Social and Economic Factors in the Health of Older Persons."

Edward Thomas Pryor, Jr., A.B. College of St. Thomas Aquinas, 1954; M.A. Michigan State, 1961. "Family Organization in Eighteenth- and Nineteenth-Century New England."

UNIVERSITY OF CALIFORNIA, BERKELEY

Sherri Cavan, A.B., M.A. California (Los Angeles), 1959, 1961. "Public Drinking Places in an Urban Setting."

O. Andrew Collver, A.B., M.A. Oregon, 1955, 1956 "Human Fertility in the Western Hemisphere"

Miguel Murmis, A.B. Buenos Aires, 1958. "The Emergence of a National Popular Regime: The Case of Peronism in Argentina."

Audrey Wipper, A.B., M.A. McGill, 1952, 1955 "The Labor Movement in Kenya and the Role of the International Confederation of Free Trade Unions."

UNIVERSITY OF CALIFORNIA, LOS ANGELES

Tamar Becker, B.S., M.A. Teachers College, Columbia, 1952, 1953. Anticipatory Adjustment: A Mechanism of Attitude Selection."

Judith Ellis, B.A. London School of Economics, 1959; M.A. California (Los Angeles), 1961. "The Attitude of White Liberals to the Negro Movement for Civil Rights."

CATHOLIC UNIVERSITY OF AMERICA

Rev. Douglas F. Campbell, B.A. St. Francis Xavier, Antigua, 1955; M.A. Catholic, 1962. "Religion and Values among College Students in Nova Scotia."

Sister M. Esther Heffernan, O.P., M.A. Chicago, 1951. "Roles, Conformity, and Deviance in an Inmate Social System."

Sister M. George O'Toole, R.S.M., B.A. St. Joseph's (Windham), 1951; M.A. Catholic, 1961. "A Religious Community as a Social System."

UNIVERSITY OF CHICAGO

Jacob Feldman, "A Study of Recruitment into the Medical Profession."

David Heise, A.B. Missouri, 1959. "The Effects of Motivation on Word Choice in Verbal Behavior."

Terrance A. Nosanchuk, A.B. Wayne State, 1958, A.M. Chicago, 1962. "A Multivariate Analysis of *The Acquaintance Process*."

Larry Rosenberg, B.A. Brooklyn College, 1956. "Communication Styles and Learning."

William Simon. "Economy, Community, and Morale: A Contextual Analysis."

UNIVERSITY OF COLORADO

David William Gordon, B.A. New York, 1958. M.A. Colorado, 1961 "The Cultural and Role Content of Psychotherapy Sessions"

Azmy Ishak Ibrahim, B.A. Alexandria (Egypt), 1955; Diploma of the "Social Sciences" High Institute of Education (Egypt), 1956. "The Process of Group Identification as Causal Variable for Reference Group Theory"

Truxon Richard Young, A.B. Eastern Michigan, 1952; M.A. Michigan, 1958 "Some Correlates of Role Convergence: A Study in Adult Socialization."

COLUMBIA UNIVERSITY

Arnold Birenbaum, A.B. City College of New York, 1960. "Comparative Study of Social Protest Movements."

Leiba Brown, A.B. Maryland, 1956. "Educational Research and the Liberal Arts."

Madeleine Giguere, A.B. College of New Rochelle, 1947; M.A. Fordham, 1948. "Age at Marriage"

Stanley S. Guterman, A.B. Chicago, 1956 "A Study of Machiavellianism among Hotel Personnel."

Nathan Joseph, B.S.S. City College of New York, 1943. "The Political Socialization of Adolescents."

Helen M. Simon, B.S., M.A. New York, 1951, 1954. "Peer and Stranger Relationships between Public Health Nurses and Patients."

Nicholas Tavuchis, A.B. Columbia, 1955. "Social-Class Mobility and Family Cohesion: A Study of Greek-American Families."

UNIVERSITY OF CONNECTICUT

Harry Kienzle, A.B. Omaha, 1960; M.A. Connecticut, 1961. "The Philosophy and Sociology of Émile Durkheim: A Study of the Philosophical Presuppositions of a Science."

DUKE UNIVERSITY

Herbert Collins, A.B. Brooklyn, 1942; A.M. Duke, 1946. "The Socioeconomic Composition of Constituent Parts of Urbanized Areas."

Adam Clarke Davis, B.S. North Carolina, 1956; M.S. North Carolina State College, 1962. "Family Relationships and the Process of Disengagement."

Kenneth Edward Miller, B.A., M.A. Alabama, 1953, 1956. "The Structural Correlates of Community Power Systems."

UNIVERSITY OF FLORIDA

Leonard L. Linden, A.B., M.A. Florida, 1957, 1959. "An Appraisal of the Quality of Mortality Data from the Nations of South America."

FLORIDA STATE UNIVERSITY

Joanine H. Gavin, B.A., M.A. Bradley, 1954, 1957. "Norm Content and Leadership as a Function of Verbal Reinforcement."

FORDHAM UNIVERSITY

Rosemary Higgins Cass, B.A. St. Elizabeth, 1950; LL.B. Columbia, 1953. "The Effects on Family Life of Mothers in Paid Employment."

Sister Mary Vera Duvall, R.S.M., B.A. Mount St. Agnes College, 1950; M.A. Catholic, 1951. "A Sociocultural Study of the Convergence of Some Fundamental Analogues in Christianity and Hinduism, Buddhism, and Confucianism."

Madeline Engel, B.A. Barnard College, 1961; M.A. Fordham, 1963. "A Definition and Description of Subcultures within the Lower Class of New York City."

Sister Mary Fabian Matthews, B.A. Dalhousie, 19[illegible]; M.A. Gonzaga, 1956. "The Role of the School in the Integration of the Italian Immigrant Child."

HARVARD UNIVERSITY

Charles Dean Ackerman, A.B. California (Los Angeles), 1961. "Marriage in Primitive Societies: Three Studies of the 'Affinal Collectivity.'"

Stephen David Berger, B.S. City College of New York, 1961; A.M. Harvard, 1963. "Postwar Germany: A Study of Large-Scale Social Disintegration and Reintegration."

Robert Raymond Blain, A.B. Massachusetts, 1960. "A Panel Study of Changes in Individualistic-Collectivistic Values."

Dexter Colboyd Dunphy, A.B., M.Ed. Sydney (Australia), 1956, 1961. "The Differentiation of Non-rational Roles in the Evolution of Small Social Systems."

Rev. John Drennan Early, S.J., A.B., A.M., and Ph.L. St. Louis, 1950, 1952, 1952. "An Ethnography of the Saints in Zinacantan."

Bruce Finnie, A.B. Harvard, 1956. "Category and Magnitude Scales of Attitudes."

Martha Midwood Kant, A.B., M.A. Boston, 1959, 1960. "Institutional Careers of Delinquent Boys."

Edward Otto Laumann, A.B. Oberlin, 1959; M.A. Harvard, 1962. "Social Distance and Urban Stratification."

William Roy McPherson, A.B. Macalester, 1961; M.A. Harvard, 1963. "Lobbying as an Integrative Factor in Political Processes."

William Michael Michelson, A.B. Princeton, 1961; M.A. Harvard, 1963. "The Relationship between Value Orientations of People and the Physical Form of Cities."

Kenneth Irwin Rothman, A.B. New York, 1960; M.A. Harvard, 1962. "Organizational Consequences of Changing Patterns of Recruitment and Control."

Richard Paul Taub, A.B. Michigan, 1959; M.A. Harvard, 1962. "The Role of Elites in a Changing Indian City."

UNIVERSITY OF ILLINOIS

Lois Jean Begitske, A.B. Blackburn College, 1958; M.A. Indiana, 1961. "Delinquency Patterns in Córdoba, Argentina: A Cross-Cultural Study of the Delinquent Subculture."

Aaron M. Bindman, A.B., M.A. Illinois, 1960, 1961. "A Study of the Relation between Association Patterns and Academic Achievement of Negro College Students within a Midwestern State University."

Charles W. Dean, A.B. Ashbury College, 1955; M.A. Illinois, 1961. "Inconsistencies of Sectarian Ideology and the Socialization of Sect Members."

James N. McCrorie, A.B., M.A. McGill, 1958, 1959. "The Farm Movement in Saskatchewan."

Emil Oestereicher, A.B., M.A. Illinois, 1960, 1962. "The Social Theories of George Lukacs."

INDIANA UNIVERSITY

Sheldon Bockman, B.A., M.A. Colorado, 1958, 1961. "Power Processes within and between Institutional Sectors of a Small City."

David H. Howard, A.B. Fisk, 1950; M.A. Indiana, 1953. "The American Negro's Dilemma: Attitudes of Negro Professionals toward Competition with Whites."

Francis Okediji, A.B. Ottawa, 1960; M.A. Kansas, 1961. "Strangers and Access to Opportunity Structures for Social Interaction on University

Campuses: A Study of American Students' Reactions at Indiana and Purdue."

Grafton D. Trout, A.B. Miami, 1951; M.A. Indiana, 1961. "Comparable Urbanism and Fertility."

IOWA STATE UNIVERSITY

Melathathil Johanan John, B.S. Allahabad (India), 1948; M.A. Agra (India), 1951. "People's Participation in Community Development Programs of India."

Everett Laning, B.A. Simpson College, 1953; M.A. Northwestern, 1959. "Clergy and Their Careers."

Jenniev J. Poulson, B.S. Brigham Young, 1933; M.S. Iowa State, 1954. "Factors Affecting Extent of Goal Agreement between Husbands and Wives."

Dick Warren, B.S., M.S. Iowa State, 1952, 1960. "Influence of a Dealer-training Program."

STATE UNIVERSITY OF IOWA

Irving L. Allen, A.B. Morris Harvey College, 1959; M.A. Iowa, 1961. "The Effects of Cultural Levels in Iowa Communities."

Jack Balswick, B.A. Chico State College, 1960; M.A. Iowa, 1962. "The Family as a Factor in Vocational Rehabilitation Success."

James A. Black, A.B., M.A. Iowa, 1959, 1963. "Sex Crimes in Iowa: A Sociological Analysis."

Jerrold Buerer, A.B. Luther, 1960; M.A. Iowa, 1961. "Urbanization and Party Competition: A Study of Selected Midwest States."

Phillip C. Campbell, B.S. Eastern Michigan, 1960; M.A. Iowa, 1963. "Higher Education and Social Mobility: Migration Plans of College Graduates in Sweden."

Dean S. Dorn, B.A. Northern Illinois, 1959; M.A. Iowa, 1962. "Empirical Investigation of the Self-Concept of Members of Contra-Cultures and Subcultures."

Richard L. Ingersoll, B.A., B.S. Central Michigan College, 1958; M.A. Iowa, 1961. "Self-Complementarity and Similarity: An Empirical Study of Mate Attraction."

Donald McTavish, B.A., M.A. Iowa, 1956, 1960. "The Situs and Industry Components of Occupations: Predictive Utility in Five Surveys."

Harold Mark, B.A., M.A. Iowa, 1959, 1961. "The Attitudes and Values Associated with Agricultural Land Tenure."

Joseph H. Meyerowitz, B.A. Chicago, 1953; M.S. Ed. Hunter, 1956. "Sociological Aspects of Early Identification and Special Treatment of the Educationally Retarded Child."

Donald G. Moore, B.S. Central College of Connecticut, 1959; M.A. Northwestern, 1962. "The Behavior of the Patients in the Mental Hospital as Related to the Formal and Informal Dimensions of Total Institutional Structure."

James H. Parker, A.B. Bates College, 1958; M.A. Chicago, 1961. "Patterns of Consultation: A Study of Public School Professional Employees."

LaFrances Rose, A.B. Morgan State College, 1958; M.A. Fisk, 1960. "A Test of Three Stages of Harry Stack Sullivan's Developmental Theory of Personality: Juvenile Era, Preadolescence and Early Adolescence Stages."

Raymond L. Schmitt, B.A., M.A. Notre Dame, 1958, 1959. "An Empirical Study of Social Integration."

Robert Walsh, B.S. Illinois State Normal, 1961; M.A. Iowa, 1962. "Dynamics of Community Power Structure and Stratification System."

Ronald Wilson, B.A. Wisconsin State, 1960; M.A. Iowa, 1961. "The 'Health Team' and the Community."

JOHNS HOPKINS UNIVERSITY

Jerome Kirk, A.B. Reed College, 1960. "The Moral Career of the College Freshman: Socialization of the Undergraduate Community at a Technological Institute."

UNIVERSITY OF KENTUCKY

M. Jay Crowe, B.A., M.A. Washington State, 1956, 1959. "The Occupation Adaptation of a Selected Group of the Eastern Kentuckians in Southern Ohio."

Narsi Patel, B.A. Calcutta, 1945; M.A. Banaras Hindu (India), 1950; B.Ed. Gujarat (India), 1954. "Determinants of Communication Pathways in Farm Communities: A Dyadic Analysis."

LOUISIANA STATE UNIVERSITY

John Drysdale, B.A. Millsaps, 1959; M.A. Louisiana State, 1962. "Faculty Participation in Voluntary Organizations."

Roland Wingfield, B.A., M.A. Louisiana State, 1960, 1961. "Haiti: A Case Study of an Underdeveloped Area."

UNIVERSITY OF MICHIGAN

Michael T. Aiken, B.A. Mississippi, 1954; A.M. Michigan, 1955. "Kinship in an Urban Community."

T. R. Balakrishnan, B.A. Madras (India), 1951; A.M. Michigan, 1953. "Migration and Opportunity: A Study of Standard Metropolitan Areas in the United States."

Richard LeBlond, A.B., A.M. Cincinnati, 1959. "Changing Patterns of Professionalization and Organization in the Performance of Serious Music in the United States."

David Reynolds, A.B., A.M. Michigan, 1955, 1956. "The War and Peace Policies of Selected Protestant Denominations."

Roy C. Treadway, A.B. Earlham, 1960; A.M. Michigan, 1962. "Metropolitan Population Decentralization."

Donald Warren, A.B., A.M. Wayne State, 1957, 1959. "Formal Organizational Structure and the Character of Informal Peer Relations: An Investigation of Teachers in Diverse Public School Milieux."

MICHIGAN STATE UNIVERSITY

Steven Deutsch, B.A. Oberlin College, 1958; M.A. Michigan State, 1959. "Skill Level, Social Integration, and Ideology: A Study of Automobile Workers."

Gary King, B.A. Grove City College, 1951; M.A. Michigan State, 1957. "Modes of Adjustment to Suburban Residence: A Comparative Study of Three Midwestern Suburbs."

Malcolm McReynolds, B.S. Missouri, 1951; M.A. Michigan State, 1956. Exploratory study concerning some hypothesized variables within the concepts of belief, sentiment, means, goals, and reference groups as they are related to social interaction within social systems; an investigation into how some of the above vary among social-structural categories; and relationships between these concepts and choice behavior.

MISSISSIPPI STATE UNIVERSITY

Joseph H. Bruening, B.A., M.A. Florida, 1954, 1956. "Some Factors of the Internal Frame of Reference of the Departmental Chairman."

Edward C. Lehman, Jr., B.A., M.A. Mississippi State, 1958, 1963. "Social Participation and Locality Reference-Group Orientation among Urban Adult Males."

Kenneth P. Wilkinson, B.A. Louisiana College, 1960; M.A. Mississippi State, 1962. "The Community and Limited-Interest Programs."

UNIVERSITY OF MISSOURI

George William Barger, A.B. Butler, 1948; M.A. Christian Theological Seminary, 1952. "Historical Sociology: Its Nature and Practice."

Paul Frederick Green, A.B. Missouri, 1956. "Power Expectations in the Perception and Resolution of Role Conflicts."

John Jacob Hartman, B.S. Southwest Missouri State College, 1961; M.S. Missouri, 1963. "A Structural Analysis of the Intermediaries in the American Dairy Association Communication Program."

Irma Bendel Mathes, B.S., M.A. Washington (St. Louis), 1945, 1959. "Role of Female as Perceived by Males; by Age Groups."

Etha Sue Titus, B.S. Texas Woman's University, 1960; M.A. Missouri, 1962. "A Study of the Self-Concept of Deaf Teen-agers."

UNIVERSITY OF NEBRASKA

David Janovy, B.A. Wayne State College (Nebraska), 1956; M.A. Nebraska, 1962. "Participation in Activity and Adjustment to Retirement."

NEW SCHOOL FOR SOCIAL RESEARCH

Elliott Camerman, B.A., M.A. New School for Social Research, 1949, 1955. "Organic Gardening and Farming: A Case History in the Perception of Anomie."

Peter Chang, Doctorate of Canon Law, Urbanion (Rome), 1952; M.A. Fordham, 1957. "Social Types in Chinese Novels, 1800–1900."

NEW YORK UNIVERSITY

Boris Karashkevych, A.B. Brooklyn, 1955; M.A. New York, 1959. "The Postwar Fertility of the American Negro."

Edward Sagarin, A.B., M.A. Brooklyn College, 1961. "Sex Deviation and Social Protest."

UNIVERSITY OF NORTH CAROLINA

Bert Newton Adams, A.B. William Jewel, 1954; B.D., and Th.M. Southern Baptist Seminary, 1959, 1960. "Values, Affection, and Interaction in the Wider Kinship Group."

Charles Norman Alexander, A.B. Alabama, 1961; M.A. North Carolina, 1963. "Aspects of Structural Perceptions."

Richard Balyon Ames, A.B. George Washington, 1958; M.A. American, 1962. "Methodological Study of Multiple-Regression Analysis of Survey Data."

Paul R. Dommermuth, A.B. Houghton College, 1958; M.A. Rochester, 1960. "Retail Pharmacist: Professional Contingencies in a Business Setting."

Nancy Leonora Gates, A.B. Oberlin College, 1960; M.A. North Carolina, 1962. "Types of Organizational Commitment."

Paulivelil M. George, A.B. Earlham College, 1958, M.A. Boston, 1961. "Evaluative-Cognitive Balance and the Centrality of the Occupation in the Occupational Choice of College Students."

Gerard J. Hunt, A.B. Fordham, 1959; M.A. Emory, 1962. "Friendship Patterns in Two Southern Communities."

Satoshi Ito, A.B. Long Beach State College, 1960, M.A. North Carolina, 1963. "Sociological and Psychological Correlates of Negro Education Aspirations."

Dean Knudsen, A.B. Sioux Falls College, 1954, M.A. Minnesota, 1961. "The Family and High-School Dropouts."

Charles V. Mercer, A.B. Florida State 1957, M.A. North Carolina, 1960. "A Theory of Role Assumption and Involvement."

Clyde Z. Nunn, A.B. Furman, 1957, M.A. Louisville, 1962. "Determinants of Achievement Motivation."

Charles Ray Wingrove, A.B. Richmond, 1959, M.A. North Carolina, 1962. "Rural-Urban Continuum in Industrial Society: A Conceptual Reformulation and Case Study."

NORTHWESTERN UNIVERSITY

John Warren Barkey, B.A. Nebraska, 1955, M.A. Northwestern, 1963. "Changes in Urban Social Differentiation: An Application of Social Area Analysis."

Shigeo Nohara, B.A. Hawaii, 1950; M.A. Northwestern, 1963. "Segregation, Social Participation, and Anomia: A Study of Urban Negroes."

Ruth Simms, B.A. Talladega College, 1958; M.A. Northwestern, 1963. "Participation in Voluntary Organizations in Accra, Ghana."

James K. Skipper, B.S. Northern Illinois, 1956; M.A. Northwestern, 1960. "Social Relations in a Hospital Setting."

Martin S. Weinberg, B.A. St. Lawrence, 1960; M.A. Massachusetts, 1961. "Sex, Modesty, and Deviance."

James L. Wilkins, B.S. Northern Illinois, 1958; M.A. Northwestern, 1960. "Collective Erasures: Changing of Social History."

UNIVERSITY OF NOTRE DAME

Sister Virginia McGreevey, O.S.B., A.B. Mount Angel College (Oregon), 1949; M.A. Gonzaga, 1955. "A Social Survey of Aged Catholics in the City of Portland, Oregon."

Sister James Rau, O.P., A.B. Aquinas, 1942; M.A. Catholic, 1949. "Attitudinal Correlates of Catholic Education: A Study in the Sociology of Education."

OHIO STATE UNIVERSITY

P. K. Geevarghese, M.A. Miami, 1961. "Factors in the Growth and Decline of Protestant Churches."

UNIVERSITY OF OREGON

John Scanzoni, A.B. Wheaton College, 1958; "Resolution of Role Incompatibility in Church and Sect Clergy Marriage."

UNIVERSITY OF PENNSYLVANIA

Finn Hornun, Law, Copenhagen, 1954; M.A Haverford College, 1956. "A Cross-Cultural Comparison of the Measurement of Delinquency."

Gerald Robin, B.A. Temple, 1958; M.A. Pennsylvania, 1960. "A Sociological Analysis of Occupational Deviance: A Study of Employee Theft in Department Stores."

Leroy O. Stone, A.B. Howard, 1960. "Demographic Concomitants of Economic Changes in Subareas of Puerto Rico, 1950–60."

Walter F. Zenner, A.B., M.A. Notre Dame, 1950, 1952. "Catholic-Non-Catholic Differentials in Fertility in Norristown."

UNIVERSITY OF PITTSBURGH

Stanley E. Shivley, B.A., M.A. Colorado, 1953, 1956. "Relationships between Temporal Horizons and Behavior."

PRINCETON UNIVERSITY

Victor A. Liguori, A.B. Haverford College, 1959; M.A. Princeton, 1961. "Stability and Change in the Social Structure of Atlantic Coast Commercial Fisheries."

Gerald Rosenblum, A.B. California (Berkeley), 1957; M.S. Oregon, 1962. "Social Stratification and Social Change."

Irving Zeitlin, B.A., M.A. Wayne State, 1958, 1961. "Sociological Theory and Method in Karl Marx."

PURDUE UNIVERSITY

Eugene G. Sherman, Jr., A.B. Fort Valley State, 1954; M.A. Southern Illinois, 1955. "A Substantive and Methodological Study of Family Data in Leon County, Florida."

UNIVERSITY OF SOUTHERN CALIFORNIA

John F. Crowther, B.A., M.A. Southern California, 1955, 1956. "Power Struggle, Anomie, and Goal in Formal Organizations: A Case Study of an Industrial Firm."

Michael Faia, B.A., Southern California, 1959; M.A. Chicago, 1962. "Extremism as a Function of Socioeconomic Status, Alienation, and Political Position."

Robert Eugene Knoll, B.A., M.S.W. California (Los Angeles), 1953, 1958. "The Differential Experience of Siblings: A Study in Delinquency."

Alexander Blair Taylor, B.A. California (Los Angeles), 1955; M.A. Southern California, 1959. "Role Perception and Marital Adjustment."

SYRACUSE UNIVERSITY

Leo Miller, B.A. Harvard, 1940; M.S. in S.S. Boston University, 1948; M.P.H. Harvard School of Public Health, 1955. "Social Aspects of Schizophrenia."

Robert J Turner, A A. Santa Rosa Junior College, 1955; B.A. Sacramento State College, 1957. "Social Structure and Crisis."

UNIVERSITY OF TEXAS

Jerry Kenneth Benson, A B. Baylor, 1959; M.A Texas, 1962. "A Study of Anomie, Alienation, and Social Structure."

Walter J. Cartwright, A B. Southern Methodist, 1943; M.A. Texas, 1960 "The Minority-defining Process in a Minority without Ethnicity."

Jess Lord, B.S. Texas Wesleyan College, 1960, M.A. Texas Christian, 1961. "Development and Case-Study Test of an Axiomatic Sociological Theory of Organization Merger."

Jerome Michel, A.B. Texas A&M College, 1954, M.A. Texas Christian, 1960 "The Structure of Influence in the Legislative Process."

Ted R. Vaughn, B.S., M.A. Sam Houston State College, 1955, 1957. "Group Determinants of Self-Conception: An Empirical Assessment of Social-Interaction Theory."

UNIVERSITY OF TORONTO

Samuel Sidlofsky, B.A., M.A. Toronto, 1960, 1962 "Postwar Immigrants in the Changing Metropolis with Special Reference to Toronto's Italian Population."

TULANE UNIVERSITY

William W. Pendleton, B.A. Wofford College, 1958; M.A. Tulane, 1962. "Middle-Class Mobility and Values: A Study of the Urban-Industrial Transition in Cali, Colombia."

VANDERBILT UNIVERSITY

Douglas Lipton, B.A., M.A.T. Vanderbilt, 1960, 1961. "The Conforming Boy in the Deviant Family."

WASHINGTON UNIVERSITY (ST. LOUIS)

Kathleen Ann Archibald, B.A. British Columbia, 1957; M.A. Illinois, 1960 "The Role of the Social Scientist as Outside Expert in Public Policy Planning."

Sarah Boggs, B.A., M.A. Washington (St. Louis), 1951, 1953. "An Analysis of Crime in St. Louis. A Reconceptualization of Crime Rates and Patterns."

Mary W. Sengstock, Ph.B. Detroit, 1958, M.A. Michigan, 1960. "New Telkaif. A Study in the Maintenance of Community Boundaries."

UNIVERSITY OF WASHINGTON

Richard Boyle, B.A. San Diego, 1958, M.A. Washington, 1963 "Factors Affecting Level of Aspiration among High-School Girls."

Allen Dorius, A.B. San Francisco State, 1955, M.S. Utah, 1957. "Interaction of Social and Contextual Factors in the Reinforcement Effects of Mass Communications."

Leroy Gould, A.B. Harvard, 1959, M.A. Washington, 1962. "Social Perception of Deviant Behavior."

Joseph Jones, B.A., M.A. Washington, 1960, 1962 "Structural Ambiguity: Its Occurrence and Effects in Hospital Settings."

Joseph Julian, A.B. San Francisco State, 1958, M.A. Washington, 1961 "Compliance Patterns and One Dimension of Organizational Effectiveness in Hospital Structures."

William R. Larson, A.B., M.A. Washington, 1955, 1958 "Intrafamily Role Relations and Adolescent Behavior."

Clifford Paxton, A.B. Seattle Pacific, 1957, M.A. Washington, 1961 "Effects of Competition upon Interpersonal and Object Orientations."

Johannes Schiller, B.A. Capital, 1945, M.A. Kansas City, 1959. "The Significance of Universals, Particulars, and Value Commitment in the Process of Socialization."

Thomas Steinburn, B.S., M.A. Washington, 1951, 1959. "Goals: Achievement Norms and Perception of the Self."

M. Glenn Walker, B.A., M.A. Stanford, 1952, 1953. "Rites of Passage in Contemporary American Society."

Judith Wilks, B.A. Denison, 1956; M.A. North Carolina, 1961. "Preparation, Process, and Produce: An Investigation of the Interrelationship of Group Properties and Member Perceptions."

WASHINGTON STATE UNIVERSITY

Harold Frank Mackey, B.A. Hope College, 1943 "A Study of the Prestige Associated with Selected Personal Attributes and Role Functions in a Land-Grant University."

WESTERN RESERVE UNIVERSITY

Juanita Murphy, B.A. Oklahoma Baptist, 1955; M.S. Western Reserve, 1961 "Status Congruence: Some Inferences of Status Differentials within the Clinical Situation of the Professional Nurse."

Ruth Rautenstrauch, B.A. Meredith, 1944; M.A. Bucknell, 1948 "Socioeconomic Status, Social Involvement, and Alienation as Factors in Political Protest."

Sherwood B. Slater, B.A. Western Reserve, 1957, M.S.W. Ohio State, 1959 "Utilization of the Kinship Network by Families with Chronically Ill Members: A Longitudinal Study."

YALE UNIVERSITY

Gerald D. Bell, B.S., M.A. Colorado, 1960, 1961 "Structure and Process in Organizations: A Comparative Study within the General Hospital."

Donald E. Larsen, A.B. Minnesota, 1955, M.A. Indiana, 1959. "A Study of Role Consensus: The Role of the Psychiatric Nurse."

Lois Kay Richards, B.A. Kent State, 1958, M.A. Yale, 1959 "Indices of Social Isolation: A Study of Social Resources Following Childbirth."

Powhatan J. Wooldridge, B.A. Chicago, 1956, M.A. Florida, 1960 "Observer Reliability in Interaction-Process Analysis: A Case Study of a Measurement Problem."

BOOK REVIEWS

Formal Theories of Mass Behavior. By WILLIAM N. MCPHEE. New York: Free Press of Glencoe, 1963. Pp. viii+244. $5.95.

Mathematical formalizations in the social sciences sometimes give the impression that they are divorced from reality by the large gap of operationalization. One solution to this problem may be, as Coleman once suggested, to worry less about the formalization of models for measuring properties and to go directly to the formalization of processes. McPhee illustrates in this book how one may go about such formalization, by developing models that are intended to reflect processes either to account for their temporal development or for their final equilibrium distribution. In the course of developing models of this sort, various unobservable properties are deduced, and ways to "measure" them from empirical data are also derived from the formal statement of the process. McPhee further proposes, in a suggestive but sometimes ambiguous introduction, that one may realistically assume that social behavior may be analyzed into separate processes, to be modeled separately (in "modules") and later combined to obtain a formal theory of the more complex aggregate.

The processes presented by McPhee, with one exception, have to do with collective behavior in semistructured situations. The first utilizes a sophisticated extension of a simple Markov model to account for variations in the quality of mass culture by considering the screening methods employed. The extension involves the realistic assumption that different items of culture are characterized by different transition probabilities, so that one does not make the mathematically more simple assumption of a homogeneous body of culture. Ingenious deductions (all of these models are characterized by "deductive richness") are made about the nature of the culture "mix" at any particular time as assumptions are varied. Chapters ii and iv deal with a digital computer model of the influences impinging on the electorate during a campaign, with which readers may be familiar from earlier presentations; McPhee suggests that, by incorporating assumptions about individual dispositions, sociometric relations, individual "learning" processes, and stimulation from political issues, he has answered criticisms of an overly deterministic view of social-choice behavior. Because of the obvious difficulty in communicating about the details of a computer program, this model still retains certain areas which have to be taken by the interested reader as "black boxes."

The third chapter covers what may be considered a learning model, of a simple sort, applied to "real-world" situations in which the presentation of the stimulus itself is problematic. Various paradoxical findings, largely of the "market research" sort but with much more general applicability, are explained, at least in part, by this model. Again, the model utilizes a sophisticated assumption of heterogeneity—this time in the subjects rather than the stimuli—in what may be viewed as a continuous-time stochastic process.

The final chapter deviates from the others by presenting a model of what seems best viewed as individual behavior in an unstructured situation—one of "passions," "addictions," etc., in which the processes are actually psychological rather than sociological. Nevertheless, it may be of the most interest to readers interested in formal theorizing because it is perhaps the easiest model to follow, utilizing simple difference equations, and because of the remarkable number of insightful applications that are suggested. In contrast to the earlier mathematical models, and like the computer simulations, this model focuses on the variations over time of processes conditioned by a simple set of parameters which seem (intuitively) to reflect something very real about the addiction process.

A great virtue of the presentation of these models is that each one is also accompanied by the application, in some sense, to actual empirical data. At many points the mathematics is not easy to follow, but it seems accurate and sophisticated; the difficulty is that each chapter could be expanded into a book of its own.

It takes a remarkably creative mind to *(a)* see the problematic character of everyday empirical observations, *(b)* translate the problematics into mathematical models that are simple enough to work with mathematically yet not trivial in interpretation, and *(c)* deduce a variety of subtle implications from the formal properties of the model. This is not something one can expect of the typical mathematician, or the typical social scientist, or even the typical mathematical sociologist. This book is unique in the extent to which it has accomplished all three of these things.

ROBERT H. SOMERS

University of Michigan

Arbeiten zur Soziologie. By THEODOR GEIGER. Edited by Paul Trappe. Neuweid: Hermann Luchterland Verlag, 1962. Pp. 484.

This book is a selection of the writings of Theodor Geiger (1891–1952), a German sociologist who spent much of his adult life in Scandinavian countries. He also made extensive visits to Canada and the United States. The selection has been made by Paul Trappe, who has written a biographical and critical introduction to it. The book is published as Volume VII of *Sociological Texts* edited by Heinz Maus and Friedrich Furstenberg.

Geiger wrote voluminously on a variety of subjects. The excerpts in this collection are organized under four headings. First is a section on methodology which contains Geiger's dynamic analysis of social mobility and the mechanics of social fluctuations. The second part deals with the analysis of large contemporary societies, with special emphasis on the mass phenomena that they exhibit. This part also includes articles on social stratification and the theory of social classes. A third part contains extracts on the sociology of law and an analysis of norms. Finally, there is a critical analysis of the concept of ideology as used by Mannheim and its function in mass society. A complete bibliography of Geiger's writings is supplied.

Geiger developed his thought in the tradition of Georg Simmel's conception of sociology, and his empirical studies follow lines familiar to American sociologists. The chief merit of this book, therefore, is to make some of the formulations of modern sociology accessible to German readers.

THEODORE ABEL

Hunter College

The Problem of Social-Scientific Knowledge. By WILLIAM P. MCEWEN. Totowa, N.J.: Bedminster Press, 1963. Pp. xxix+590. $10.00.

In this book a philosopher inquires into the problem of acquiring dependable knowledge in the behavioral sciences and sets himself the task of systematizing the epistemological principles that emerge out of the study of human behavior. McEwen, who is professor of philosophy at Hofstra College, expresses the hope that in so doing he can provide the social sciences "with something like the epistemological foundation that Whitehead formulated for the natural sciences."

It is unfortunate that McEwen in pursuing this aim did not adopt the procedure employed by Whitehead. Whitehead used the substantive generalizations (laws and theories) discovered by physical scientists as the basis for his philosophical analysis. In contrast, McEwen rests his case exclusively on the programmatic writings of behavioral scientists dealing with methodological questions instead of on the results of social science investigation. Most of these writings (McEwen's chief sources of reference for sociology are Lundberg, MacIver, and Parsons) are not analyses of substantive findings but present with more or less cogency the opinions and preferences of the writers on issues that pertain to the scientific status of the behavioral sciences. As a consequence of his sources, McEwen's book does not provide an independent, freshly conducted analysis by a philosopher of the results of behavioral research.

However, the book is valuable for students and scholars in several respects. First among these is McEwen's co-ordination and comparison of the methodological writings of anthropologists, economists, historians, political scientists, psychologists, and sociologists and his systematic discussion of their common problems. Second is his construction of a "synoptic model" which is a comprehensive conceptual framework for the representation

of behavioral data. Third is a summary of principles and rules for obtaining reasonably acceptable knowledge about human behavior. Fourth is the introduction of an illuminating distinction between knowledge-situation (the method and criteria of warranted belief) and the meaning-situation (what kind of knowledge is desired and is obtainable). McEwen shows conclusively that the futility of most methodological discourses is due to the lack of recognition that in all sciences the knowledge-situation is derived from the meaning-situation, that is, epistemology has priority over methodology. He also presents excellent arguments in favor of situational versus radical relativism and of a broad versus a narrow (logico-positivistic) operationalism.

This book is strongly recommended to all those who have doubts or questions about their conduct as scientists. Parts III and IV, in which the epistemological and methodological patterns of reflective inquiry are discussed, should be required reading for all students in the behavioral sciences. Indeed, one might hopefully envisage a time when social scientists with McEwen's book and the brilliant *Conduct of Inquiry* by Abraham Kaplan on their shelves will desist from further excursions into methodology and concentrate on the explanation and interpretation of the significant and puzzling phenomena in their fields.

THEODORE ABEL

Hunter College

Invitation to Sociology: A Humanistic Perspective. By PETER L. BERGER. ("Anchor Books.") Garden City, N.Y.: Doubleday & Co., 1963. Pp. viii+191. $0.95.

Peter Berger's witty, urbane *Invitation to Sociology* should be read together with the late C. Wright Mills's *The Sociological Imagination* and Edward Shils's essay, "The Calling of Sociology," which closes Volume II of Parsons' *Theories of Society;* for like these it is primarily indicative of the drift of sentiment in sociology. Mills's statement is the pamphleteering of a rebel mounting a mocking, slashing attack on his field; Shils's essay is the white paper of a self-conscious defender of the structural-functional status quo. In his praise of "consensual sociology," his emphatic insistence on the value of positive thinking in sociology, and his denunciation of all other traditions, Shils emerges as sociology's redoubtable Norman Vincent Peale. Peter Berger's *Invitation to Sociology* is the nearest thing to a straight line that can be drawn between the positions of C. Wright Mills and that of Edward Shils and the structural-functionalists.

Like Mills, Berger objects to current sociological tendencies to employ the physical scientist as a model, to emphasize statistical techniques and to be concerned excessively with methodology. His view, that the sociological journals are concerned with trivia and the society with intrigue, is quite parallel. "The sensible person reads the sociological journals mainly for the book reviews and the obituaries and goes to sociological meetings only if he is looking for a job or has other intrigues to carry on" (p. 11). Like Mills he assimilates sociology most closely to history and maintains that the sociological imagination (Berger's phrase is "form of consciousness") is a peculiar property of the modern mind (p. 25). At the same time, against Mills and with the structural-functionalists, he rejects the conspiratorial concepton of society (p. 112) and in the end identifies his own position most closely with that of Shils. "A recent statement on the same subject that is quite close to the viewpoint here presented . . . may be found in Edward A. Shils's "The Calling of Sociology" (p. 186).

Some of the apparent clarity of Berger's volume is produced by the eclectic procedure of treating a series of partly conflicting theoretical positions successively as if each were correct. Hence in one chapter he writes like the most radical holist. One could not ask for more extreme formulations of the causal priority of society to the individual than: "One becomes wise by being appointed a professor . . . ready for battle by marching in formation" (p. 96), or "the young man with anxieties about virility becomes hell-on-wheels in bed when he finds a girl who sees him as an avatar of Don Giovanni" (p. 102). Elsewhere, however, Berger abandons such extreme holism and accepts an almost equally extreme elementarism. "In one way or another, we are all impostors. The ignoramus impersonates erudition, the crook honesty, the skeptic conviction—and any normal uni-

versity could not exist without the first confidence trick, no business organization without the second and no church without the third" (p. 135).

In the same manner Berger assigns to sociology both a value-free task (p. 5) and an evaluative task (p. 162). He assigns to it both a debunking function (pp. 38–39) and the task of avoiding such debunking (p. 163). In the end he makes a plea for a humanistic sociology. "This humanism to which sociology can contribute is one that does not easily wave banners, that is suspicious of too much enthusiasm and too much certainty. It is an uneasy, uncertain, hesitant thing, aware of its own precariousness, circumspect in its moral assertions" (p. 162). Humanistic sociology, it would appear, can perhaps best be described as faint-hearted ethics.

DON MARTINDALE

University of Minnesota

Der Begriff der Natur in der Lehre von Marx. By ALFRED SCHMIDT. *Frankfurter Beiträge zur Soziologie,* Vol. XI. Frankfurt: Europäische Verlagsanstalt, 1962. Pp 182.

Philosophical and theological interest in Marxism and especially in the young Marx has been extensive in western Europe since 1945, but in the United States the so-called *Economic and Philosophic Manuscripts* of the Paris period (1844) were not published until 1961. In the same year Robert Tucker brought out his study relating the early Marx to the later (*Philosophy and Myth in Karl Marx*). American writers have tended to approach Marx segmentally from the viewpoint of their interests in industrialization, stratification, and work satisfaction or from the perspective of psychoanalysis. Rare have been interpretive studies by native Americans, such as Vernon Venable's *Human Nature: The Marxian View* (1946), which failed to take into account the Paris manuscripts or Daniel Bell's later essay on alienation.

Although Alfred Schmidt's study appears in a sociology series, it is a philosophical and philological analysis, which collates those passages in Marx that refer directly or implicitly to nature. Schmidt shows that Marx's concept of nature is not that of "shallow" materialism but is sociohistorical because of its dialectical relationship to human labor: Nature produces man as a conscious and active subject who confronts it as a "natural force," and man changes his own nature by overcoming the "estrangement" and "externality" of external nature. But it is important to understand that this process is infinite and that no complete "reconciliation" of man and nature is possible. Following Horkheimer and Adorno's theory that the "dark" philosophers are the true representatives of the Enlightenment, Schmidt makes a case for Marx as one of the great pessimists, whose utopia does not do away with what another great enlightened pessimist called the reality principle: The abolition of wage labor cannot remove the realm of necessity and the irksomeness of creative work.

The author builds his case on yet another relatively unknown part of Marx's work, the draft notes of 1857–59 for the first volume of *Das Kapital* and for *A Contribution to the Critique of Political Economy* (published as *Grundrisse der Kritik der politischen Ökonomie* [Berlin, 1953]). Not surprisingly, Schmidt claims that this draft contains the most important philosophic formulations of Marx and provides an indispensable link between the earlier and the later writings. Like most Marxist literature, Schmidt's work, too, has an apodictic strain, yet the present reviewer would agree that the difference between the young philosopher and the older economist has been exaggerated—a point by now made by Marcuse, Fromm, Tucker, and others that the Hegelian affinity even of Marx the economist is probably greater than has been apparent and that the eschatological element has been overly stressed (in Löwith's writings and other anthropological interpretations).

Readers may want to consult Schmidt for the latest European literature on Marxism. To a significant extent his study seems to arise out of a strong interest in the work of Ernst Bloch, the aged Marxist utopian, who is practically unknown in this country but presently prominent in Western Germany, if only for his defection from the East.

GUENTHER ROTH

State University of New York
Stony Brook

Principles of Sociology. By AJIT KUMAR SINHA. Agra: Laksmi Narain Agarwal, 1963. Pp. v+293. Rs. 10.

This book was written for the introductory sociology course for students in Indian universities. Hence the author selects a number of examples from Indian social life to demonstrate some of the principles he discusses. The sociological content of this textbook is a more important feature than the matter of for whom the book was written.

The author of the book is both a sociologist and a philosopher. The philosophical emphasis found in this volume constitutes one way in which this textbook differs markedly from those textbooks written by American scholars. The author asserts that "Sociology gives us clear understanding of the laws of social development" (p. 1). In speaking of the relation of sociology to social philosophy, the author asserts that "the chief aim of social philosophy consists in discovering the meaning of the actual mode of existence in society. Social philosophy tries to interpret the results of social science" (p. 19). American sociologists, in the reviewer's opinion, may find this attribute objectionable solely on sociological grounds. The reader will also find no references to the empirical literature. The author relies on the published books of other writers, and some of these are introductory textbooks by American writers. The book is relatively short, containing only seven chapters.

The topics which the author includes in this volume are: the nature, scope, and methods of sociology; the nature of human society; the individual and society; social organization; society as the social order; social disorganization; and social change. The discussion of these topics, for the most part, represents an admixture of sociology from the past to present, philosophy, and the works of European, American, and Oriental scholars. The author relies to a great extent upon the insights of the founding fathers of sociology and social philosophers. The use of these sources of information often results in the incorporation of ideas which have been either discarded or considered less important in relation to current developments in sociology. One can point to other instances in which it appears that the author is writing from the viewpoint of informal and personal observations.

The reviewer does not know the extent to which this type of textbook is used in Indian universities, or the extent to which this book is representative of the development of sociology in India. In his opinion, this textbook does not offer its readers the best selection of what is available. From the reviewer's knowledge of various professional activities, American and Asian scholars have been working co-operatively toward the goal of developing, promoting, and stimulating the growth of sociology in Asian countries. Sinha's book reflects the need for continued or more intensified communication among sociologists at the international level.

EDWARD E. HARRIS

Prairie View Agricultural and Mechanical College Texas

Community Power and Political Theory. By NELSON W. POLSBY. New Haven, Conn.: Yale University Press, 1963. Pp. 152. $5.00.

One hesitates to contradict such notables as Bernard Barber, James Coleman, and Norton Long in their high praise (quoted on the jacket) for this slim volume, but it seems to me that only in the last twenty-six pages is a contribution made to theory and method in the study of community power relationships. Much of the rest presents an attack on the application to this field of stratification theory, "one of the commonest, most conventional perspectives from which sociologists view social life" (p. 108). Polsby charges that "stratification theorists" believe that a single upper-class power elite totally rules community life exclusively on behalf of their own class interests, with political and civic leaders in subordinate roles and all forms of social conflict relevant to community power relationships reflecting the clash between upper and lower classes. Moreover, these theorists are addicted to "reputational" techniques for assessing power relationships. Both data contradicting their thesis and evidence of their methodological shortcomings can be found in the very publications of this "school."

A fifth of the substantive text is used to expose flaws in the research procedures, theoretic reasoning, and empirical generalizations of the Lynds, W. Lloyd Warner, and August

Hollingshead. Polsby sometimes sounds as if he were the first to discover that the descriptions and analyses of Middletown, Yankee City, and Elmtown-Jonesville provided us only with seminal beginnings, not final conclusions. Hunter's study of Atlanta gets worked over once again—for eleven pages—while his improved study of Salem is once again neglected. Articles by Pelligrin and Coates, Schulze, and Miller receive their knocks for the next eleven pages. Only footnotes and asides recognize other and more recent studies that ought to have been taken into account in a revised and refreshed version of this section of Polsby's "reincarnated doctoral dissertation" (p. viii). The theoretic and methodological attack on the stratification thesis continues through a chapter on the New Haven study undertaken by Dahl, Wolfinger, and Polsby, followed by a reprise on the incongruities between stratification patterns and power relationships in American communities. The useful and stimulating last two chapters provide a "pluralist" image of community power structures emphasizing complexity, flexibility, and lack of stability and suggest protocols for future research into local systems of social power.

Polsby's intense and unremitting criticism of others invites similar treatment of his own text, without the balancing generosity one usually implants in a review. He makes most of the same kind of errors that he exposes. Space permits noting only a few. He refutes the caricature which he calls "stratification theory" but neither formulates nor invalidates explicit relational propositions in which social characteristics and power relationships are treated as variables rather than nominal categories. He shows that many middle-class people are important in community decision-making, while many upper-stratum individuals are not, but he ignores both the general exclusion of lower-middle and lower-class personnel from significant decision-making roles, other than voting, and the proportionate overrepresentation of those upper-middle-class and upper-stratum persons, just as he neglects the high correlation with class status of at least seven of the eleven kinds of power "resources" listed under pluralist theory (pp. 119–20). In the opening chapter a conception of power almost equivalent to general social causality (similar to March's definition of "influence" which disregards intent and deliberate resistance) is confounded with one which sees social power as one category of causal variables (following Simon and taking account of goals and opposition); this confusion is never resolved. Moreover, he fails to see how the New Haven study, along with all the rest, suffers from our lack of any consensus about one or more operational units of social power. Polsby properly whacks many sociologists for failing to sample adequately from the universe of local decision-makers, yet contrives an elaborate but unconvincing apology for failing to sample adequately from the universe of community decisions. He lambastes researchers who impose their own often middle-class values on others by deciding what various groups' "objective" interests are, and then asserts that community decisions can be evaluated for importance "objectively" by taking account of numbers of people affected, amounts and kinds of resources involved, and degrees of change in actual resource distribution. He searches out statements which violate practical wisdom and "good" theory, but feels free to assert that the climate of opinion of the whole city of New Haven was substantially changed largely by "the work of one man Mayor Lee" (p. 71). One could go on, but why? "People in glass houses . . . etc."

In spite of Polsby's footnote declaration to the contrary (pp. 12–13), this book often has the feel of one of those ritual raids on sociology by which some adherents to the upstart sect of behavioralism prove they are really still loyal to the tribe of Polisci. In addition, most of these arguments have been aired in monographs, journals, and conference papers, making the book redundant for anyone who has kept abreast of publications and current commentary in this area of theory and research. It can be useful for supplementary reading for courses on community structures, power relationships, social stratification, research design, and the like, though many teachers would do well to compare it with the more economical paperbound monographs, *Social Science and Community Action*, edited by C. R. Adrian, and *Current Trends in Comparative Community Studies*, edited by B. E. Swanson.

WARNER BLOOMBERG, JR.

University of Wisconsin—Milwaukee

The Structure and Government of the Carpenters' Union. By MORRIS A. HOROWITZ. Pp. xv+168. *Labor's Paradox—the American Federation of State, County, and Municipal Employees AFL–CIO.* By LEO KRAMER. Pp. xx+174. *Democracy in the International Association of Machinists.* By MARK PERLMAN. Pp. xi+113. *The International Brotherhood of Teamsters: Its Government and Structure.* By SAM ROMER. Pp. xi+160. *The Government of the Oil, Chemical, and Atomic Workers Union.* By MELVIN ROTHBAUM. Pp. xv+185. *The Brotherhood of Railroad Trainmen: The Internal Political Life of a National Union.* By JOEL SEIDMAN. Pp. iv+207. *Governing the UAW.* By JACK STIEBER. Pp. xiv+188. *The Government of the Steel Workers' Union.* By LLOYD ULMAN. Pp. xii+200. ("Trade Unions Monograph Series," edited by Walter Galenson.) New York: John Wiley & Sons (for the Center for the Study of Democratic Institutions), 1962.

Commissioned by the Fund for the Republic under the general editorship of Walter Galenson, these nine monographs on internal labor politics are a response to the growing academic interest in the problems of democracy in private government and the recent topical concern with union corruption. They confirm the impression of most labor specialists that the typical union is neither an oligarchy with a monolithic power structure negating the rights and interests of its members, nor a pluralistic Garden of Eden ripe with competitive politics and institutionalized dissent. Sensitive to the multifaceted character of union democracy, the authors tend to concentrate on four dimensions: responsiveness to the needs and interests of their memberships, the distribution of power between local unions and national officers, institutional safeguards for the rights of individual members, particularly dissenters, and opportunities for organized opposition to the national leadership.

The result is a mixed, though generally positive balance sheet. Viewing these organizations as a sample of American labor as a whole, the monographs suggest that unions are most universally and satisfactorily democratic with respect to responsiveness. And in many unions, local and intermediary bodies retain considerable autonomy with respect to negotiating contracts and determining working conditions, although there is a general long-run trend toward centralization of power in the national office. As for protecting the rights of members who come into conflict with the union apparatus, the record is surprisingly good: the majority have constitutional provisions that permit appeals and new hearings with a certain amount of (though not enough) due process guaranties. The tolerance and acceptance of organized challenges to leadership is clearly the weakest link in the democratic complex. Only two unions, the Brotherhood of Railroad Trainmen (BRT) and the Oil, Chemical, and Atomic Workers (OCAW), have a tradition of tolerance toward "factional" opposition movements; elsewhere contests for top offices are rare and anti-administration currents tend to be viewed as subversive expressions of "dual unionism" that must be suppressed through invoking slogans of unity and solidarity.

The diversity of institutional solutions to the problems of democratic control is striking. The OCAW has an executive board made up of rank-and-file working members, which functions as a "watchdog" group over the decisions of the full-time officers and professional staff. The United Automobile Workers has its Public Review Board, a group of eminent citizens outside the union empowered to make final decisions on the appeals of members who have been aggrieved by its internal judicial processes. The BRT is unique in permitting candidates who are defeated in an election for president or other high office to run for one of the many vice-presidencies or other offices at the same convention—thus preserving a political base to carry on their criticisms of administration policies.

Seidman's study of the Trainmen strikes me as the outstanding book in this series with much of value for the general sociologist and the organizational analyst. In contrast to the other studies, which rely primarily on secondary sources such as published union convention proceedings, Seidman also observed the day-to-day workings of this extremely interesting union at the local level and gives us a flavor of the typical grievances of its members and the conflicts within the organization, as well as good analyses of the work roles of the railroaders, the "role sets"

of general chairmen and other officials, and the impact of the personalities and political styles of three union presidents on the nature of their administrations. The relatively unexplored area of the judicial process within formal organizations has been advanced by Horowitz' analysis of the content and the disposal of appeals made by aggrieved members of the Carpenters' Union to its national president—this is the most interesting use of primary source materials in the series. In the majority of cases, the president either reversed the local union's decision against the member or drastically reduced its disciplinary action, an indication that even in a union which is far from a model of democratic procedure and "ethical practice," a strong president may function to safeguard individual rights. Ulman's analysis of the "dues protest" movement of the Steel Workers during the late 1950's will provide valuable source material for students of organizational conflict.

Despite their rich empirical materials, the series as a whole and many of the individual monographs are disappointing. Largely factual and historical, the studies not only lack a theoretical framework, but they also contain little analysis. Research tends to be somewhat superficial, relying primarily on union documents, especially constitutions and conventions, previously published histories, and interviews with officers and staff. Of course the lack of more intimate knowledge and insights also reflects the difficulties of getting "inside" information about these sensitive questions in a period when unions are under heavy attack, a problem which is most manifest in Romer's very unsatisfying book on the Teamsters. Further, some of the writers seem too willing to justify union practices, a "positive-thinking" tendency carried to the extreme in Kramer's monograph on government employees, where the author's admiration for the national president approaches a cult of personality. But the most disappointing aspect of this research enterprise is its lack of a comparative perspective. These parallel studies of nine unions with diverse histories, socioeconomic conditions, and industrial environments provided an ideal situation for the development of comparative generalizations on the causes and conditions of democracy in the labor movement. But aside from the brief introductions to each volume by the series editor, Walter Galenson, each monograph is written as if it bore no relation to others. What a missed opportunity!

ROBERT BLAUNER

University of California, Berkeley

Cities and Space: The Future Use of Urban Land. Essays from the Fourth Annual Resources for the Future Forum. Edited by LOWDON WINGO, JR. Baltimore: Johns Hopkins Press, 1963. Pp. 261. $5.50.

The several papers brought together in this book deal with two major themes. One concerns the implications of modern transportation and communication technology for the concept of urban space and for the realization of the advantages of urban life. The other challenges the relevance of prevailing planning concepts for guiding urban development in an age of high mobility.

Three planners, Melvin Webber, Stanley Tankel, and Catherine Bauer Wurster, join issue over the probable and the desirable pattern of urban space. Is human intercourse and freedom of access to opportunity to be fostered by allowing the logic of transportation and communication improvements to work their dispersive effects on the urban settlement pattern or by restraining those tendencies to preserve a traditional concept of the city? The cogently argued debate raises for the reader a number of ancillary questions. For example: What is "realism" and what is "romanticism" in a consideration of that issue? Is the assumption that some proportionality must be maintained between the urban scale and "the human scale" merely doctrinaire, or does it have some substance to it?

In the examinations of planning concepts and procedures that follow, there seems to be general acceptance of the proposition that the old imperatives which built the compact city are no longer operative. Frederick Gutheim sees the new situation as a unique opportunity to employ design to achieve an expression of man's values and aspirations. To him design means not a master-plan; instead it means a continuing artistic effort

to illuminate and harmonize indigenous variety. But he leaves open the question of what values or whose values should gain expression. Robert Dahl observes that middle-class rather than broadly held communal or welfare values have dominated planning. But he also offers no prescription for the education of planners to the responsibility for selecting and interpreting values for an entire community. A criticism of more immediate bearing, which might also be construed as a suggestion for planning education, emerges from Charles Haar's admirable review of the history of land use controls. He finds planning guilty of vagueness of objectives, of inattentiveness to market mechanisms, and of failure to articulate local plans with federal policies and programs.

The content of this book, including the introduction by the editor and an epilogue by Henry Fagin, possesses a seminal quality of an unusually high order. It is as provocative as it is informative. It deserves to be widely read.

Amos H. Hawley

University of Michigan

Man, Time, and Society. By Wilbert E. Moore. New York: John Wiley & Sons, 1963. Pp. vii+163. $4.75.

There are some topics inherently more pretentious than others, and people ought not write about them unless they have something quite substantial to say. God, the syllogism, the nature of man, the meaning of *Hamlet*, and the temporal ordering of men's and organizations' activities are examples of such topics, for different reasons. Moore has brought his very substantial intellectual resources to bear on the last of these, and it is difficult to decide whether to be pleased with the power of his mind, interested by the lode he has tried to open, or unhappy at how little metal has been produced. Although this is a wise and intelligent book, it will probably not stimulate the further work desperately needed in this area, for Moore has not taken the many opportunities to point out the research implications of his summaries of previous work and of his theories.

For example, he points out that one main resource that organizations devote to reach goals is time. This immediately suggests that we measure the goals of such highly scheduled organizations as secondary schools by studying the crude number of hours they devote to different purposes. The schedules would be easy to obtain on a large scale, and could be related to community characteristics, school administrative ideologies, and so forth.

Or when he points out the interrelations between age and career stage, he might suggest that we study the variance of ages at which people go through such rites of passage as high-school graduation, marriage, promotion to full professorships at Princeton, retirement, or time of reaching a million dollars in assets. Such data would be relatively easy to collect over historical periods and in different societies, so that the relations between the structure of societies and the structure of individual biographies could be better studied. I suspect that the variance in age is going down in nearly all such rites.

Or a very interesting question suggested at various points is how much time different people spend engaged in social interaction, in a state of talk. Research on how much time people spend talking might explain the recurrent finding that people who do more of one kind of social activity also do more of others. Time spent talking has been studied in small-group situations in which someone was always talking. In many families and work places, not to speak of early morning commutations, very little talk goes on. I suspect that whether people have anything to say, their intelligence, education, cumulated analyzed experience, and degree to which they are interested in a great many things, are primary variables in determining how much they talk.

For those who believe that theory ought to stand in an authority relation to research, with theorists telling researchers what they should be doing, this book does not satisfy without considerable work by the reader. For those who demand that research be true and that theory be wise or interesting, this is a well-written book by a powerful mind. It would deserve to be immortalized for the following epigram alone: "Clearly the major labor-saving device benefiting the housewife in industrial societies is the contraceptive" (p. 32).

Arthur L. Stinchcombe

Johns Hopkins University

Soziologie der Organisation. By RENATE MAYNTZ. Hamburg: Rowohlt, 1963. Pp. 155.

This introduction to the study of large-scale or formal organization is published in the sociology series of *Rowohlts Deutsche Enzyklopädie,* which is similar to the Random House and Prentice-Hall series. Thus the proper criteria for evaluating this paperback are judiciousness and up-to-date information, apart from expository skill and readability. The book scores well on all these criteria. It would make good introductory reading for American undergraduates.

Since the book is one more example of the impact of American sociology abroad, its strategy of persuasion (or missionary quality) and its link with indigenous intellectual traditions deserve brief comment. The author sketches the historical reasons for the different treatment of large-scale organization in the United States, in Germany, and elsewhere in Europe; she regrets the absence of a comprehensive social history of organizations, but then proceeds to argue for the utility, nay, inevitability of organizational analysis on an ahistorical, highly formalized level. After outlining the social-System approach as the basis of organizational theory she deals, chapter by chapter, with the dimensions of goals, purpose and function, structure, membership, and adaptiveness. The author emphasizes that social-system analysis must take into account the organizational dualism of power and authority, conformity and consensus, opportunism and solidarity; organizations, she points out, should be conceptualized as more than systems of social roles and of institutionalized values, and the motives of compliance should be studied in addition to the organization's rationale of legitimacy. Despite her modifications of Parsons and Weber one might say that she tries to balance a Parsonian and a Weberian perspective. It is, of course, through Weber that the book relates to German intellectual history; this link is also apparent in the author's advocacy of a greater historico-political context for organization theory than has been typical of American studies.

The cosmopolitan quality—in both the conventional and the professional sense—of this introduction seems related to the fact that the author belongs to the first generation of German postwar sociologists; she began her academic studies in the United States when the period of re-education was not yet over. In the most successful cases the outcome was a comprehension of American (and British) society and social science without undue partisanship and a gain in international breadth, qualities that also distinguish Ralf Dahrendorf's *Industrie- und Betriebssoziologie* (1956), to which the present work is an intellectual sequel. On some of these implications the author herself has thrown light in her (self-) analysis of "The Visiting Fellow" (*American Sociological Review,* Vol. XXV, No. 5 [1960]).

GUENTHER ROTH

State University of New York
Stony Brook, New York

Organizations: Structure and Behavior. Edited by JOSEPH A. LITTERER. New York: John Wiley & Sons, 1963. Pp. viii+418.

While this is not the first book of reading to focus exclusively on the topic of organizations, nor, I suspect, will it be the last, it may very well be one of the better of its kind. Litterer employs a deceptively simple schema for organizing the forty-four selections included in this volume. Part I is devoted to the "formal organization" and includes selections from the writings of Barnard, Dale Gulick, Mooney, Urwick, and Weber. Part II takes up the "informal organization" with materials chosen from Gardner and Moore, Homans, Roy, Sayles, and Selznick. The effects of the formal organization on the informal are examined in Part III, which includes materials by Argyris, Walker and Guest, Whyte, and Worthy. Part IV deals with "organizational adaptation," which is broadly defined to include both variation in organizational structure and organizational innovation and growth. The selections in this section are taken from the writings of Anderson and Warkov, Harbison, Lawrence, Selznick, and Smigel. The first four sections having been devoted to organizational structure and process, it is only in the final section that attention is turned to the adjustments made by individual participants. Part V contains materials from Blau, Breed, Dalton, Gouldner, and Merton.

Litterer is a conscientious editor. The materials for the most part have been judiciously selected, reasonably organized, and carefully edited. The editor, relying heavily on Homans' analytical system as developed in *The Human Group,* also supplies useful, if unimaginative, introductions to each of the major sections.

Since the editor is not himself a sociologist, it is of interest to note that the work of sociologists is well represented—indeed, dominant—in all sections but the first. While it would be comforting to attribute the lack of sociological materials in Part I to the business bias of the editor, it is, unfortunately, an accurate reflection of the past inattention of sociologists to the formal structure. There are, however, some signs to indicate that this situation is being remedied. Perhaps, by the time the second edition is being readied. . . .

If the editor can be exonerated from this shortcoming, he does not so easily escape a second. Although the book purports to deal with all types of organizations, virtually every selection focuses upon either a business concern or a public administrative agency. Prisons, universities, voluntary associations, political parties, hospitals—even unions—get short shrift in this collection. The editor's arbitrary decision to limit attention to organizations pursuing certain types of goals renders the discussions of formal and informal structure, of adaptation and individual adjustment much neater—and less interesting—than would otherwise be the case.

W. RICHARD SCOTT

Stanford University

Psychology in Administration: A Research Orientation, Text with Integrated Readings. By TIMOTHY W. COSTELLO and SHELDON S. ZALKIND. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. vii+500. $8.50.

This is a successful text. The aim of the book is to bring to administrators and students of administration the findings of psychological research. It provides the same function for the sociologist interested in organizations by supplying him with an apparently up-to-date review of the relevant psychological literature, something he is quite apt to miss without such an integrated volume as this.

The readings in the book consist of full or abridged articles as well as a number of excerpts and summaries of research. These are all integrated, and contrasted where necessary, through clear discussions. A major strength of the text lies in the organization of the material. The book is divided into six parts, each dealing with a major psychological concept: perception, motivation, reactions to stress, learning, attitudes, and thinking. Concise accounts are provided of Maslow on need hierarchies, Skinner on learning, Festinger on cognitive dissonance, and McClelland on need for achievement. There are reports of research on such phenomena as: group versus individual risk-taking and problem-solving; the incidence of hypertension among executives versus non-executives; and the effects of success and failure on level of aspiration.

An attitude of moderation prevails throughout the book. Although the authors are quick to point out the pitfalls for an administrator who relies only on common-sense notions about human behavior, they are also cautious about the contributions of psychology for administrative behavior. For example: "Although even now industry is beginning to use both tests and biographical inventories to select creative individuals, it is accurate to state that for business use the best (and until further research is done, the only) means of assessing creativity is past performance that was creative in comparable circumstances."

Caveats sprinkled throughout the book are a welcome addition to the field. Many researchers in the administrative and industrial area are quick to make exaggerated claims for the amount of firmly established knowledge of organizational behavior possessed by behavioral scientists. Some are even now approaching industry for funds to set up a center to study how to change organizations by the application of these "scientific findings."

The sociologist who must deal with administrators for financial support or for access to data must know something of psychological research. The administrators look upon their own organization as a collection of personalities. They attempt to reduce administrative and organizational

problems to psychological dimensions. Their solution to any problem is to have the "right person in the right job." Everything else will take care of itself.

The sociologist, to justify his approach to administrators and executives, must at least be familiar with the claims made for psychologically oriented research and action. This book enables him to be so.

FRED H. GOLDNER

Graduate School of Business
Columbia University

Coal and Conflict: A Study of Industrial Relations at Collieries. By W. H. SCOTT, ENID MUMFORD, L. C. MCGIVERING, and J. M. KIRKBY. Liverpool: Liverpool University Press, 1963. Pp. 214. 25s.

This research report has several admirable characteristics. First, a comparative research design was used: two coal collieries, each consisting of two pits, were studied and the entire range of occupational categories in the collieries was sampled. Second, a variety of data-collecting procedures was utilized: discussions were held with key persons at all levels, documentary records were analyzed, work activity and meetings were observed, and a cross-section of the work force was interviewed. Third, the research was conducted within a general theoretical framework.

Although the authors discuss traditional concepts of industrial sociology such as satisfaction, morale, and conflict, they view these phenomena within the context of a formal organization. Thus, they make the significant distinction between organized and unorganized conflict. Organized conflict is a group phenomenon: there are disputes, and negotiating and grievance procedures can deal with them. Unorganized conflict is relatively indirect and personal and expressed in the form of labor turnover, absenteeism, poor timekeeping, and negative attitudes; these manifestations are usually not subject to containment or resolution by established procedures. Presumably the alternative means of expression of conflict reflect both the kind of dissatisfaction and the bargaining power or ability of some group of workers to establish and utilize formalized bargaining procedures.

On the whole, the findings support this view. Thus, members of the lower-status occupational categories tend to be dissatisfied with their rewards; they have low morale and rate relatively high on measures indicating unorganized conflict. Members of the high-status occupational categories are relatively satisfied and have high morale and utilize the established grievance procedures. The authors, of course, do discuss the many ways in which these variables interrelate and are affected by technology, age composition of the occupational categories, and other factors.

The concepts are not rigorously defined, and the measures for some of the concepts are not the best that can be imagined. Nevertheless, the general orientation of the research is excellent, and the discussion is rich with suggestions.

LOUIS KRIESBERG

Syracuse University

Changing the Structure and Functioning of an Organization: Report of a Field Experiment. By STANLEY E. SEASHORE and DAVID G. BOWERS. (Survey Research Center Monograph No. 53). Ann Arbor: Institute for Social Research, University of Michigan, 1963. Pp. iii+113.

The field experiment reported in this book was conducted over a three-year period in an 800-man manufacturing plant of a medium-sized packaging and printing firm. The experiment was designed to test whether "an organization is likely to achieve its purposes better: (1) if there is an emphasis on the work group, rather than exclusively on the individual, as the unit is supervised, (2) if there is a high rate of interaction and mutual influence among work group members, (3) if there is a high degree of participation in decision making and control activities in the lower echelons of the organization, and (4) if supervisors provide to subordinates a high degree of supportiveness." In general the findings support this hypothesis, although the authors point out that the evidence is not conclusive.

The authors are to be strongly commended for having carried out two steps that are only too rare in current organizational be-

havior research: they not only conducted a large-scale field test of an explicit theory of management (the theory associated with Likert's work) but they also wrote a candid description of what happened—including the evidence of considerable loss of control over their experimental conditions. The report amply demonstrates the considerable problems of conducting field experiments of this magnitude. They found at one stage that the "control" groups were adopting some aspects of the experimental treatment of their own volition. Many changes were introduced into the plant beyond the control of the experimenters. At one point a combination of adverse economic facts and internal organizational confusions lead the researchers to withdraw from active participation in the experimental program. In spite of these problems that in some form are probably inevitable in any large field experiment, the study was carried through to a conclusion that succeeded in partially testing the theory.

The study served to reinforce the authors' belief that the theory of "participative management" they tested is a partial theory "to apply within the general framework of the classical line-and-staff, hierarchical organization." It appears, however, that they missed an opportunity to use their data to push further our understanding of this matter of the relation between organizational structure in the classical sense and their modified theory of managerial methods. For example, they state that some "organizational revisions" initiated by a new company president removed "the conditions which had been diagnosed as insuperable blocks to the experimental change program." This suggests that some structural changes were a necessary precondition to changes in interpersonal relations among managers, but no adequate description or analysis is provided. This aspect of the study provides additional evidence that we are at that stage of inquiry into organizational behavior where we must abandon the catch-all hypotheses about *all* organizations and focus instead on the study of the various conditions, environmental, structural, technological, etc., under which different methods of management produce predictable outcomes.

It is certainly to be hoped that the difficulties of these field experimenters will in no way discourage others from following their excellent examples in the choice of research method and reporting style.

PAUL R. LAWRENCE

Harvard Business School

Lawyers on Their Own: A Study of Individual Practitioners in Chicago. By JEROME E. CARLIN. New Brunswick, N.J.: Rutgers University Press, 1962. Pp. x+234. $6.00.

This volume, by a sociologist-lawyer, is presented as partly "a study in the sociology of the professions" and partly "a report to the public and to the legal profession itself on what actually is going on in the practice of law." More concretely it presents a study of ninety-three (from a sample of one hundred) lawyers in Chicago who list themselves as solo practitioners. It describes their solo practice of law from the perspectives of several related problems: recruitment and selection, getting and keeping business, ethical dilemmas, and work satisfaction. Generally each problem is discussed in terms of a number of variables, including the social backgrounds and motives of the lawyers, the kinds and professional levels of the law work, relations to other components of the legal institution (including law firms and lawyers' organizations), and contacts with the larger community (including ethnic subcommunities). The author concludes that "today the individual practitioner of law . . . is most likely to be found at the margin of his profession, enjoying little freedom in choice of clients, type of work or conditions of practice," and that this state of affairs raises the serious question "of the individual practitioner's ability either to practice law in the traditional sense or to live up to the ethical norms of the profession" (p. 206).

The style of this easily read book is highly reminiscent of some earlier published Ph.D. dissertations done at Chicago; it follows closely in the tradition of such works as *Taxi-Dance Hall* and *Gold Coast and the Slum*. There are, as in these earlier works, several questions concerning both its methodological and theoretical sufficiencies. Many of these questions have been raised by other reviewers. Fortunately the author has not

rested on his laurels. He has proceeded on to a much larger and more systematic study of the metropolitan bar in New York City that is providing substantial corroboration of much that is only suggested in this study. Thus it may be unfortunate that this little book is available only in a relatively expensive, hard-cover edition, for it could be valuable collateral reading in a variety of academic courses.

Finally, special note must be taken of the extent to which this study and its successor, limited to metropolitan settings, are stimulating and facilitating other related studies by sociologists (K. Reichstein, for example) and lawyers (J. Handler, for example). Although it was itself stimulated by earlier studies by Lortie and Hale, greater availability has enhanced the impact of this work. For this reason alone it should be in the library of all sociologists with special interests in the study of social control, societal integration, or the professions.

HARRY V. BALL

University of Wisconsin, Madison

New Estimates of Fertility and Population in the United States. By ANSLEY J. COALE and MELVIN ZELNIK. Princeton, N.J.: Princeton University Press, 1963. Pp. xvi+186. $4.00.

Coale and Zelnik have provided us with what will probably be the standard figures on births and population for the United States from 1855 to 1960. They have made effective use of the Censuses and vital statistics at hand; they seem to have used good judgment on what to accept and what to dismiss; they have made many bold corrections of the official material.

Their general method was to link the births of a given year with the same cohort as subsequently enumerated in successive Censuses. Since over most of the period births were available for few states if any, this linkage of births and enumerations was most useful when operated in reverse: the fact that there were 670,000 native white males aged 15 and 648,000 such females in the United States in 1900 implies a certain number of white births about the year 1885. Since the native-born did not emigrate in large numbers and, by definition, could rarely be immigrants, they constitute a closed population, and the main item of information needed to infer the births from which they were generated was a suitable life-table. This was contrived from data on the registration area of the United States and from European countries of similar levels of sanitation. The probability of a boy born in 1885 living fifteen years was worked out to be 0.752; that for a girl 0.772. Taking the previously cited figures for age 15 in the 1900 Census, we have 670,000/0.752 + 648,000/0.772 = 1,740,000, which is the estimate shown on page 22 for the white births in 1885. The same calculation may be made from the boys and girls aged 5 in 1890, along with the probabilities of living five years from 1885, and we find 1,741,000 as the 1885 births. Unfortunately this reviewer was not able to reconstruct other figures in the book as well as the 1885 births, either because the calculations made were not always specified clearly enough to be reproduced, or because he is not a sufficiently alert reader to follow the explanations.

The series of Censuses gave a corresponding series of estimates for the native white births of each year and among these estimates, say, for 1885, the highest one was selected. It was found, for instance, that a cohort of women was reported most completely when it passed through its twenties; births inferred from younger or older ages in respect of a given year were lower. To these highest figures were added further allowances for underenumeration in the Census in which they were given best. Once such a series of births was secured a forward projection could be made, in which the numbers in the several cohorts were brought forward to the several Census dates. This puts all the Censuses on a level with the Census in which the given cohort was enumerated most completely.

Some substantive results appear. The birth rate of the United States was over 50 per thousand in 1830 when that of Sweden and France was about 30; the fall of births in the United States and France did not seem to be related to industrialization, but preceded it; up to 1880 the birth rate of the United States dropped more rapidly than that of England, which was much more industrialized. Earlier child-bearing and a more favorable age structure were part of what made the United States crude birth rate high in relation to

Europe through most of the nineteenth century; by the end of the century the United States had a lower age-specific fertility than any European country except France.

While I say again how useful this book will be to students of the demographic history of the United States, my congratulations to the authors are tempered by the difficulty I have had in trying to reproduce their figures and by the extreme carelessness in some of the mathematical exposition, especially the argument on page 83.

NATHAN KEYFITZ

University of Chicago

Medieval People. By EILEEN POWER. 10th ed. London: Methuen & Co.; New York: Barnes & Noble, 1963. Pp. xii+210. $4.50.

Eileen Power excelled in shrewd and humorous description of the kind of human relationships that interest sociologists. Indeed, her inaugural lecture as professor at the London School of Economics was essentially a plea to historians to make better use of sociological theory and to contribute to its development. This new edition of one of her early books, which in the forty years since it first appeared has captivated hundreds of thousands of readers, opens with a hitherto unprinted chapter that she had planned to add to it. Entitled "The Precursors," it sketches typical members of the comfortably rich who lived through the decline of Roman civilization without grasping the direction of their time. Today's students will perhaps not recognize in it the shadows of the year 1938, when it was written, but they will enjoy the grace and wit with which it challenges materialistic complacency. The six original narrative portraits that follow—of a ninth-century peasant family in France, of Marco Polo, of Chaucer's worldly Prioress, on an aging fourteenth-century Parisian bourgeois with a teen-age wife, and of a merchant and a clothier in fifteenth-century England—are increasingly rich in background. The last two, though as gay as the rest, still stand as solid contributions to economic history. No sociologist who would like to know something of the norms of economic activity in pre-Protestant culture can afford to overlook this delightful book.

SYLVIA L. THRUPP

University of Michigan

Politics and Social Change, Orissa in 1959. By F. G. BAILEY. Berkeley: University of California Press, 1963. Pp. xiii+234. $5.00.

In this his third book on India, Professor Bailey sets out to answer the question: What is the relationship between parliamentary democracy in Orissa and the older traditional forms of social and political organization?

In Part I (two chapters) Bailey examines voting behavior in two Orissa villages. The first one, Bisipara, a small, isolated village, was Bailey's home during 1952–54, and he gives us a rare view into the political outlook of his former neighbors. Three nearly verbatim texts describe how the candidates and issues of the 1957 elections appeared to three citizens of Bisipara. The critical issues that generated preference for the Ganatantra party were related to the local traditional feudal hierarchy and to local caste sentiments.

The second village, Mohanpur, is located only a 10-minute bus ride from the center of the large city of Cuttack. Unlike the campaign in Bisipara, the 1957 campaign in Mohanpur did not develop along the lines of internal cleavage in the village. Nor were the larger interests of state politics translated into local rivalries as they were in Bisipara. The Congress party won in Mohanpur partly because the control of such controversial local institutions as the school committee and festival committee had been removed from the realm of state politics and so did not become an issue, and partly because by 1957 voting for the Congress party had become a habit.

How has it been possible for this habit to develop in Mohanpur, when it has not developed in Bisipara? Bailey suggests that it has developed at least in part because of structural factors that have little to do with party platforms or personalities. "The relatively low degree of parochialism in Mohanpur is an index of political maturity—measuring maturity from the standards of parliamentary democracy—and of the degree to which Mohanpur is integrated into the larger society of Orissa. In Bisipara the reverse position holds. The village has a high degree of internal integration and is very little connected with the world outside; and representative politics have not yet achieved any considerable legitimacy, as things which exist in their own right and not merely as one of several weapons used to achieve dominance in the village"

(p. 86). The degree to which Mohanpur is integrated into the larger society of Orissa is also reflected by the presence in Mohanpur of a large number of part-time political "brokers"—men with their own contacts with the administration and the world of commerce. This contrasts sharply with Bisipara, where there is a cleavage between the village and the administration and only a few full-time political "brokers" link the two.

In Part II (four chapters) Bailey examines Oriya political behavior from the standpoint of the politicians. Because of the relatively small voter turnout and the large number of candidates standing for election, a politician courting a group with as few as a thousand votes is not wasting his time, for those votes may represent the margin necessary for his victory. At times while they were electioneering in 1957, the politicians approached the newer men of influence—schoolmasters or successful businessmen—who might control vote banks. At other times they approached the men of influence in the traditional feudal political units or in caste assemblies, where there might be accessible vote banks. Bailey suggests, however, that the traditional feudal vote banks will disappear rapidly as the new generation grows up, and the caste assembly vote banks will be of little influence until they are converted into caste associations. Even where caste associations have been organized, Bailey finds no evidence yet for their being systematically used by candidates to promote an election. Nonetheless he predicts that "caste in the form of associations . . . may become, for a time, a main organizing factor and a main cleavage in the new political system" (p. 134).

In the absence of ready-made social groups coinciding with the boundaries of constituencies candidates seeking election are forced to create new groups through which they can reach the public. Such new groups include political parties. Bailey draws the distinction between a political "machine" and a political "movement." Individuals work for a "machine" because they expect some tangible reward for their services. Individuals work for a "movement" because they are in the habit of doing so or because they are morally (without calculation of immediate personal gain) convinced of the rightness of their party's position. Bailey found no genuine political "movements" in Orissa. Where political "machines" existed, they were able to stick up posters, spread rumors, and engage in other types of political behavior. However, at the same time they were hobbled by their poverty and their parochialism. Bailey concludes that the multitude of defactor political "machines" may actually impede the development of morally united political "movements." A step required for making the shift would be to professionalize the role of "broker" and to establish career administrators within the party.

Part III (three chapters) deals with Orissa State politics. Bailey traces the historical relationships between the hill people and the coastal people, as well as the relationships between the Utkal Union Conference (concerned with obtaining a separate Oriya state, encouraging the use of the Oriya language in schools, courts, etc.) and the Orissa branch of the Indian National Congress Party, working for all-India independence. Bailey's description of the state-union differences prior to independence is a good antidote for any who feel that regional loyalties in India are a post-independence phenomenon. Bailey notes that in order to win elections the Congress party in Orissa has increasingly selected candidates with non-Congress or even anti-Congress backgrounds. Hence the 1959 dissolution of the Congress ministry and the establishment of a coalition government were merely continuations of a trend already under way.

In his conclusions Bailey draws a series of distinctions—between "arena" and "group," "elites" and "masses." He concludes that the constituency is a meeting place between the village and elite areanas—"the elite arena and the village arena are for their respective protagonists, ways of life, the constituency is merely an instrument for the preservation of those ways of life" (p. 234). Representative politics force both the elites and the villagers to lose their "parochial" qualities, in this way representative politics provide a direction for social change.

This reader has only two questions to raise regarding Bailey's book. The first has to do with his treatment of caste. ". . . there are good grounds for assuming that castes (in their present definition of endogamous groups) must always have been small. . . . Among the peasants cognatic kinsmen for the most part live within a day's walk—roughly fifteen miles at the most" (p. 126). A consideration of caste in this manner runs into operational diffi-

culties, for then the castes of kinsmen fifteen miles apart become two different (but partially overlapping) structures; technically they are not members of the *same* caste. If we use Bailey's formulation, then the question of whether villagers *x*, *y*, and *z* are or are not all members of the same caste depends on whether we are looking at caste from *x*'s, *y*'s, or *z*'s perspective. Is Bailey's formulation an improvement upon the more general "caste" and "subcaste" distinctions?

The second question has to do with Bailey's categorizations of political parties. He begins chapter vi with a series of analytic distinctions—between "python" parties and "representation" parties. Within the "representation" parties he further distinguishes between those organized as "machines" and those organized as "movements." However, having done this, he does not remain consistent with his own definitions, and comes up with such difficult-to-interpret statements as "Congress ceased to be a movement and became a party" (p. 206). Chapter ix suffers especially from inconsistent use of key terms. This in turn weakens his concluding chapter, where he attempts to provide a single conceptual framework for the three parts of his book.

These are, however, but minor weaknesses in an otherwise valuable book. Bailey is at his best when dealing with the people of Bisipara. The analytic distinctions he uses in comparing Bisipara with Mohanpur allow for meaningful comparisons with political activities in other parts of India as well as in other newly democraticized nations.

JOSEPH W. ELDER

University of Wisconsin

International Stratification and Underdeveloped Countries. By GUSTAVO LAGOS. Chapel Hill: University of North Carolina Press, 1963. Pp. xviii+302. $7.50.

This is a truly pioneering work. In the third line of the book, Professor Lagos refers to "the problem of human inequality, of social stratification—to use a concept of sociological theory." The whole volume is characterized by a constant interweaving of sociological theory and social analysis, by a thorough awareness of the societal issues behind the conceptual variables. Marx and Weber, Parsons and Merton, all are marching hand-in-hand.

The units under analysis are nations and classes in context of the world, viewed as "a great social system composed of different groups interacting and . . . these national groups occupy various positions within the social system. These positions can be ranked in terms of economic stature, power, and prestige, and they constitute the status of the nation." Such a macroscopic approach is pure poetry to the ear of a sociologist anxious to see the concepts and tools developed in the study of small groups of Sophomores applied to the study of larger units and greater problems.

Of course, we have had in the past writings that deal with civilization, life-cycles of societies, and the universe. But unlike these largely pre-empirical works, Lagos' is—bless his heart—concerned with facts. He uses such statisics as gross national product, population, word distribution of income, defense budgets, and the like to establish the relative status of nations, in terms of wealth and income and military power. Prestige is determined qualitatively.

Such enterprises are far too few, and the reviewer is much tempted to stop here on a congratulatory note. But enthusiasm for this approach cannot blind us to the other aspects which reveal the trailblazing quality of this work. It might be too early to expect major new insights or validated propositions to come from such works. At least Lagos does not provide many. Although a great deal of time is spent placing various nations on various scales, the Lagos model yields comparatively little. The potentially promising line of examining the consequences of status inconsistency, of a country rich but weak, poor but honest, etc., is not sufficiently developed. The observations Lagos makes are no better than a first-rate journalist would make without benefit of such a model, and many of them are quite a bit less sophisticated. The titles of two of Lagos' headings illustrate the somewhat flat nature of his observations: "A Policy of Prestige Corresponding to the Real Status of the Nation: The Case of India" and "A Policy of Prestige Not Conforming to the Real Status of the Nation: The Case of Peronist Argentina" ("real status" refers to the position of a nation in the international stratification sys-

tem, as compared to the formal, legalistic one according to which all independent nations are equal).

The basic problem seems to be that Lagos seeks to explain concrete policies by a small number of analytical variables, largely excluding domestic processes. While the ultimate test of a theory is such an application, the model which Lagos has begun so effectively to construct cannot yet carry such a load. Lagos' discussion of a model for economic development and its sociological concomitants again does not go much beyond that widely held in the development literature before the Lagos model was constructed.

Nor is Lagos as consistent in application of his terms as he ought to be. While he starts on a Weberian note, offering three independent sources of status, he ends on a more Marxist note, implying that "in the long run" the economic status determines the two others. This is unfortunate because it deprives his model of its major dynamic force, the consideration of the strains resulting from status inconsistency.

Still, the student of the sociology of international relations, a young field, can hardly afford to skip this book; the aim is inspiring, the basic approach valid, and the task—immense.

AMITAI ETZIONI

Columbia University

Brazil on the Move. By JOHN DOS PASSOS. Garden City, N.Y.: Doubeday & Co., 1963. Pp. 205. $3.95.

Brazil: People and Institutions. By T. LYNN SMITH. Rev. ed. Baton Rouge: Louisiana State University Press, 1963. Pp. xx+667. $12.50.

These two books on Brazil both appeared in the fall of 1963 and represent high competence in journalism and sociology respectively. Dos Passos gives us a travelogue of his three visits there in 1948, 1956, and 1962, the important people he met, and the good or harm he thought they were doing the country. Lynn Smith provides us with an even greater bulk than in the earlier edition of statistical and other factual data on the population, its agricultural practices, and its marriage, educational, religious, and other institutions. The two have in common their references to the tolerance of Brazil for the alien in religion, in race, and in culture; the difference between Brazil and Spanish America; the vast energy of contemporary urban Brazil and the listlessness of so much of the countryside.

Lynn Smith has drawn not only on his own observation of Brazil, but on writers of the present and the past. The works of foreign visitors—Richard F. Burton, Roger Bastide, Giorgio Mortara—and of local writers—Josué de Castro, Gilberto Freyre, Teixeira de Freitas—have been combed to insure that whatever genuine knowledge of Brazil is to be had will be included somewhere in his pages. If the result is a book that few will read from beginning to end, it is also one which no one interested in Brazil can avoid examining with some care.

The only point on which John Dos Passos is better than Lynn Smith is in giving the flavor of the Vargas dictatorship and some of the governments subsequent to it. For Lynn Smith, Vargas was notable chiefly for having changed the constitution in the direction of more power to the central government. Vargas made ample use of the South American practice of appointing intervenors in place of the governors of the provinces, and of having each of these control the province as firmly as the force at his disposal permitted. But in fact he represented much more than centralism. Dos Passos shows us how, like Peron in Argentina, Vargas was a mighty agent of social change. He rested his drive for power on the urban masses rather than the oligarchy, and with every measure which he took to climb to the top and then to stay there, he left an imprint on political and social life. Vargas was demagogy combined with force, intrigue, and even some elements of progressive social legislation. To speak of him as being left or right makes no sense; at times he resembled Stalin, at other times Mussolini, and when the Axis seemed to be losing the war he tried to identify himself with Franklin Roosevelt. He was thoroughly totalitarian in his intolerance of opposition; but evidently Brazilian society, more open than that of Europe, could not be unified and co-ordinated in the Estado Novo. In particular, the Army and the Navy remained in opposition to one another and, ultimately, to Vargas. But the deception worked by Vargas, his lying appeals to that hunger for

participation in the national life which seems to be the driving force in Latin America today, his dealings with the extreme left and his persistent confusing of all political ideologies have set a pattern which has by no means been eradicated.

NATHAN KEYFITZ

University of Chicago

Women of Tropical Africa. Edited by DENISE PAULME. Berkeley: University of California Press. 1963. Pp. ix+308. $6.50.

Women of Tropical Africa is an English translation of *Femmes D'Afrique Noir,* which appeared in 1960. The translator, H. M. Wright, has done a careful rendering into English while maintaining something of the style of the original essays.

Denise Paulme, the editor, has contributed a general introduction to the book. It is followed by six extremely good accounts of the role of women in African society, all of them written by women anthropologists. Five of the articles are detailed descriptions of the life of women in five societies of French-speaking Africa: Coniagui, pastoral Fulani, Nzakara, Urundi, and the urban areas of Dakar. The sixth article is based on published accounts from various parts of Africa and discusses the role of women in political life. A final section is devoted to a useful analytical bibliography organized both by topic and by geographical region.

Women of Tropical Africa is a splendid contribution not only to the literature on Africa but also to the understanding of the role of women in non-industrialized societies. Each essay deals with women in their everyday life and with the problems that most particularly concern them. African women turn out to be far from the passive figures content to accept the guidance and domination of their men that some accounts suggest. The formal rules of the society may subordinate women, but they have their own sphere of action in which they show a considerable degree of independence and from which they occasionally emerge to interest themselves in a wider field of action. Although their activities may take place mainly within the context of domestic groups, this poses for them problems of adaptation which are perhaps greater than those faced by men. Perhaps the most striking thing that emerges from the descriptive articles is the way in which the women feel themselves aliens in their marriages and the degree to which they manage to cling to their own kin throughout the course of such marriages.

This book is a most welcome addition to the literature on Africa, on the position of women, and on the nature of family life.

E. COLSON

Northwestern University

Handbook of Medical Sociology. Edited by HOWARD E. FREEMAN, SOL LEVINE, and LEO G. REEDER. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. xvi+602. $11.00 (trade); $8.25 (text).

This is an excellent book which does credit to the editors, the contributors who prepared its twenty chapters, and the field of medical sociology. With its publication, this young and blossoming, but often anomalous, sub-speciality is seen in a new perspective of form, purpose and maturation.

The editors have been both insightful and fortunate in their selection of contributors The authors have maintained a noteworthy level of quality while preparing concise but thorough chapters on many topics which might more easily have been handled in the context of a monograph. Especially impressive is the success with which the book through many of its chapters maintains an historical perspective, provides excellent reviews of existing literature, amplifies or extends existing knowledge, and introduces useful and meaningful conceptualization. Each chapter includes a valuable and definitive set of references and the final chapter is a useful brief bibliography, complied by Ozzie G. Simmons.

The stage is set by three physicians who are respected social scientists (or should we say three social scientists who happen also to be physicians). Hugh R. Leavell provides an introductory note on the emergence of a social-science health field. George G. Reader is both generous and thoughtful in his analysis of the nature and categorization of sociological contributions to medicine. George Rosen in his scholarly review of the evolution of social medicine provides an important

perspective which will enhance the education of medical sociologists.

Four chapters on the sociology of illness effectively summarize knowledge and raise questions. These are devoted to the chronic illnesses, by Saxon Graham; social psychological factors in illness, by Stanley H. King; the addictive diseases, by Edward A. Suchman; and mental disease, by John A. Clausen.

A section on practitioners and patients includes an analysis of medical education by Howard S. Becker and Blanche Geer, a chapter on nursing and other health professions by Ronald G. Corwin and Marvin J. Taves, a fine review of the roles and status of marginal and quasi-practitioners by Walter F. Wardwell, and thoughtful essays on interpersonal relations in medical settings and patient-practitioner relations prepared, respectively, by Sydney H. Croog and Robert N. Wilson.

As elsewhere in the volume, chapters on the sociology of medical care reflect the editors' appropriate selection of topics and their ability to prevail on the right man to prepare each chapter. Eliot Freidson covers the organization of medical practice, Sol Levine and Paul E. White consider community health organizations, Odin W. Anderson reports on the utilization of health services, Irwin T. Sanders on public health in the community, and Steven Polgar reviews cross-cultural health action.

Finally, sociomedical research is considered in terms of strategy by Richard H. Williams and methodology by Jack Elinson, and the editors review the current strategy of medical sociology.

This is a volume of substance and authority which belongs on the basic shelf of every medical sociologist. It is also recommended as a text and as an excellent reference for interested physicians and for social scientists in general.

ROBERT STRAUS

University of Kentucky Medical Center

Hospital and Medical Economics. 2 Vols. By WALTER J. McNERNEY and STUDY STAFF, the University of Michigan. Chicago: Hospital Research and Educational Trust, 1962. Pp xii+1,492. $20.00.

In 1957, following a series of rate increases granted to Michigan Blue Cross, a Governor's study commission asked the University of Michigan to conduct a thorough study of hospital and medical economics. Financial support (close to $400,000) was received from the Kellogg Foundation and a study group under the direction of Walter J. McNerney was given a free hand to design and execute the research. The present massive report, appearing only four years after work was started, is a tribute to the organizational and research skills of the study director.

Where many would have focused on a particular aspect of the problem or have relied upon a particular research technique McNerney surrounded himself with a large and interdisciplinary study staff, conceptualized the problem in its broadest terms, marked out the major areas, designed specific projects to throw light on these areas, invented a variety of ingenious research methods to collect and analyze his data, and here presents his findings with due regard for whatever limitations attach to the data but without sidestepping his obligation to draw conclusions and make recommendations

The major projects reported on are studies of family health care and its financing, by means of a personal-interview household survey; of the character of hospital use through analysis of patient discharge records, and its "effectiveness" through comparison of actual medical records of a sample of cases with objective criteria for admission and discharge as determined by panels of physicians; and of changing patterns of hospital care through comparison of 1938 records with those of 1958. There is also an inventory of health man-power; an analysis of hospital facilities and costs; a study of accounting and fiscal controls; and several projects concerned with the role of prepayment plans and of government.

As the senior author states in his foreword, "The report is not cast in a traditional mold It is, at once, a book, a series of monographs, a research manual, and a useful blueprint for action." This approach has its advantages, of course, but it also has its disadvantages. It is doubtful that anyone will sit down and read the 537 statistical tables and 1,492 pages of double-column type as "a book"; and to serve as "a useful blueprint for action," the

work would benefit by either a general summary of findings and recommendations or abridgement to two or three hundred pages. As a series of research reports, however, *Hospital and Medical Economics* meets the highest standards, and it is certain that the data reported therein will be cited for many years to come.

PAUL B. SHEATSLEY

National Opinion Research Center
University of Chicago

The Hospital in Modern Society. Edited by ELIOT FREIDSON. New York: Free Press of Glencoe, 1963. Pp. xv+346. $6.95.

Six blind men examining an elephant did not, so the story goes, emerge with identical or even consistent images of the beast under examination. Just so, seventeen sociologists have not in this collection of previously unpublished articles provided us with a clear or consistent view of the hospital as a type of social organization. The two groups fail for somewhat similar reasons. Both are unsuccessful, in part, because no two members examine the same aspect of the phenomenon under investigation. Elling, for example, examines the feet (support systems), Perrow, the head (goal-setters), and Goss, the skeleton (authority structure). Both groups also fail because of difficulties with their vision: although none of the sociologists is blind, each chooses a focus or perspective different from that of the other contributors. Thus, Rosen utilizes a historical approach, Glaser a cross-cultural one, Elling an ecological perspective; Goss examines internal structure while Sommer and Dewar focus on the hospital ward as a physical environment. The sociologists have an additional handicap. While the blind men were content to examine a single kind of animal, the contributors to this volume have ranged across an entire species. The types of hospitals investigated include voluntary general (Elling, Perrow), university (Goss), obstetrical (Rosengren and Devault), tuberculosis (Roth), custodial and rehabilitation (Coser), and psychiatric (Strauss *et al.*, Sommer and Dewar). In addition, Rosen examines some of the important historical forms, Glaser contrasts United States with foreign, and Kendall's survey includes both voluntary general and university-affiliated hospitals. In short, because of the variety of problems examined, the diversity of perspectives employed, and the range of hospital types investigated, no clear conception emerges as to the nature of "the hospital in modern society." Further, the articles assembled here are such that each selection contributes but little to an understanding of the others and benefits but little from their presence.

If attention is turned to the individual articles, four of the eleven selections seem particularly noteworthy. However, two of these—Perrow's discussion of the relation between controlling elites and types of goals and Goss's analysis of authority relations among physicians in an outpatient clinic—add little to the authors' earlier published reports on these same topics. The third article of interest is Glaser's acount of his cursory investigation of hospital structure in countries other than our own. Noting that "the American facts and reasons have been stated as if they were inherent in hospital organization," Glaser reports such findings from his foreign survey as that, in most countries, the administrative tasks are performed by clinicians; in many countries the medical staff organization assumes the form of a clear-cut bureaucratic hierarchy; and in some countries, nurses are actually contented with their situation and with their position in the structure. That such ideas sound almost heretical is an indication of the importance of cross-cultural research in the study of specific organizational forms.

A fourth article deserving special mention is that by Rose Coser, in which she contrasts a rehabilitation with a custodial hospital. In the latter setting, the goal of the organization has been scaled down from curing the sick to merely caring for them and staff members are found to adapt collectively to these circumstances with ritualistic and retreatist behavior. Coser argues that such adaptations have important consequences both for the organization (e.g., reduction of interprofessional controls) and for the individual (increased alienation from work and self).

W. RICHARD SCOTT

Stanford University

The Doctor and His Patient. By SAMUEL W. BLOOM. New York: Russell Sage Foundation, 1963. Pp. 270. $4.50.

This book developed principally out of the author's experiences teaching, "on a graduate but introductory level, basic concepts and theories of sociology" to medical students in the Baylor University College of Medicine. In the opinion of this reviewer, it is the best book available attempting "to describe a sociological approach to the study of human behavior in terms that are relevant to medicine" (p. 49). It is a systematic text rather than a survey of the field and should, consequently, be of great value to all who are engaged in teaching sociology to health personnel.

The body of the book is divided into four parts. Part 1, "The Frame of Reference," introduces basic sociological concepts, describes three basic models of the doctor-patient relationship, presents a description and preparatory analysis of a case illustration, and suggests a graphic model of the doctor-patient relationship conceived as a social system. This model emphasizes that the doctor-patient relationship is not isolated. The doctor's behavior is significantly regulated by the medical profession; the patient's behavior is significantly regulated by his family; while all of these system components—doctor, patient, medical profession, and family—are imbedded in a sociocultural matrix. Part 2, "Social Roles and Their Institutional Context," flows logically from the conceptual scheme of Part 1. Social organization of the medical profession, social role of the physician, structure of the sick role, and social organization of the family are discussed in turn. In each instance, the reader's understanding is aided by pertinent historical and cross-cultural comparisons. Changes which have led to development of the hospital as "the major social institution of modern medicine" are explored, paving the way for Part 3, "The Hospital as a Social Institution." In successive chapters the student is introduced to a sociological interpretation of the development of modern hospitals, the organization of general hospitals, and the organization of mental hospitals. A crucial discussion (chap. ix) presents sociological theories and research findings relating social structure in the mental hospital to the conduct and course of patients' illnesses. In Part 4, "The Frame of Reference at Work," the case illustration introduced in chapter i, focused on culture and subculture, is considered again in terms of the more fully developed conceptual scheme. A new case illustration, focused on social roles and systems, is also introduced. Finally a full interactional system focused on the doctor-patient relationship is sketched.

Several features of this book deserve special praise. First, it is well organized. Careful attention has been given to the relationship of each chapter to every other chapter and to the guiding frame of reference. Second, Bloom has in many instances clearly shown the connecting links between social-psychological and sociocultural phenomena and successes and failures in the treatment process. He has not been content simply to repeat the axiom that cultural and subcultural forces are related to illness. He insists that these variables must be refined if they are to become therapeutic instruments. Finally, he has introduced concepts sparingly as they became necessary to the exposition.

The Doctor and His Patient performs heuristic as well as educational services. Bloom's ordered and generally symmetrical presentation suggests to this reviewer the need for a discussion of research findings, similar to that of chapter ix, following chapter vii, "The General Hospital as a Human Organization." The author's inclusion of a chapter closely connecting *mental* hospital social structure with patient welfare and his exclusion of a chapter of the same type concerning the general hospital may partially reflect the psychiatric setting from which the book originated. It seems more probable, however, that in this most unmistakably sociological arena too little existing work convincingly documents direct effects of hospital social structure on the doctor-patient relationship. In any event, *The Doctor and His Patient* provides a framework which should, as the author hopes, facilitate "both education and research in the social science of medicine."

S. DALE MCLEMORE

University of Texas

Man's Image in Medicine and Anthropology. Edited by IAGO GALDSTON. New York: International Universities Press, Inc., 1963. Pp. xvii+525. $10.00.

All but one of the fifteen papers in this volume were prepared for a conference on medicine and anthropology held at Arden House in 1961. The exception is a paper by John C. Whitehorn, "The Doctor's Image of Man," which was chosen to stand as a "thematic introduction" to the book. The other fourteen papers are grouped into four parts. The first part, "Medicine and Primitive Man," includes an excellent paper by Charles C. Hughes entitled "Public Health in Nonliterate Societies," which reviews much of the anthropological research in this field and constitutes a welcome addition to the review articles on anthropology and public health, some rather more comprehensive, already available in the literature.

The second major part of the book, "Medical Man and Medicine Man in Three North American Indian Societies," consists of three reports of empirical studies. The first is by John Adair on "Physicians, Medicine Men and Their Navaho Patients," and considers one set of findings from the Cornell-Navajo Field Health Research Project; the second is A. Irving Hallowell's "Ojibwa World View and Disease"; and the third is by Robert Ritzenthaler, entitled "Primitive Therapeutic Practices among the Wisconsin Chippewa."

The third part, "Psychiatric Medicine Cross-culturally Viewed," contains papers by George Devereux, William Caudill and L. Takeo Doi, Edward T. Hall, and Marvin K. Opler. These papers are concerned with primitive psychiatric diagnosis, interrelations of psychiatry and culture in Japan, proxemics (the study of man's spatial relations), and cultural definitions of mental illness in Ghana. The last part of the book, "Culture and the Practice of Modern Medicine," consists of papers by F. S. C. Northrop, a philosopher, and Jurgen Ruesch, a psychiatrist.

The editor states that "the relationship of medicine to anthropology was the core issue" of the Arden House conference. This statement and the title of the book give the impression that this collection is intended to provide a comprehensive review of the collaborative relationship between anthropologists and health practitioners. Anthropologists have been making substantial contributions to a number of areas concerning health and illness, however, to which little or no reference is made in this book, such as the epidemiology of illness, the success and failure of health action programs, and professional-patient relations in cross-cultural settings, all of which have constituted areas of major interest for anthropologists in their collaborative relationships with the health professions. Moreover, much of this research has been done among groups who are carriers of contemporary rather than "primitive" culture. It is evident that these serious omissions are largely a consequence of the almost exclusive emphasis placed by this Arden House conference on non-literate societies on the one hand, and on psychiatry on the other.

OZZIE G. SIMMONS

University of Colorado

Persuasion and Healing. By JEROME D. FRANK, M.D. New York: Schocken Books, 1963. Pp. 282. $1.95.

Jerome D. Frank, a psychiatrist and psychologist, uses both his clinical experience and his extensive knowledge of academic research to analyze the components of psychotherapy and to put this technique into sociological as well as humane perspective His critical approach, dispassionate and abrasive, far from debunking psychoanalytically oriented method, succeeds in validating it as a legitimate technique of healing at the same time as he separates it from undue claims made on its behalf.

Frank compares psychotherapy to other types of healing: "As a relationship in which one person tries to induce changes in another, psychotherapy has much in common with child-rearing, education, and various forms of leadership. Its closest affinities, however, are with time-limited interactions between a sufferer and specially trained persons that stress either healing or attitude change. The former include therapeutic rituals in primitive societies and healing religious shrines in our own, the latter religious revivalism and Communist thought reform." Like other methods of attitude change, psychotherapy

aims at improving a person's functioning by helping him to correct errors and resolve conflicts in his assumptions concerning himself and others. Effectiveness is largely due to the ability to arouse hope by capitalizing on the patient's dependency on others. The healer acts as mediator between the patient and his group, while the ideology and ritual supply the patient with a conceptual framework for organizing his chaotic, mysterious, and vague distress and give him a plan of action, helping him to regain a sense of direction and mastery.

Critics of psychotherapy like to point to the fact that it is "nothing but" influencing, that it is "merely" supportive, that it "only" makes the patient feel better, that it offers to the patient "only" the hope of a cure, and so forth. Perhaps Frank's main contribution is his assigning positive value to what usually has been an object of contempt in our doing-oriented culture. Thus, in explaining the placebo effect, Frank says: "The ability to respond favorably to a placebo is not so much a sign of excessive gullibility, as one of easy acceptance of others in their socially defined roles." And he concludes from some experimental studies of the subject: "Apparently placebo responsiveness was an indicator of the ability of these patients to trust their fellow men . . . and this had something to do with their capacity to adjust to the world outside the hospital." This does not mean, however, that he advocates religious or magic healing. Rather, he would have us, unlike primitives, act on the basis of our recognition of cause and effect. He shows that anxiety and despair can be lethal, confidence and hope life-giving. He hence assigns a method that aims at giving the instilling of confidence and hope a legitimate place among therapeutic techniques.

In analyzing the therapeutic relationship, Frank places much emphasis on sociological concepts such as reciprocity, mutual expectation, and interaction. He uses the same method when analyzing the shaman's role in primitive society. His analysis of religious healing of the Lourdes type is especially brilliant.

It is regrettable that Frank does not sustain his dispassionate critical ability all through the book. When speaking about group therapy, which he very clearly prefers to all other therapeutic techniques, he stresses the supportive functions of the group without even hinting at the similarities between group process and thought reform—a subject so beautifully illuminated by Robert Lifton. On the other hand, he speaks of thought reform when dealing with traditional mental hospitals—which he clearly does not like. Although this aspect of mental hospitals is not to be disputed, Frank would undoubtedly recognize, if challenged, that even there patients derive much support from one another—a fact that probably helps to account for their survival under adverse conditions.

It is easy, in such a short review, to overemphasize the few shortcomings, just as it is difficult to do justice to the brilliant insights, the clarity of writing, and the breadth of perspective that characterize the book. In short: it must be read.

ROSE LAUB COSER

McLean Hospital
Belmont, Massachusetts

The Mental Patient Comes Home. By HOWARD E. FREEMAN and OZZIE G. SIMMONS. New York: John Wiley & Sons, Inc. 1963. Pp xi+309. $7.95.

In this research, Freeman and Simmons extend and modify their earlier investigations on post-hospital adjustment by a more massive attack. Their sample consisted of 649 selected relatives of patients released from nine state and three Veterans Administration hospitals. These relatives were clustered in six Massachusetts counties, and the patients were released in the first six months of 1959. Male patients numbered 280 and female 369. Specially trained social workers interviewed family members in two waves: one as soon as possible after the patient in that family left the hospital, and the second a year after the patient returned to the community (unless he had been rehospitalized, in which case the second interview occurred as soon as possible after return to the hospital). The two-stage method of interviewing was employed to procure data on the independent and dependent variables respectively.

The authors explored a wide variety of

independent variables and devote half or more of the book to their findings regarding these and their interrelationships. On the other hand, they chose as their dependent variables (1) the patient's success or failure to remain in the community without rehospitalization for a year, and (2) the patient's level of role performance while in the community. On the whole their predictors were far more successful in relation to performance (36 out of 45) than for success or failure in remaining in the community (16 out of 45); in the latter instance, only three variables held true for both males and females (p. 174). In addition, the authors explored the effect of symptomatic behavior variables which proved to have even higher correlation with success or failure at remaining out of the hospital or being returned to it. Eventually they refer to this "psychiatric status" as an "intervening variable" (p. 194).

Although their findings are multiple and extensive, the most important seemed to be (1) post-hospital role performance, tenure in the community, and symptomatic behavior vary quite independently of one another; (2) high expectations of role performance by family members are associated with an absence of symptomatology. Since these conclusions contradict many psychiatric assumptions of current hospital practice, rehabilitation, and mental health programs, the authors draw sharp implications for each of these in a set of summary conclusions.

The style of the book is unfortunate; written in psychiatric language, it appears excessively compulsive in detailing every step of the methodological procedure throughout. More of this could be relegated to the appendixes. Only in the last chapter do the theoretical issues become lucid, and here the authors are at their best. But they have lost most of their readers by that time, and this is a great pity.

R. A. SCHERMERHORN

Western Reserve University

Studies in Adolescence. Selected and edited by ROBERT E. GRINDER. New York: Macmillan Co., 1963. Pp. vii+524. $4.50 (paper).

The cover of this paperback carries the additional description: "A Book of Readings in Adolescent Psychology." This and the blurb on the back are equally misleading. The author's introduction states that "the book brings together current substantive studies, both of a theoretical and empirical nature,' to which it may seem desirable to introduce students. He also affirms the necessity of an interdisciplinary approach to the study of adolescent development. Since one may assume that the publisher is probably responsible for the copy on the covers, Macmillan may not have done either the author or the reader a favor.

A short review may not be the place for assessing the rationale underlying the entire *genre* of "Readings." Although there will not be space for a discussion, this reviewer wonders whether such collections really can replace an adequate college library, whether they help or hinder the growth of intellectual curiosity among our students, and whether they are appropriate to some subjects and not to others. Provided that such questions are answered positively in the present context, this book offers some solid virtues. It does provide a variety of selections from the various social science disciplines dealing with both theoretical explorations and empirical investigations. These will, as claimed, acquaint students with a variety of approaches and methods. It is not clear who these students are, however. An introductory course could hardly introduce students to Parsons and Freud, analysis of variance and factor analysis, etc., without creating more pseudosophistication than real insight. A more advanced student may well find these selections challenging him to explore the journals on his own.

Another virtue of this book is that it will introduce students to *Child Development*, which together with the *Child Development Monographs*, both published by the Society for Research in Child Development, represent one of the better sources of high-quality research reports. Out of the thirty-eight readings, eight come from this source; there are five each from the *Teachers College Record* and the *Journal of Abnormal and Social Psychology;* three from the *Journal of Educational Psychology;* two from the *American Sociological Review;* and a variety of other

sources are represented by one selection each.

From a sociologist's point of view, the third virtue of these selections is that three of the four parts into which the book is organized deal with socialization and only one deals with physical and intellectual growth. But most teachers will probably prefer to make up their own selection of reading assignments, even with limited library resources. The price of $4.50, not indicated on the book, will not endear this volume to many students if they have to buy it in addition to a textbook. The pages are sewn, not glued, for which the publisher is getting paid well enough and for which the serious reader will be thankful.

KURT JONASSOHN

Sir George Williams University

Social Deviancy and Adolescent Personality: An Analytical Study with the MMPI. By JOHN C. BALL. Lexington: University of Kentucky Press, 1962. Pp. 119. $3.00.

This is a study of 262 adolescents from public schools in two Kentucky cities (population in the 10,000–19,000 range) and from the training school for delinquents in that state. "Personality" is measured by responses to the complete Minnesota Multiphasic Personality Inventory. "Social deviancy" is defined as "behavior or status which is contrary to existing social norms of what is right and desirable" (p. 3). Operationally, delinquency, minority group (being Negro), lower-class and broken-home status, poor academic performance, and being rated by teachers as maladjusted in personality are regarded as deviance. The book devotes a chapter to each of these forms of "social deviancy" and "personality," together with discussion of related variations in each of the other dimensions of deviancy.

Public school students were in the ninth grade and included all youngsters in school on the testing dates (an absentee rate of 9 per cent is noted). The thirty-eight training school boys and girls studied were in the eighth grade or above and constituted, according to the author, "all incarcerated delinquents who were deemed capable of comprehending the MMPI items" (p. 7). Thirty-four valid MMPI profiles were obtained from the training school boys (19) and girls (15), and 200 from public school students (95 boys and 105 girls).

Findings tend to confirm previous MMPI studies, for example, elevation of training school youngsters on scale 4 (psychopathic deviate), mean scale-score similarity with results obtained in Minnesota by Hathaway and Monochesi. There are some surprises, however, and issues are raised which warrant further inquiry. MMPI profiles of public school students with records of delinquency are similar to those without records (in contrast to differences found between training school and public school youngsters). Negro girls were notably superior in school achievement and showed fewer "neurotic tendencies" than did Negro boys. Boys from broken homes showed marked neurotic (especially depressive) tendencies, while girls did not. Neurotic tendencies were more closely associated with academic achievement than with socioeconomic status, low-achievers evidencing "various personality inadequacies." Throughout, profiles are compared along dimensions within categories, and by sex, but small *N*'s prevent comparisons for more than one type of "deviancy" at a time.

Ball questions the interpretation of MMPI profile elevation as indicating rebellion against adult restraint or conventionality. He proposes that the profiles may, in addition, reflect "developmental progress in becoming socialized" (p. 107). The major thrust of the study is not theoretical, however, but empirical. It adds to an impressive array of studies employing the MMPI. The major theoretical questions, and many empirical ones, remain. *Why* do these patterns emerge as they do? To what extent do they reflect and to what extent do they cause behavioral differences? Ball recognizes the necessity for longitudinal studies in answer to the latter question. The former requires more than the accumulation of data, however, for it is not essentially an empirical question. The strength of the MMPI has always been empirical—the strength of standardization, multiple indicators, validity and reliability checks—and these are all too rare in sociology. The hard task of conceptual and theoretical specification regrettably comes under the heading of unfinished business.

JAMES F. SHORT, JR.

Washington State University

Status Forces in Delinquent Boys. By MARTIN GOLD. Ann Arbor: Institute for Social Research, University of Michigan, 1963. Pp. 229. $6.00.

With Lewinian field theory as a model, this monograph seeks "to discover factors which link social structure to individual behavior," and more specifically to "patterned delinquent behavior" as manifested by repeated delinquents. The literature on delinquency and social structure is searched and a series of hypotheses generated concerning "social status, attraction, control, and delinquency" (p. 43). With respect to attraction, the author expects to find that "higher and lower status delinquents alike" will be characterized by low attraction to the community and to their fathers. Lower-status boys "who do not believe in the American Dream are more likely to become delinquent," but "anticipation of occupational failure, prompted for example by poor performance in school [is expected to provoke] delinquency among higher and lower status boys alike."

Ninety-three Caucasian boys within the normal range of intelligence (above 79) who were currently enrolled in the Flint, Michigan, public schools, and who had more than one police contact for "fairly serious crimes within the three years previous" (1955–57) were matched on these criteria and on social status with boys who had no police contact for any offense. Forty-three of the pairs were then matched with "sometime delinquents," defined as boys who during the previous three years of living in Flint had "committed a serious crime only once." Interview schedules, individually administered, were drawn up for the boys and for their parents. In addition, boys responded to a paper-and-pencil questionnaire in groups at their schools. Chapter iv is a long (47 pages) but very instructive presentation of "One Set of Interviews" with a repeated delinquent, his matched nondelinquent, and their parents. The bulk of the book is devoted to analysis of data from these classes of respondents.

Findings are complex. The quality of neighborhood educational and recreational facilities is found to be relatively unimportant to the task at hand. The principal status forces with which Gold is concerned have to do with the broader social order, with the family as a unit and vehicle for upward mobility, and with orientations of boys and their parents toward future occupation. Lower-status families are found to be "less attractive" to their sons, and even less so to repeated delinquents among them. A good many of the repeaters "do things" with their parents, talk to them about problems, and want to be like their fathers, however, and particularly at the white-collar level, differences between repeaters and non-delinquents are often unreliable. Although many parents are uncertain as to the kinds of jobs they wish their sons to have as adults, those who express a preference indicate a strong preference for professional and technical occupations. Belief in the American Dream (equality of opportunity) is expressed by one-half or more of the parents and boys at all status levels and does not reliably differentiate repeaters from non-delinquents. School performance is perceived as important to future occupation by nearly all boys, but repeaters have poorer grades and lower their job aspirations to a greater extent than do non-delinquents. The latter is interpreted as a defense reaction against feelings of personal failure. The conclusion thus is reached that "most boys do not perceive their life chances as a function of the social system itself but as a function of their personal characteristics" (p. 171). This is an important conclusion, for it is contrary to the suggested linkage between social structure and the generation of delinquent subcultures which is proposed by two recent theories (Cohen and Cloward and Ohlin). The interpretation is not demonstrated in this study, but the data are consistent with our study of Chicago gangs which finds that gang boys do not reject conventional values (see the *American Journal of Sociology,* September, 1963) and retain high educational aspirations despite poor performance.

No doubt the controversy will continue, but evidence seems to be mounting against the notion that adolescent youngsters "blame" the social structure, and conventional institutions and opportunity structures in particular, for *their personal lot in life*. This does not mean that the defensive reaction hypothesis is the only alternative explanation, for some evidence exists that disadvantaged youth generate and embrace more immediately rewarding,

peer-oriented status systems rather than (in some cases, in addition to) lowering their expectations relative to the future. We are told that "there were no flourishing teen-age gangs in Flint in 1957 or since" (p. 48), but a future volume on the peer group is promised.

The Inter-Center Program on Children. Youth, and Family Life at the University of Michigan has been perhaps the most conscientious and skilful research organization in the country in the hard business of communicating research findings to participating lay and professional groups. This monograph continues the tradition. Statistical usages are carefully explained, table titles summarize findings rather than simply indicating which variables are examined, and the use of unnecessary jargon is eschewed.

Given the virtues of the research program and of this report, however, one may question the adequacy of the causal model and just what it is that is being "explained." It is not at all clear, for example, in what sense repeated delinquents constitute "patterned delinquency." Does the fact that a boy has had more than one police contact for "fairly serious crimes" over a three-year period (52 per cent had two contacts, 17 per cent five or more) make his delinquent acts any less "isolated"? Is this a satisfactory indication "that his delinquency was an integral part of his life pattern"? Is the behavior of these boys homogeneous with respect to any other criterion?

We need to know a great deal more about the "target populations" of various delinquency theories. The Flint study is important in part because, as Gold indicates, it is conducted in a city where juvenile delinquency is "not an exceptional problem" and it may be more typical of delinquency in this country than are studies which concentrate on gang delinquency in urban slums. The fact that it is not especially visible or troublesome, however, does not mean that it is homogeneous with respect to etiology.

Finally, the analysis in this volume does not really examine the total impact of forces identified to be important in "causing" delinquent behavior, a step which seems logically implied by the field-theory model. Even if it did, there are uncomfortable gaps between "provocative and controlling forces" and behavior.

But enough. This is an important research program, and the many provocative findings of the book probably are more important to the advancement of knowledge than are the provocations to delinquency, in any case!

JAMES F. SHORT, JR

Washington State University

The Search for Ability: Standardized Testing in Social Perspective. By DAVID A. GOSLIN. New York: Russell Sage Foundation, 1963. Pp. 204. $4.00.

Dr. Goslin's book, the first in a series of studies of testing to be sponsored by the Russell Sage Foundation, was written "to provide a basis for a program of research on the social consequences of testing." At a time when testing practices are being subjected to a series of sensationalistic and poorly considered attacks, Goslin's scholarly and dispassionate view of the problems concerned with standardized ability-testing comes as a welcome change.

The book is divided into three main sections. The first section, "Ability Testing in American Society," which consumes nearly two-thirds of the text, is primarily documentary in nature. The author presents a brief history of ability-testing, discusses the probable extent of current ability-testing, and concludes the section with detailed descriptions of the various ways in which ability tests are used today in education, business, civil service, and the military. Much of the material on the use of tests in education and business may already be familiar to social scientists and educators, although the information on the crucial role of ability-testing in the military and civil service will probably be news to many.

The middle section is concerned with the validity of ability tests. The separate chapters on "construct" validity and predictive validity would be more informative if the author had kept in mind the distinction (made earlier in the first section) between the use of tests as *predictors* and the use of tests as *criteria* (i.e., to assess outcomes). Thus, when a test is used for prediction, the question of construct validity (what does the test measure?) is secondary in importance to the question of predictive validity (how well does the test predict what it is supposed to predict?). Since most ability tests are used for predictive purposes, it might

have been more useful to devote a greater proportion of the discussion on test validity to the very difficult and basic problem of criterion development.

In the final section the author discusses some of the possible influences of ability-testing both on the individual and on the society. The possible impact of certain major *decisions* which are made on the basis of test scores (e.g., the homogeneous grouping of students according to ability) is given special attention. While no specific research projects are actually proposed, many important and potentially researchable problems are discussed.

In brief, this book presents a lucid and comprehensive analysis of the nature, extent, and possible effects of ability testing as it is currently practiced. It is recommended reading for all.

ALEXANDER W. ASTIN

National Merit Scholarship Corporation

Segregation and Mental Health. Edited by MARTIN M. GROSSACK. New York: Springer Publishing Co., 1963. Pp. 247. $4.00 (paper).

In the case of Brown *versus* the Board of Education, the local court stated that "segregation with the sanction of law . . . has a tendency to retard the educational and mental development of Negro children and to deprive them of some of the benefits they would receive in a racially integrated school." The Supreme Court quoted this statement and added that "this finding is amply supported by modern authority." The Court might have been more correct had it said that the finding was widely *believed* by modern authority. The research which had prompted the overwhelming majority of social scientists to oppose school segregation on psychological grounds could hardly be considered complete. The broad argument had been sketched in, but research was meager, sometimes subject to problems of interpretation, and several major problems were essentially untouched. We had no idea, for example, whether school desegregation would result in a sizable increase in the achievement of Negro students. The over-all problem was complicated by a host of factors whose relation to segregation was poorly understood.

It is now 1964, and the intervening ten years have provided ample opportunity for research as thousands of school systems have been desegregated. Meanwhile, the need for this research has become even more pressing, as Northern school systems have begun to worry about their problems of desegregation. Thus it is both a proper and an opportune time for a thorough review and synthesis of the last decade's research.

Segregation and Mental Health would appear to be such a project, but it is not. It is a reprinting of twenty-two journal articles written over the last few years, but the bulk of these are either anecdotal discussions, reports of a single research project, or case studies. The editor has contributed a four-page introduction and one and one-half pages at the end, entitled "Some Needed Research." The major sections include one on Negro children with short papers by David P. Ausubel, Martin Deutsch, and Kenneth and Mamie Clark; a section of research on the personality of Negro adults (including a paper by Sheldon Roen which makes a contribution about the effect of personality on intelligence); a series of papers comparing the mental disease rates among Negroes and whites; and finally, three discussions of Negro patients in psychotherapy.

While several of the papers in the volume are important ones, the quality is not consistent and the high-quality papers are sometimes abridged versions, which again restricts the usefulness of the book. The glaring omission is the absence of any attempt to bring research together on the topics. Ausubel's paper is the only review of the literature included, for example. Four papers on the rate of hospitalization of Negroes fall two against two on opposite sides of the issue; the editor apparently feels that no conclusion can be reached from this material. In that case, it is difficult to see why he included the papers at all. The reader who is familiar with the problem will know the papers he has published here; the reader who does not know the literature will receive little benefit from wandering quickly through the battlefield.

The editor cannot be criticized for the quality of the research reported herein. It must be recognized that this is, at the least, a representative sample of the recent research in this area—research which has been inadequate in both quantity and quality. Of course, it may

be that we have been forced to concentrate our efforts in other areas which are more interesting and where research is more badly needed. If this is the case, perhaps someone can comfort this reviewer by providing for his inspection a list of these more pressing problems.

ROBERT L. CRAIN

University of Chicago

Out of Wedlock. By LEONTINE YOUNG. New York: McGraw-Hill Book Co., 1954. Pp. viii+261. $1.95 (paper).

In comparison with the volume of material available concerning other forms of delinquency or deviancy, giving birth out of wedlock has received little attention. This undoubtedly accounts for much of the acclaim awarded this volume, although the publisher's suggestion that this is the first book-length treatment of the problem indicates a remarkable lack of historical perspective.

In this volume, the author attempts to extract from her direct experience working with mothers in maternity homes and from available case material a psychoanalytically oriented explanation of the individual's behavior. Only superficial attention is given to sociological variables. Giving birth out of wedlock is interpreted as "acting-out" behavior directed by the unwed mother against her parents. This thesis rests upon a presumed absence of a meaningful relationship between the unwed mother and the father of the child and little or no interest in the baby as a person. More recent field studies, such as the work of Clark Vincent, cast doubt upon the assumption. Given the thesis, the author fails to derive logically consistent hypotheses. Rather she proposes every possible form of the hypotheses, exhausting the range of possibility but contributing little clarification of the problem. Thus every kind of relationship between a mother and daughter can lead to unwed pregnancy. Case material is used not to test hypotheses but to illustrate them. The case histories described are almost exclusively drawn from middle- or upper-income groups. The group for whom the problem is most common, the very poor, receive least emphasis. In addition, no comparison with the normal experiences of women in pregnancy or childbirth is included. While not all unscholarly volumes are inaccurate, in this instance the author perpetuates, if not enhances, popular notions of the case of the birth of the illegitimate child which recent national and local statistics demonstrate to be false.

In summary, this volume is seriously limited in the source of data, in uncritical acceptance of common notions, in the lack of rigorous logical derivation of hypotheses, and in the failure to use the available material to test these hypotheses.

JANE C. KRONICK

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Slums and Social Insecurity: An Appraisal of the Effectiveness of Housing Policies in Helping Eliminate Poverty in the United States. By ALVIN L. SCHORR. (U.S. Department of Health, Education, and Welfare, Social Security Administration, Division of Research and Statistics, Research Report No. 1.) Washington, D.C.: Government Printing Office, 1963. Pp. viii+168. $0.50.

An American Dependency Challenge. By M. ELAINE BURGESS and DANIEL O. PRICE. Chicago: American Public Welfare Association, 1963. Pp. xxiii+285. $5.00.

Increasingly insistent promptings by government officials, social scientists, and journalists over the past three years have now brought forth from President Johnson a clear statement that poverty is to be regarded as a state of the union. As Dwight Macdonald has suggested, the climate of opinion in the country has changed since J. K. Galbraith's sunny assertion in 1958, accepted by most of his readers as obvious, that poverty in this country is no longer "a massive affliction [but] more nearly an afterthought." Studies like the two reviewed here are reflections of rising concern about how many Americans do not participate in the prosperity the country as a whole has experienced in recent times. These books carry the message that 20-25 per cent of the population live in "modern poverty"; that four million people are supported by just one government assistance program (the ADC program). Schorr is concerned with investi-

gating the multiple effects of poor housing in reinforcing and perpetuating poverty; Burgess and Price describe in statistical detail the families who received ADC support in forty-three states.

"A conception has yet to be developed that sees man in relation to his physical environment. Until such a scheme is developed, and research adapted to it, we shall not fully perceive the relationship of man to shelter. Meanwhile we shall build houses." In this statement Schorr indicates his interest in the theoretical dimension of the problem, in descriptive accuracy of how the poor are housed, and in ameliorative action. His book represents a thoughtful and challenging essay on these problems; his method is that of an exhaustive review and integration of the literature, carefully cited and imaginatively interpreted. The author acknowledges that solid research evidence in the area is slight; he makes the best of the situation by suggesting both fruitful lines for further inquiry and ways in which what little is known can be applied to deal with current housing problems. He is concerned with the effects of housing (on self-perception, stress, health, general morale) and of neighborhood. He considers the institutional framework which conditions the kinds of housing and neighborhoods in which the poor live (physical and social planning, relocation, community services, segregation, housing codes and financing, the public and private dimensions of housing, public assistance as it bears on housing). Clearly this book was written to influence the research interests of social scientists and the policy deliberations of all whose decisions affect housing. If it is as widely read as it deserves to be, it will accomplish both of those purposes.

An American Dependency Challenge looks at the poor through a more narrowly focused lens. It is designed in part as a follow-up survey to a 1950 study of recipients of ADC, but its emphasis on *families* rather than upon aided *children* is in keeping with the shift in emphasis of federal programs toward a concern with "needy families." The study is based on interviews with 5,500 families having 17,500 children. The sample is of ADC cases closed in the first quarter of 1961 in 43 states (the rest did not co-operate). The interviews were conducted by case workers and some of the coding and card-punching was carried out by state agencies. The authors do not seem to be particularly concerned about the effect of this procedure on the validity of the respondents' answers to questions about illegitimacy, family problems, etc. The study presents a detailed description of "family structure and characteristics, reasons for and length of dependency, family budgeted needs and income, housing and level of living, status of children, family problems and potential for independence." It will be a basic reference for those who are interested in ADC families. The bureaucratic jungle in which these families find their livelihood, however, makes it difficult to interpret some of the findings; the characteristics of the population are as much a function of momentary (and varying) definitions of eligibility as of the life situation of the family. An exhaustive study of the whole client-agency social system should be considerably more enlightening, though more unsettling for its sponsors and agency participants.

LEE RAINWATER

Washington University

Alcoholism and Society. By MORRIS E. CHAFETZ and HAROLD W. DEMONE, JR. New York: Oxford University Press, 1962. Pp 319. $6.95.

In the last decade there has been an uninterrupted flow of brochures, articles, monographs, and books which have as their goals the understanding of alcoholism and the creation of a more positive treatment climate toward the alcoholic. These goals, exemplified by this book, are commendable given the general negative attitudes toward the alcoholic of both the general public and the therapeutic professions. But this statement should not confuse the fact that Chafetz and Demone have provided little that is new to the field of alcohol studies.

The book is divided into four sections I "Alcoholism and Society," II, "A Review of Previous Work," III, "Contemporary Alcoholism Programs," and IV, "The Concept of Alcohol Prevention." The materials are geared for the college Freshman or Sophomore course in social problems. Despite the over-all adequacy of the presentation for this basic level, the text has a number of shortcomings from a sociological point of view.

First, the title, *Alcoholism and Society,* is a misnomer for the student expecting systematic accounts of the relationships of societal values, norms, and institutions to various cultural drinking behaviors and their pathological manifestations. The all-too-brief accounts of drinking practices in thirteen "preliterate societies" and "literate groups" unfortunately only superficially indicate the crucial role of normative attitudes toward beverage alcohol in the development of drinking pathologies. This is probably a consequence of the authors' narrow conception of alcoholism as stated in the following sentence: "Alcoholism, we believe, is the result of disturbance and deprivation in early infantile experience and related alterations in basic physiochemical responsiveness; the identification by the alcoholic with significant figures who deal with life problems through the excessive use of alcohol; and a socio-cultural milieu which causes ambivalence, conflict, and guilt in the use of alcohol" (p. 4).

A theoretical conception that relies so heavily on psychoanalytical thought immediately provides the researcher with difficulty when he examines the case of France, which is reputed to have the highest incidence of drinking pathologies in the world. Researchers from southern Europe, especially France, vociferously reject, as Jellinek has pointed out on innumerable occasions, the idea that "prealcoholic maladjustments lead to the heavy use of alcoholic beverages" and instead emphasize the strong role of social customs and habits.

Social control mechanisms to cope with beverage alcohol and the drinking pathologies are the focus of the section on alcoholism programs. But this book, and this is a shortcoming in the field of alcohol studies, does not grapple with the question of to what extent governmental controls over beverage alcohol (prohibition, taxation, licensing, age when drinking allowed, etc.) actually affect drinking behaviors and pathologies. Furthermore, despite the existence of several longitudinal studies (Gibbons and Armstrong, Norvig and Nielsen, Selzer and Holloway) of treated alcoholics in both private and public settings in this country and abroad, the authors provide little information on what happens to samples of alcoholics after treatment. From this reviewer's perspective, the major breakthrough in terms of understanding alcoholism pathologies will come from systematic longitudinal studies of different *samples* of alcoholic patients experiencing different therapeutic regimes and social situations.

Partially reprinted in the book is Chafetz' paper, "Alcoholism Problems and Programs in Czechoslovakia, Poland, and the Soviet Union," which created considerable discussion in Prague and Warsaw when I was there in 1962. My impression from having visited the programs in Czechoslovakia and Poland is that in his statements, "all in all, the Czechs are doing an excellent job" (p. 130) and "the inadequate effort in Poland is in contrast to the comprehensive alcoholism program in Czechoslovakia" (p. 104). Chafetz simplified into two extremes the actual conditions. Institutional care of alcoholics in Czechoslovakia left me depressed with its emphasis upon patients' keeping daily diaries in pencil which are corrected in ink by their "psychotherapists," by the graded weekly "quizzes" given to the patients on alcoholism, and by the use of aversion therapy, to mention only three points. In short, the Czech program views the alcoholic in mechanistic terms—as one to be resocialized despite his motivations; fortunately the Polish program has not as yet advanced to this stage.

In the area of bringing psychoanalytical theory to bear on the problem of alcoholism, especially in the "psychotherapy case-history experiences" in the Appendix, the book makes a worthwhile contribution to the field of alcohol studies.

DAVID J. PITTMAN
Washington University

The Homosexual and His Society: A View from Within By DONALD WEBSTER CORY and JOHN P. LEROY. New York. Citadel Press, 1963. Pp. xii+276 $5 95.

The Problem of Homosexuality in Modern Society. Edited by HENDRIK M RUITENBEEK New York. E P Dutton & Co , 1963. Pp. xiv+304. $4 95

The Ruitenbeek anthology contains sixteen articles, all but one previously published Eight cover a wide range of psychoanalytic thinking. Freud, Ferenczi, Kardiner, Clara Thompson, Lindner, Ruitenbeek, Rado, and

Ovesy. I found, as I suspect many sociologists would, the chapter from Linder's *Must You Conform?* the most interesting of the lot. Sociologists more attuned to psychoanalytic theorizing will presumably find some of the others of more value than I did.

The remaining eight papers contain material closer to sociological interests. Some (those by Leznoff and Westley, Reiss, and Devereux) will be familiar. Others, such as Hooker's report on "The Adjustment of the Male Overt Homosexual," a journalistic account of male prostitutes in London, and Ellis' account of psychotherapy with homosexuals, are worth becoming acquainted with. All in all, it is a useful collection of papers, but hardly exciting to anyone familiar with the area. It is a little sad to think how little sociology has yet contributed to the study of homosexuality.

The Cory-Leroy book is a document in what might be called the "civil liberties" literature on the problem. Homosexuals have in recent years created several organizations devoted to self-help, which devote a fair amount of their energy to crusading against discriminatory practices against homosexuals and to campaigning for more humane treatment and better understanding. The resemblance to ethnic "defense" organizations becomes vividly clear as one reads the continuing statement of the viewpoint in this book. Aside from its interes as a political document, so to speak, the book contains the clearest description I have ever seen of homosexual practices, an interesting (though journalistically shallow) description of homosexual ways of life, and a useful glossary. There are also some interesting arguments here and there, as when the authors contend that distinctions between "active" and "passive" or "male" and "female" roles need to be used cautiously, because so many homosexuals switch back and forth between them.

HOWARD S. BECKER
Stanford University

The People Look at Educational Television. By WILBUR SCHRAMM, JACK LYLE, and ITHIEL DE SOLA POOL. Stanford, Calif.: Stanford University Press, 1963. Pp. 209. $5.50.

Audience surveys, in their concern with who likes what, have been prone to overlook the major tenet on which much public criticism of the media is founded: Commercial television, aimed as it is at "majority" audiences, leaves important needs of many minorities inadequately satisfied. Educational television (ETV) is meant to fill some of these gaps. The survey findings from nine educational television reception areas reported in this volume offer interesting observations on the appeals and attraction of non-commercial stations and how their programs supplement the commercial broadcasting fare.

It comes as no surprise that by the standards of commercial broadcasting the audiences for ETV are not very large. The proportions of "regular" ETV viewers (here defined as those watching at least once a week) vary considerably. In San Francisco and Pittsburgh, where ETV stations seemed well established, the proportion was about 25 per cent; it was less than 3 per cent in Columbus, Ohio. To be sure, a much larger number view ETV "occasionally," but even the hard core of regulars spend relatively little time with ETV, preferring many of the features offered on commercial stations.

As a group regular viewers are better educated and come from higher socioeconomic levels than non-viewers. These characteristics account also for the general program preferences, pursuit of "high culture" and of information, the higher use of print, participation in a greater range of social and leisure activities, etc., that distinguish the ETV audience from other televiewers. Still, there is considerable deviation from this over-all pattern, and some of the most interesting sections of the book are those relating the profile of a specific station audience to the programming profile of that station. Even the audience of a single station is found to contain several different types of viewers, each with its distinct interests. For example, there is a sizable number of blue-collar households where ETV is watched; though these viewers generally watch *more* TV than the typical upper-middle-class ETV viewer, the general media interests of both are otherwise quite similar.

The volume gives ample illustration of the variety existing in the audience and program fare of educational television. In their entirety the findings also make amply clear how grave a misnomer is the term "educational television," evoking as it so frequently does an

image of formal instruction. Strictly instructional programs are only a small part of the fare and by no means the major attraction. But small audience figures when analyzed in terms of too many variables tend to result in too few cases for definitive discussions. This is a shortcoming of the analysis in some places. Some typologies, for example, are mere *ad hoc* constructions made to fit the data rather than carefully derived from them, as in the "types of audiences that different ETV programs attract" in Appendix C (Table 4A) Beyond this the findings suggest that the conclusion so generally derived from the quantitative success of commercial TV—big audiences, ergo satisfied audiences—must be somewhat qualified. Over the years the book may also come to serve as a first "trend point" for observing the changing role of ETV and the responses to it.

KURT LANG

Queens College of the City University of New York

Data Quality Control. By RAOUL NAROLL. New York: Free Press of Glencoe and Crowell-Collier Publishing Co., 1962. Pp. 198. $5.50.

It is good to see that elementary statistics has finally diffused to the ethnographers. This book contains a statistical case study of the quality of ethnographic data. The focus is on the quality of observational data as dependent upon the observational situation.

Using the Human Relations Area Files the author studies four types of behavior: drunken brawling, defiant homicide, protest suicide, and witchcraft attribution. For each type of behavior the bias of the informant, of the ethnographer, and of the user of the ethnographic data is assessed. A fourfold table of frequency of behavior classified by characteristics of observation is summarized by Yule's Q. The characteristics of observation are: (1) listing of particular cases of behavior or general comment on frequency of behavior; (2) lived among people or not; (3) length of stay one year or less; (4) knew native language or not, (5) professional observers or not; and (6) great or small amount of coding of ethnographic report by comparative student.

The conclusions are: (1) drunken brawling is not correlated with any of the behavioral situations, presumably because an informant is not needed; (2) defiant homicide reports are higher for longer observations and for knowledge of native language; (3) protest suicide is higher for qualitative reports than for quantitative reports, higher for living among the people studied, and higher for knowledge of native language; (4) witchcraft attribution is higher for longer stay and knowledge of native language.

The general implication is that if observation requires reliance on informant reports, then the quality of data depends on rapport with the informants. This principle is well known to all field workers, but it is nice to have statistical support for it.

The author does not take into account or fully comprehend the importance of the intercorrelation among his measures of the behavioral situation. For example, in contrasting missionaries with anthropologists on witchcraft attribution, it is noted that all missionaries are high, and they are therefore judged as better observers. But all missionaries are also in the long-stay category—is the missionary relationship spurious?

One also notes that the criterion of accuracy of observation is weak or absent. Some restudy data are analyzed in an effort to cover this gap

In summary, the problem taken up in the book is sensible and the statistical analysis is suggestive, yet not uniformly competent Unfortunately, the book is marred by a very silly extended analogy to industrial quality control and by pretentious claims of new methods and new procedures.

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A Strategy of Decision. Policy Evaluation as a Social Process. By DAVID BRAYBROOKE and CHARLES E. LINDBLOM New York: Free Press of Glencoe, 1963. Pp 268 $5.95.

Ancient tradition tells us that to make rational decisions we must fix a set of ranked objectives, consider all possible relevant consequences of all possible policies for reaching them, and select the best policy in terms of those objectives Charles E Lindblom has long opposed this "synoptic ideal" as he calls it

In this book he further elaborates his conception of a more realistic decision-making scheme, which he calls "the strategy of disjointed incrementalism," and David Braybrooke extends the strategy to certain knobby problems in the field of ethics.

Attempts to use the synoptic ideal run into real problems for which the ideal offers no solutions. These problems stem from man's inability to "get all the facts" about objectives, policies, and consequences, or to use them consistently even if he could, and from value multiplicity, fluidity, and conflict.

Decision-makers in an open society make various adaptations to these problems, all in the direction of practicality. They work with "themes of concern" rather than fixed values or rules of choice, focus on incremental and remedial changes, search for acceptable value themes as much as for alternative policies, limit attention to a few policies and a small range of consequences, attack given problems repeatedly, and rely upon the pluralistic organization of interests to take up whatever they overlook. These adaptations constitute the "strategy." It achieves a tolerable level of rationality in society as a whole.

Whether the strategy is a description of a "social process" or an alternative ideal of rationality is not clear. Empirical evidence is thinly adduced, and one suspects the actual policy process is even more complex. The use of both the terms "process" and "strategy" suggests the ambiguity. The point is important. Because of enormous implications for social organization, the legitimacy of the strategy should be fully established. The synoptic ideal is monocratic; the strategy, pluralistic. This reviewer gets the impression that the authors are not certain they have fully established this legitimacy and feel safer when they claim only a more realistic description of the policy-making process in a democracy.

Some will feel that the dependence of the strategy on existing interest-group politics requires a much fuller treatment of both the representativeness of interest groups and whether they limit consideration of consequences too severely.

This book is important, and one hopes the authors will keep working at the job—to provide an intellectual foundation for the open, pluralistic society.

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Errata

The *Journal* regrets two errors in the review by Lewis A. Coser of John Wilson Lewis' book, *Leadership in Communist China,* which appeared in our March, 1964, issue. In the fourth line of the first paragraph, the word "not" was omitted from the phrase "much of which has not previously been available." In the second paragraph the ninth line should have read "without critical or systematic evaluation," and *not* "without critical or sympathetic evaluation" as printed.

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The Professionalization of Everyone?[1]

Harold L. Wilensky

ABSTRACT

A popular generalization is that occupations are becoming "professionalized." The label is loosely applied to increasing specialization and transferability of skill, the proliferation of objective standards of work, the spread of tenure arrangements, licensing, or certification, and the growth of service occupations. This paper argues that these loose criteria are less essential for understanding professional organization than the traditional model of professionalism which emphasizes autonomous expertise and the service ideal. Examination of the history of eighteen occupations uncovers a typical process by which the established professions have arrived. Among newer and marginal "professions," deviations from the process can be explained by power struggles and status strivings common to all occupations. Barriers to professionalization are pinpointed. Analysis of the optimal "technical" base for professionalism suggests that knowledge or doctrine which is too general and vague or too narrow and specific provides a weak base for an exclusive jurisdiction. Data on the clash between professional, organizational, and client orientations among 490 professors, lawyers, and engineers suggest that (1) bureaucracy may enfeeble the service ideal more than it threatens autonomy; (2) a client orientation undermines colleague control and professional norms. The main theme: (1) very few occupations will achieve the authority of the established professions; (2) if we call everything professionalization, we obscure the newer structural forms now emerging.

Many occupations engage in heroic struggles for professional identification; few make the grade. Yet there is a recurrent idea among students of occupations that the labor force as a whole is in one way or another becoming professionalized.[2] In

[1] This paper is a general development and test of ideas about the professionalization of social welfare occupations in H. L. Wilensky and C. N. Lebeaux, *Industrial Society and Social Welfare* (New York: Russell Sage Foundation, 1958), chap. xi, and of hospital administration in my "The Dynamics of Professionalism," *Hospital Administration*, VII (Spring, 1962), 6–24. It owes much to the work of E. C. Hughes, *Men and Their Work* (Glencoe, Ill.: Free Press, 1958), and R. K. Merton *et al.* (eds.), *The Student-Physician* (Cambridge, Mass.: Harvard University Press, 1957). The material, used by permission of the Free Press of Glencoe, is based on a forthcoming book. I am grateful to the National Institute of Mental Health (M-2209, 1958–63), the Department of Sociology of the University of Michigan, and the Center for Advanced Study in the Behavioral Sciences for generous support, and to Albert J. Reiss, Jr., and Erving Goffman for critical readings of an early draft.

[2] The Webbs early predicted that "the Trade Unions of the Workers . . . will more and more assume the character of professional associations." Representing crafts based on "expert specialization" validated by "public opinion," they wrote, "Each Trade Union will find itself, like the National Union of Teachers, more and more concerned with raising the standard of competency in its occupation, improving the professional equipment of its members, 'educating their masters' as to the best way of carrying on the craft, and endeavoring by every means to increase its status in public estimation" (S. and B. Webb, *Industrial Democracy* [London: Longmans, Green & Co., 1902], pp. 825–26. Cf. R. H. Tawney, *The Sickness of an Acquisitive Society* [London: Fabian Society and George Allen & Unwin, 1920], chap. vii). Discussion of the professionalization of management has continued for decades. See, e.g., L. D. Brandeis, *Business—*

a worthy effort to uncover similarities among all occupations, many sociologists have succumbed to the common tendency to label as "professionalization" what is happening to real estate dealers (realtors) and laboratory technicians (medical technologists). Personal service functionaries like barbers, bellboys, bootblacks, and taxi drivers, it appears, are also "easily professionalized."[3]

DEFINING PROFESSION

What are the differences between doctors and carpenters, lawyers and autoworkers, that make us speak of one as professional and deny the label to the other? The best way to approach the problem is to assess critically the argument that labor is becoming professionalized (see Chart 1).

If, as Chart 1 suggests, specialization, the ultimate application of theory, transferability of skill, stability of employment or attachment to firm, and the existence of work rules do not help in defining a "profession," what does?

Any occupation wishing to exercise professional authority must find a technical basis for it, assert an exclusive jurisdiction, link both skill and jurisdiction to standards of training, and convince the public that its services are uniquely trustworthy. While this traditional model of professionalism, based mainly on the "free" professions of medicine and law, misses some aspects of the mixed forms of control now emerging among salaried professionals, it still captures a distinction important for the organization of work and for public policy. In the minds of both the lay public and professional groups themselves the criteria of distinction seem to be two: (1) The job of the professional is *technical*—based on systematic knowledge or doctrine acquired only through long prescribed training.[4] (2) The professional man adheres to a set of *professional norms*.

To say "technical" is not to say "scientific." For the basis of the claim to exclusive competence varies according to the distinctive features of each profession's functions and background. Contrast, e.g., two of the oldest professions—medicine and the ministry. Medicine, since its "reform" in the United States some sixty years ago, has emphasized its roots in the physical and natural sciences along with high, rigorously defined, and enforced standards of training designed to impart that body of knowledge. Among the dominant denominations in the ministry, rigorous standards of training are also stressed, and doctrines are well codified and systematized, providing a technical base for practice—a base less secure than that of medicine, but still within the scope of the definition.

These two cases tell us not only that both scientific and non-scientific systems of thought can serve as a "technical" base for professionalism but that the success of the claim is greatest where the society evidences strong, widespread consensus regarding the knowledge or doctrine to be applied. In modern societies, where science enjoys extraordinary prestige, occupations which shine with its light are in a good position to achieve professional authority. Thus, while medicine has had its sectarian dissenters (chiropractors, osteopaths, and at one time psychoanalysts), it enjoys more acceptance than the ministry, whose doctrines are anchored in conflicting re-

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a Profession (Boston: Small, Maynard & Co., 1914). And more recently, talk of the professionalization of labor has been revived (see N. Foote, "The Professionalization of Labor in Detroit," *American Journal of Sociology*, LVIII [January, 1953], 371–80; and A. L. Stinchcombe, "Bureaucratic and Craft Administration of Production: A Comparative Study," *Administrative Science Quarterly*, IV [September, 1959], 168–87).

[3] T. Caplow, *The Sociology of Work* (Minneapolis: University of Minnesota Press, 1954), pp. 48, 139.

[4] An operational test for "technical" is that preference in hiring is given to those who have proved competence to an agency external to the hiring firm or consumer (cf. Stinchcombe, *op. cit.*). Again what counts is training for practice in an exclusive occupational jurisdiction; there is no notion that it can all be learned on the job.

ligious communities. There is clearly more consensus about the products of applied science than about spiritual values; the best way to avoid smallpox is more certain than the best way to achieve salvation. Nevertheless, in some places the ministry comes close to science-based professions in it monopoly of recognized skill while carpentry does not; many of us might construct a homemade bookcase, few would forgo a clergyman at the grave. The key difference is that the clergy's tasks and tools, unlike the carpenter's, belong to the realm of the sacred—which reinforces a jurisdictional claim grounded in formal training and indoctrination. Occupations

CHART 1

IS THERE A PROFESSIONALIZATION OF LABOR?

Labor Becoming Professional*	Labor Not Becoming Professional
1. More manual jobs involve a *specialized technique* supported by a body of *theory*—mathematical, physical, chemical, even physiological and social-psychological. Systematic on-the-job training leads to *upgrading* of machine tenders. "Almost every employee in the plants of Detroit will be an engineer of one kind or another."	1. Specialization is no basis for professional authority. High degrees of specialization prevail at every skill level, both in jobs which can be done by almost anyone (assembler in auto plant) and in jobs done only by the trained (surgeon). The link between manual work and theory is tenuous. While ultimately all labor is rooted in theory, the civil engineer who designs a bridge may know some laws of physics, the workers who build the bridge do not. If by upgrading we merely mean ability to learn by virtue of previous experience and challenge, then IQ becomes the criterion for professional.
2. *More manual jobs involve transferable skills.*	2. *Transferability of skills* is limited in most manual jobs outside of the traditional crafts; much of the new technology requires training for the specific system or machine in the particular workplace Such transferability is often absent in the established professions anyway. E g., the house counsel and other staff experts have skills which are bound to a particular organization; their knowledge concerns the traditions, personalities, and procedures unique to that organization.
3. More manual jobs now provide *careers* regulated and supported by a colleague group. Unions try to "reconstruct industry so as to assure to every man a career." The evidence is in union demands for: a) *Seniority in promotions*—to assume that everyone will wait his turn and so eventually reach "a job which matches his highest powers—one object of a career." Like the system of rank and pay of the professor. b) *Demand for continuous income*—wage and work guaranties, pension and welfare programs, salaried status.	3. No evidence that colleague control of manual jobs is increasing. The pace of technological change is fast; the changes are administered mainly by employers. This makes it unlikely that manual jobs, comprising a declining fraction of the labor force, will provide stable "careers" in any sense of the term.† Most of these ways to increase the job security of high-seniority workers are traditional; they have not prevented the wiping out of obsolete crafts or unstable employment among low-seniority men. If salaried status were a criterion of professionalism, we would have to call the fee-taking doctor non-professional and the office clerk professional. If stable attachment to the enterprise is the criterion, this goes with a decline in colleague control.
4. *An objective and fair set of rules and standards* enforced by grievance procedures and arbitration under union contract is the *equivalent of professional codes of ethics* supported by public trust. Moreover, unions show an increasing responsibility toward the consumer—a new concern with the impact of wages on prices, laws to protect consumers, etc.	4. Negotiated plant rules are not focused mainly on quality of product or of work performance; they are overwhelmingly and properly concerned with protection of employees rather than the public. The public-relations programs of both unions and management should not be mistaken for their hard-core policies in contracts and daily life.

* The arguments are adapted from Nelson Foote, *op. cit.*, who states them in their sharpest form. Quotations are from Foote.

† H. L. Wilensky, "Orderly Careers and Social Participation: The Impact of Work History on Social Integration in the Middle Mass," *American Sociological Review*, XXVI (August, 1961), 521–39.

which successfully identify themselves with the sacred may achieve as much of a mandate for monopoly as those which identify themselves with science. University teaching throughout modern history has combined both strategies. And the legal profession is based on moral doctrines only slightly modified by systematic empirical research.[5] We have perhaps made too much of the difference between an occupational mandate derived from science and sanctioned by law (medicine) and one derived from morality or religion and sanctioned by public opinion or by the supernatural (the priesthood). Science, as Durkheim noted, cannot combat popular opinion if it does not have sufficient authority "and it can obtain this authority only from opinion itself."[6]

The criterion of "technical" is not enough, however. The craftsman typically goes to a trade school, has an apprenticeship, forms an occupational association to regulate entry to the trade, and gets legal sanction for his practice. But the success of the claim to professional status is governed also by the degree to which the practitioners conform to a set of moral norms that characterize the established professions. These norms dictate not only that the practitioner do technically competent, high-quality work, but that he adhere to a service ideal—devotion to the client's interests more than personal or commercial profit should guide decisions when the two are in conflict.

Despite the temptation to adopt C. Wright Mills's cynicism,[7] the norm of selflessness is more than lip-service. It is probably acted out in the established professions at a somewhat higher rate than in other occupations. Among the reasons are the following: (1) The belief that the professions offer superior opportunity for service is widespread; it is one of the motives accounting for the excess of aspirants over entrants to these occupations—and there may be a self-selection of the service-motivated. (2) The client is peculiarly vulnerable; he is both in trouble and ignorant of how to help himself out of it. If he did *not* believe that the service ideal were operative, if he thought that the income of the professional were a commanding motive, he would be forced to approach the professional as he does a car dealer—demanding a specific result in a specific time and a guaranty of restitution should mistakes be made. He would also refuse to give confidences or reveal potentially embarrassing facts. The service ideal is the pivot around which the moral claim to professional status revolves.

Supporting the service norm are several additional ideas which influence relations with clients and colleagues but which distinguish professional occupations in only minor degree. For instance, norms covering client relations dictate that the professional be *impersonal* and *objective* (limit the relationship to the technical task at hand, avoid emotional involvement) and *impartial* (not discriminate, give equal service regardless of personal sentiment).[8] How-

[5] In a sense social work has vacillated between the ministry (doctrine-oriented social reform) and medicine (science-oriented clinical practice) as models for the professional thrust.

[6] É. Durkheim, *The Elementary Forms of the Religious Life* (Glencoe, Ill.: Free Press, 1947 [first ed.; London: G. Allen & Unwin, 1915]), p. 208. The consensus of client publics regarding the knowledge base of an occupation is both cause and consequence of the degree of professionalization. Perhaps occupational groups which serve mainly high-status clientele (e.g., Presbyterian and Episcopalian clergy) are pressured into organizing themselves professionally. The members, many themselves professionals with long training and rationalized work, expect high standards of preparation and performance from those who serve them. At the same time, once an occupation has secured an institutional basis for practice (a professional school, strong organization, legal protection, etc.), it is easier for it to develop, disseminate, and claim exclusiveness for its knowledge; it can "enforce" the notion that it is "technical."

[7] C. W. Mills, *White Collar* (New York: Oxford University Press, 1956), chap. vi.

[8] Cf. T. Parsons, *The Social System* (Glencoe, Ill.: Free Press, 1951), pp. 433–39, 454 ff., and *Essays in Sociological Theory* (Glencoe, Ill.: Free Press, 1949), chap. viii.

ever, these norms do not provide a clear demarcation between professional and nonprofessional occupations, for many crafts and commercial establishments foster similar work rules; such statements describe customer-clerk relations in established department stores almost as much as doctor-patient relationships.

In the area of colleague relations, two norms seem especially well developed in established professions: (1) "Do what you can to maintain professional standards of work" (e.g., professionals tend to honor the technical competence of the formally qualified, avoid criticism of colleagues in public, condemn unqualified practitioners, avoid too much or too little work if it lowers standards, etc.); (2) "Be aware of the limited competence of your own specialty within the profession, honor the claims of other specialties, and be ready to refer clients to a more competent colleague."[9] Both norms can be viewed as essential conditions for the maintenance of the master norm—the technical service ideal.

In short, the degree of professionalization is measured not just by the degree of success in the claim to exclusive technical competence, but also by the degree of adherence to the service ideal and its supporting norms of professional conduct.

IS THERE A PROCESS OF PROFESSIONALIZATION?

While there may be a general tendency for occupations to seek professional status, remarkably few of the thousands of occupations in modern society attain it. Perhaps no more than thirty or forty occupations are fully professionalized—an estimate that moves up or down depending upon how many of the scores of engineering and scientific specialties we include. An obvious difficulty here is the overlap between a scientific discipline (psychology, physics) and a profession (the practice of clinical psychology, aerospace science). A science, in contrast to a profession, has no clients except, in an ultimate sense, society; and bosses, if any, are often indeterminate. The main public for the scientist is fellow-scientists, who are in a position to judge competence; the main public for the professional is clients or employer-clients, who usually cannot judge competence. The ambiguity arises from the fact that the scientist as teacher or employee may come to view his students or other groups as clients and reduce his sensitivity to colleagues, while the professional may have a high degree of sensitivity to his colleagues and reduce his openness to influence from clients or bosses. The typical case in the professional world of the future (assuming a larger percentage of salaried professionals and more scientific practice) may combine elements from each model. At any rate, the scientist's disinterested search for truth is the functional equivalent of the professional's technical service ideal, and where a scientific discipline has a substantial segment of its adherents fully engaged in applied work, the requisites of a profession are generally met.

Which occupations have gone how far in professionalizing? Established solidly since the late Middle Ages have been law, the clergy, university teaching (although the church did dominate universities, medieval faculty were by no means all clergy), and to some extent medicine (especially in Italy). During the Renaissance and after, the military provided professional careers for a dispossessed aristocracy. Officer cadres in the standing armies of Europe from the sixteenth to the nineteenth centuries developed a professionalism based on a sense of brotherhood in a self-regulating fraternity dedicated to codes of honor and service.[10] Dentistry, architecture, and some areas of engineering (e.g., civil engineering) were professionalized by the early 1900's: certified public

[9] Wilensky and Lebeaux, *op. cit.*, pp. 303–8.

[10] A. Vagts, *The History of Militarism* (rev. ed., Greenwich: Meridian Books, 1959), pp. 43–54. Cf. M. Janowitz, *The Professional Soldier* (Glencoe, Ill.: Free Press, 1960).

accounting and several scientific and engineering fields came along more recently. Some are still in process—social work, correctional work, veterinary medicine, perhaps city planning and various managerial jobs for nonprofit organizations—school superintendents, foundation executives, administrators of social agencies and hospitals. There are many borderline cases, such as schoolteaching, librarianship, nursing, pharmacy, optometry. Finally, many occupations will assert claims to professional status and find that the claims are honored by no one but themselves. I am inclined to place here occupations in which a market orientation is overwhelming—public relations, advertising, and funeral directing.[11] The barriers to their professionalization are discussed below.

Can a comparison of the few occupations which are clearly recognized and organized as professions tell us anything about the process of professionalization? Is there an invariant progression of events, a path along which they have all traveled to the promised professional land? Do the less-established and marginal professions display a different pattern?

Such students of the sociology of work as Everett Hughes have pictured a "natural history" of professionalism.[12] Their studies are suggestive, but to establish anything firm here we need both detailed social histories of occupations and more systematic comparison of the cases in hand. Table 1 attempts such comparison. It summarizes the history of eighteen occupations for which satisfactory information could be obtained and shows the sequence that best fits the development of the established professions in the United States; it suggests some necessary elements in the professional package and confirms the idea that there is a typical sequence of events. While that sequence is by no means invariant, the table shows that only 32 of 126 dates for crucial events in the push toward professionalization deviate from the following order:

1. An obvious first step is to *start doing full time the thing that needs doing*. The sick were always nursed, but technical and organizational developments created nursing as an occupation. Hospitals have always been managed, more or less, but the development of the modern hospital created the occupation of hospital administration. At this early stage, the practitioners come, of necessity, from other occupations.[13]

2. The question of training soon arises. The early recruits, or a client public or, less often, a professional association press

[11] Among discussions of the forces promoting and impeding professionalization, see M. Lieberman, *Education as a Profession* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1956); J. L. Colombotos, "Sources of Professionalism: A Study of High-School Teachers" (unpublished Ph.D. dissertation, University of Michigan, 1961); W. J. Goode, "The Librarian: From Occupation to Profession?" *Library Quarterly*, XXXI (October, 1961), 306–20; M. L. Fiske, *Book Selection and Censorship* (Berkeley: University of California Press, 1958); R. W. Habenstein and E. A. Christ, *Professionalizer, Traditionalizer, and Utilizer* (Columbia: University of Missouri, 1955), a study of general-duty nurses; L. Reissman and J. H. Rohrer, *Change and Dilemma in the Nursing Profession* (New York: Charles Putnam's Sons, 1957); T. H. McCormack, "The Druggists' Dilemma: Problems of a Marginal Occupation," *American Journal of Sociology*, LXI (January, 1956), 308–15; L. H. Orzack and J. R. Uglum, "Sociological Perspectives of the Profession of Optometry," *American Journal of Optometry and Archives of American Academy of Optometry*, XXXV (August, 1958), 407–24; and R. W. Habenstein, "The American Funeral Director" (unpublished doctoral dissertation, University of Chicago, 1954).

[12] Hughes, *op. cit.*, pp. 133–37. Cf. A. M. Carr-Saunders and P. A. Wilson, *The Professions* (Oxford: Clarendon Press, 1933), and Caplow, *op. cit.*, pp. 139–40.

[13] As recently as 1948, e.g., a study done by the American College of Hospital Administrators of the careers of 1,000 members showed that they were recruited from 131 diverse occupational backgrounds and varied greatly in education, age, and viewpoint (*Hospital Administration: A Life's Profession* [Chicago: American College of Hospital Administrators, 1948]; C. E. Prall, *Problems of Hospital Administration* [Chicago: Physicians' Record Co., 1948]). The recruitment base for welfare-agency executives was once heterogeneous: public administration, business administration, law, politics. Similar data on urban planners have been reported by Harry Gold in an unpublished study sponsored by the American Institute of Planners.

TABLE 1

THE PROCESS OF PROFESSIONALIZATION*

	Became Full-Time Occupation	First Training School	First University School	First Local Professional Association	First National Professional Association	First State License Law	Formal Code of Ethics	No. of Errors	No. of Ties	Per Cent Error, by Groups†
Established:										
Accounting (CPA)	19th cent.	*1881*‡	*1881*‡	1882	1887	1896	1917	0	1	
Architecture	18th cent.	1865	1868	1815	1857	1897	1909	2	0	
Civil engineering	18th cent.	1819	1847	1848	1852	1908	*ca.* 1910	0	0	
Dentistry	18th cent.	1840§	1867	1844	1840§	1868	1866	3	1	
Law	17th cent.	1784	1817	1802	1878	1732	1908	2	0	
Medicine	*ca.* 1700‖	1765	1779	1735	1847	Before 1780	1912	2	0	21
Others in process, some marginal:										
Librarianship	1732	1887	1897	1885	1876	Before 1917	1938	2	0	
Nursing	17th cent.	1861	1909	1885	1896	1903	1950	1	0	
Optometry	....	1892	1910	1896	1897	1901	*ca.* 1935	1	0	
Pharmacy	1646	1821§	1868	1821§	1852	1874	*ca.* 1850	2	1	
School teaching	17th cent.	1823	1879	1794	1857	1781	1929	3	0	
Social work	1898(?)	1898	1904	1918	1874	1940	1948	1	0	
Veterinary medicine	1803	1852	1879	1854	1863	1886	1866	3	0	27
New:										
City management	1912	1921	1948	After 1914	1914	None	1924	2	0	
City planning	19th cent.	*1909*‡	*1909*‡	1947	1917	1963	1948	2	1	
Hospital administration	19th cent.	*1926*‡	*1926*‡	....	1933	1957	1939	2	1	29
Doubtful:										
Advertising	1841	1900(?)#	1909(?)#	1894	1917	None	1924	1	0	
Funeral direction	19th cent.	*ca.* 1870	1914	1864	1882	1894	1884	3	0	29
							Total errors..	32		25

* Dates concern only events in the United States. Among the sources: *Occupational Licensing in the States* (Chicago: The Council of State Governments, 1952); *Encyclopaedia of Associations* (3d ed.; Detroit: Gale Research Co., 1961); L. E. Blauch (ed.), *Education for the Professions* (Washington, D.C.: Government Printing Office, 1955); *Encyclopedia of the Social Sciences* and other encyclopedias; J. W. Kane, *Famous First Facts* (New York: H. W. Wilson Co., 1950); professional association journals, newsletters, and yearbooks; and specialized histories, official or not. In cases of disagreement, precedence was given to a competent history—e.g., R. W. Habenstein and W. M. Lamers, *The History of American Funeral Direction* (Milwaukee, Wis.: Bulfin Printers, 1955)—or to a date supplied by a professional association cross-checked by one other independent source. I am grateful to Anne Mooney, Ted Cooper, and the headquarters of the dominant professional associations for assistance.

† The total number of dates out of order in group divided by total possible entries in group. The errors for the whole table are 25 per cent of possible entries.

‡ Dates in italics in the same row designate the same event.

§ Two dates in the same row marked with a section mark (§) designate associated events.

‖ Only three or four physicians are known to have resided in the Colonies prior to 1700. From 1607 to 1730 Colonial medical practice was relatively primitive (R. H. Shyrock, *Medicine and Society in America: 1660–1860* [Ithaca, N.Y.: Cornell University Press, 1962], pp. 7, 18).

"(?)" designates best inference from available information.

for *establishment of a training school.* The first teachers, Everett Hughes suggests, are enthusiastic leaders of a movement (like municipal reform among the city managers) or protagonists of some new technique (casework among social workers), or both (probation officer is a new occupation based both on the prison-reform movement and on a new technique).

If these training schools do not *begin* within universities, as they do in the case of hospital administration, city planning, and accounting, they always eventually seek contact with universities, and there is a steady development of standard terms of study, academic degrees, and research programs to expand the base of knowledge. A corps of people who teach rather than practice is an inevitable accompaniment. Higher standards increase the length and cost of training and force earlier commitment among recruits. In the successful case, the standardized training is requisite to entering the occupation.

It should be noted that in four of the six established professions in Table 1, university training schools appear on the scene before national professional associations do. In the less-established professions, the reverse pattern is typical. This underscores the importance of the cultivation of a knowledge base and the strategic innovative role of universities and the early teachers in linking knowledge to practice and creating a rationale for exclusive jurisdiction. Where professionalization has gone farthest, the occupational association does not typically set up a training school; the schools usually promote an effective professional association.

3. Those pushing for prescribed training and the first ones to go through it *combine to form a professional association.* Activists in the association engage in much soul-searching—on whether the occupation is a profession, what the professional tasks are, how to raise the quality of recruits, and so on. At this point they may change the name of the occupation. In this way hospital superintendents have become hospital administrators; relief investigators, caseworkers; newspaper reporters, journalists. The change in label may function to reduce identification with the previous, less-professional occupation. Many, of course, are unsuccessful in this effort. Thus funeral directors or morticians have not escaped the public image of undertaker, and salvage consultants are, alas, still confused with junk dealers.

All this is accompanied by a campaign to separate the competent from the incompetent. This involves further definition of essential professional tasks, the development of internal conflict among practitioners of varying background, and some competition with outsiders who do similar work.

a) It is in the *further self-conscious definition of the core tasks* that a pecking order of delegation occurs. The doctor allocates much of his job to less-trained nurses and laboratory and X-ray technicians; the nurses, as they seek to professionalize, allocate much of their less attractive work to practical nurses, aides, and nurse assistants; and these, in turn, allocate some of their chores to ward helpers. A similar tendency exists among all professional groups in short supply—dentists, teachers, engineers, scientists, and social workers, all of whom are redefining their functions upward and at the same time are sloughing off their dirty work, that is, their less-technical or less-rewarding tasks.

At the same time that the long process of defining and redefining the area of competence goes on, and entry is gradually restricted to those willing to go through the prescribed training, problems of internal morale and interprofessional conflict develop.

b) *The contest between the home guard who learned the hard way and are committed to the local establishment, on the one hand, and the newcomers who took the prescribed course and are committed to practicing the work wherever it takes them* (these newcomers tend to job-hop a bit

more in search of better working arrangements)[14]—this age-old conflict comes to a head. The newcomers see the oldtimers as a block to successful professionalization; the latter see the former as upstarts. If hiring and firing is out of the hands of the professional association, this remains a sore spot, for those who recruit take their choice and may prefer old experience to new training.

c) What is true of internal conflict is also apparent in external relations: *hard competition with neighboring occupations seems to go with these later stages of professionalization.* All occupations in the human-relations field have only tenuous claims to exclusive competence. This results not only from their newness, uncertain standards, and the embryonic state of the social and psychological sciences on which they draw, but also from the fact that the types of problems dealt with are part of everyday living. The lay public cannot recognize the need for special competence in an area where everyone is "expert."

The competition among clinical psychology, psychiatry, and other brands of psychotherapy for the right to practice therapy is typical. But even more clearly technical occupations, like medicine, find themselves doing battle with marginal practitioners—with peaceful absorption as one outcome (osteopathy) and all-out war as another (chiropractic).

4. There will be persistent *political agitation* in order to win the *support of law* for the protection of the job territory and its sustaining code of ethics. Where the area of competence is not clearly exclusive, legal protection of the title will be the aim (certified psychologist, registered engineer); where definition of the area of competence is clearer, then mere performance of the act by someone outside the fraternity may be declared a crime (medical practice laws).

Licensing and certification as weapons in the battle for professional authority are the least important of these events—although legal protection is a hot issue in some newer professions such as social work and clinical psychology. In Table 1 the first state licensure law usually comes toward the end of the process, but the professions clearly cannot claim this as a unique feature of their development: for some time egg-graders have been licensed in Indiana, well-diggers in Maryland, horseshoers in Illinois, plumbers and midwives in many places, notaries public everywhere.

Two lessons may be inferred: the turn toward legal regulation may be an expedient of an occupation "on the make," where internal debate persuades members that it will enhance status or protect jobs; or it may be forced on an occupation by some clear and present danger where public debate persuades lawmakers to protect the layman (e.g., from forged documents or quack "cures"). Medicine, law, and optometry, for instance, were licensed either before they established a university connection or before they formed a national professional association. Legal protection is apparently not an integral part of any "natural history" of professionalism.

5. Eventually rules to eliminate the unqualified and unscrupulous, rules to reduce internal competition, and rules to protect clients and emphasize the service ideal will be embodied *in a formal code of ethics.* Among new or doubtful cases this may appear at the beginning of a push for professional status (e.g., city management, hospital administration, funeral direction), but in ten of thirteen established professions or professions in process it comes at the end (civil engineering, law, medicine).

In sum, there is a typical process by which the established professions have arrived: men begin doing the work full time

[14] Studies of nurses (Habenstein and Christ, *op. cit.*, pp. 87, 134) and of labor union staff experts (H. L. Wilensky, *Intellectuals in Labor Unions: Organizational Pressures on Professional Roles* [Glencoe, Ill.: Free Press, 1956], pp. 133 ff.) show that those with the strongest professional commitments tend to be the most mobile.

and stake out a jurisdiction; the early masters of the technique or adherents of the movement become concerned about standards of training and practice and set up a training school, which, if not lodged in universities at the outset, makes academic connection within two or three decades; the teachers and activists then achieve success in promoting more effective organization, first local, then national—through either the transformation of an existing occupational association or the creation of a new one. Toward the end, legal protection of the monopoly of skill appears; at the end, a formal code of ethics is adopted.

Power struggles and status strivings common to all occupations help to explain deviations from the sequence. The newer and more marginal professions often adopt new titles, announce elaborate codes of ethics, or set up paper organizations on a national level long before an institutional and technical base has been formed. Note, for instance, that the first national professional association comes before the first university school in all seven of the occupations "in process" but in only two of the established professions (dentistry and architecture). There is a hint, too, that newer professions make contact with universities earlier in their careers (the first training school *is* the university-affiliated school in the cases of hospital administration and city planning). Finally, the tactical and strategic situation of an occupation, old *or* new, may demand early licensure or certification whatever the actual level of development of the technique, training, or association. Indeed, in a culture permeated by the idea of professionalism but little touched by its substance, many occupations will be tempted to try everything at once or anything opportunity and expediency dictate. The "professionalization" of labor, management, and commerce is largely of this kind.

BARRIERS TO PROFESSIONALIZATION

Those technical occupations that have gone through the above process and that to some extent adhere to professional norms have acquired extraordinary autonomy—the authority and freedom to regulate themselves and act within their spheres of competence. Elaborate social arrangements, formal and informal, sustain this autonomy.

One might argue that such control as these professions have exercised will decline—or at least that very few additional occupations will acquire the label, "established." For there are some major barriers to professionalization: organizational contexts which threaten autonomy and the service ideal, and bases of knowledge which threaten exclusive jurisdiction. The future of professionalism depends on developments in the organization of both work and knowledge.

Organizational threats to autonomy and the service ideal.—It is commonly assumed that bureaucracy, or more accurately, complex organization, clashes with professionalism. It is sometimes assumed that a client- or customer-orientation is incompatible with a professional orientation. Both assumptions contain truth; both deserve critical examination.

An increasing percentage of professionals work in complex organizations (scientists, engineers, teachers, architects, even lawyers and physicians). These organizations develop their own controls; bosses, not colleagues, rule—or at minimum, power is split among managers, professional experts, and lay boards of directors. The salaried professional often has neither exclusive nor final responsibility for his work; he must accept the ultimate authority of non-professionals in the assessment of both process and product. For instance, compare the fee-taking doctor in general practice with the salaried industrial scientist in product development. The scientist can be bypassed if his company chooses to buy up licenses or subcontract the work to outside laboratories; his work can be terminated, expanded, cut back, or disposed of according to the judgments of laboratory directors responding to outside units or higher-ups; his power and status, reflected in salaries

and tables of organization, are lower than those of the men he serves. In contrast, the doctor's authority and responsibility, often exercised under emergency conditions, is typically final (we speak of "doctor's orders"); he stands at the top of the income-prestige ladder in the medical "industry"; diversification of clients frees him from dependence on any one.[15]

The matter of autonomy is, of course, not that simple. Our scientist may be in a laboratory run by a scientist-administrator and a firm run by former members of the laboratory staff, all of whom pursue administrative policies aimed at preserving his professional autonomy. Or he may simply be in a good market position, his services so much in demand, his mobility chances so great, that his superiors tread lightly when they reject his advice or make suggestions about his work. A study of top staff experts in labor unions found that the men who had received serious outside job offers since their employment in the union had more influence on union decisions than those without such offers.[16] The crux of the issue of autonomy for salaried professionals is whether the organization itself is infused with professionalism (as measured, say, by a large percentage of professionally trained employees and managers) and whether the services of the professionals involved are scarce (as measured by a large number of attractive job offers from the outside). If the answer is "yes" in both instances, the salaried professional may have more autonomy in his work than those self-employed professionals whose relatively low income forces them to scramble for many clients or who depend on the patronage of a few powerful clients.

Does complex organization threaten the service ideal? The cases of professors and clergymen suggest the difficulties in arriving at an answer. Here are two ancient professions, practiced traditionally in well-organized bureaucratic contexts in which service motives have been strongly institutionalized. Are professors less committed to the disinterested search for truth than lawyers are to their clients' rights? Are ministers less concerned about the spiritual and social welfare of their parishioners than doctors are about the health of their patients? We lack the necessary evidence. It is true that one of the main centers of resistance to Nazi terror in Germany between 1933 and 1939 was a bureaucratic profession—personified by Pastor Niemöller of the Protestant Confessional Church and some of the leading Catholic clergy.[17] But it is also true that many salaried professors in Nazi Germany prostituted their scholarship to ends which they knew were false. Teachers generally were among the earliest and most enthusiastic recruits for the Nazi party. At the same time, however, fee-taking lawyers were subverting the rule of law and fee-taking physicians were conducting bizarre "medical experiments" in the concentration camps. "Bureaucratic" or "free," professionals in a totalitarian context do not come out well—especially when, as in the Germany of the early 1930's, their careers are threatened by unemployment and political crises.

How do salaried professionals compare with their independent colleagues in a pluralist society—especially when their services are in demand and they have frequent occasion to choose between self-interest and the interests of their clients? That commercial contexts for professional practice such as real estate agencies, advertising firms, and banks frequently put the establishment's interests before the client's requires no notice. But such non-profit service workshops

[15] Cf. W. Kornhauser, *Scientists in Industry* (Berkeley: University of California Press, 1962). A comparison of the organization of "craft production" and "mass production" leads to similar conclusions. In mass production both product and process are planned in advance by persons not on the work crew, while in craft production centralized planning of tasks is abandoned in favor of decisions made by the work crew based on their training and occupational culture (Stinchcombe, *op. cit.*).

[16] Wilensky, *Intellectuals* [illegible] *op. cit.*, pp. 222 ff.

[17] E. K. Bramstedt, *Dictatorship and Political Police* (New York: Oxford University Press, 1945), pp. [illegible]

as schools, universities, hospitals—alike in their broad community base and ideology—also acquire a life of their own and that life is not always fully described by the technical service ideal.

The image of rational, technical work routines aimed solely at client welfare is difficult to sustain in many service establishments, whatever the occupation. Medical occupations in hospitals highlight the dilemma. Some hospitals are operated frankly for the profit of their owners. And in all hospitals, patients come to feel that much of what goes on is dictated by non-medical considerations—that many rules for patient management are designed for the convenience and comfort of the staff. The feeling is strongest where the disease is chronic and the stay in the hospital long.[18]

The difficulty of sustaining the fiction that all effort is bent toward technical service is particularly apparent in the mental hospitals described by Erving Goffman,[19] and the problems of maintaining professional status for any of the personnel are accordingly intensified. Even where the inside view made possible by long-term incarceration does not give the patient the impression that medical care is sometimes a subordinate aim of the hospital, the point may be dramatized in (1) the inevitable conflict between the needs of patients and the needs of teaching and research programs; and (2) the restriction of communication intrinsic to all status systems ("I'm not going to call Dr. Smith," says the nurse to the nurse's aide, as the patient gasps for breath. "It'll be my neck if nothing's wrong with her").

All this is not to say that the hospital should be geared for full-speed production: this would mean more examinations, tests, conferences, more irregular work (examinations and surgery would bunch up), less leisure, even less sleep, perhaps lowered staff morale and increased labor turnover. It does imply, however, that service workshops, profit and non-profit private and public, organize their work in ways that protect the income, security, and well-being of their most valued personnel—and that where such institutional considerations are prominent, the technical service ideal will be threatened, whatever the anxious effort to preserve it.

In brief, perhaps bureaucracy enfeebles the service ideal more than it threatens professional autonomy. Both salaried *and* self-employed professionals are vulnerable to loss of autonomy when demand for service is low and dependence on powerful clients or bosses unreceptive to independent professional judgment is high. But where comfortable organizational routines take command, the salaried professional (or the fee-taking professional affiliated with a service workshop) may lose sight of client needs more quickly than his solo brother.

Threats to exclusive jurisdiction.—A major barrier to the professionalization of many occupations "on the make," aside from organizational threats to autonomy and the service ideal, is the nature and structure of their base of knowledge and doctrine. If the technical base of an occupation consists of a vocabulary that sounds familiar to everyone (social science and the arts of administration) or if the base is scientific but so narrow that it can be learned as a set of rules by most people, then the occupation will have difficulty claiming a monopoly of skill or even a roughly exclusive jurisdiction. In short, there may be an optimal base for professional practice—neither too vague nor too precise, too broad nor too narrow.

Many new or aspiring professions face

[18] A sensitive study of TB hospitals that focuses on what is urgent to the patient, his timetable of progress, notes that crucial decisions to discharge the patient, schedule him for surgery, promote him to a new activity classification, when made on Friday, are commonly not reported to the patient until Monday. If an X-ray is defective, physicians at a discharge conference will not order another but will instead put off the decision in this case until the next routine conference. Surgery, of course, may be put off for weeks, even months, for the sake of maintaining a regular schedule for the surgeon (J. A. Roth, *Timetables* [Indianapolis: Bobbs-Merrill Co., 1963]).

[19] *Asylums* (Garden City, N.Y.: Doubleday & Co., 1961), pp. 321–86.

this barrier because they are grounded mainly in human-relations skills or some program of reform. The search of social work for a technical base illustrates the dilemma of most "human-relations professions"—knowledge which is at once too broad and too vague.[20] *Paradoxically, knowledge at the other extreme—narrowly restricted, very precise—is also a poor foundation for professional jurisdiction.* When we are able to break a skill down into component elements, prescribe sequences of tasks in a performance, leaving little to the judgment and understanding of the worker, we have a job that can be taught to most people, often in a short time—indeed, a job which is ripe for elimination through programming on a computer. We also have a system of procedures which by its very accessibility is open to critical examination and debate by all comers—and is therefore vulnerable to quick displacement. It is by its nature anti-traditional, never "established."

Professional knowledge, like all knowledge, is to some extent tacit; and it is this that gives the established professions their aura of mystery. As Michael Polanyi reminds us,[21] "there are things that we know but cannot tell": the doctor's recognition of the characteristic appearance of a disease, the taxonomist's recognition of the specimen of a species—these are like our everyday recognition of the identity of a person, the mood of a face; they are acts of understanding complex entities which we cannot fully report. Both experimental evidence (on "subliminal perception") and everyday life (placing a physiognomy) confirm that we can know how to discriminate a complex pattern of things without being able to specify by what features we discriminate it.

The expert may be defined as a man who knows so much that he can communicate only a small part of it. Taxonomist and doctor alike acquire much diagnostic knowledge that they do not learn from books. This element of tacit knowledge in the sciences and professions helps explain their achievement of exclusive jurisdiction; it also helps explain their traditionalism. The client public sees a mystery in the tasks to be performed, a mystery which it is not given to the ordinary man to acquire.[22] Since tacit knowledge is relatively inaccessible, it is also less subject to direct criticism and quick change. The tacit component of their knowledge base is a seldom-recognized cause of the tenacious conservatism of the established professions.

While the nature of the knowledge base is the main reason for the aura of mystery, mysteriousness may also be deliberately used as a tactical device, a means of building prestige and power. The legal profession is a case in point. Like primitive medicine-men who cultivated the occult knowledge of the supernatural, lawyers have always sedulously cultivated the myth of the majesty and the mystery of the law. "The client has to have confidence in the law and the lawyer," a general counsel for a union once told me. "It builds [the top officer's] confidence in the lawyer when he sees us in action. He gets the idea we know what we're talking about."[23] Ferdinand Lundberg, pointing to such metaphysical constructs as "corporate entity," "property rights," "fair value," "conspiracy," "proximate cause," "good faith, bad faith," and "malice,"[24] observes that the law must still be classified with theology and lawyers as political theologians.

In short, the optimal base of knowledge or doctrine for a profession is a combination of intellectual and practical knowing,

[20] For details on the traditions of social reform and social science on which social work practice is based, see Wilensky and Lebeaux, *op. cit.*, pp. 283–334.

[21] "Tacit Knowing: Its Bearing on Some Problems of Philosophy," *Reviews of Modern Physics*, XXXIV (October, 1962), 601.

[22] Archaic usages of the noun "mystery" include "occupation," "craft," "art," "calling," and "skill."

[23] Wilensky, *Intellectuals* . . . , p. 234.

[24] "The Priesthood of the Law," *Harper's Magazine*, CLXXVIII (December, 1938–May, 1939), 515–26.

some of which is explict (classifications and generalizations learned from books, lectures, and demonstrations), some implicit ("understanding" acquired from supervised practice and observation). The theoretical aspects of professional knowledge and the tacit elements in both intellectual and practical knowing combine to make long training necessary and to persuade the public of the mystery of the craft. If an occupation is based on knowledge or doctrine which is too general and vague, on the one hand, or too narrow and specific, on the other, it is not likely to achieve the exclusive jurisdiction necessary to professional authority.

In assessing the barriers to a claim of exclusive jurisdiction, I have so far focused on the technical underpinnings of professional practice, without reference to the changing character of the clientele which must honor the claim. What does the rising educational level of the population imply for professionalism? The facts present a pardox: with education come (1) greater sophistication about matters professional, more skepticism about the certainties of practice, some actual sharing in professional knowledge (the mysteries lose their enchantment); but at the same time (2) more willingness to *use* professional services.[25] The question is open whether a population prone to greater use of professional service which is at the same time more critical and less deferential will mean greater pressure for high standards of technical and ethical performance or an increasing skepticism, a discounting of professional claims, even a tendency to see in the professional just another commercial vendor. We lack the necessary data on the perspectives of potential clients varying in education, information, troubles, and experience with various professions.

[25] E. Friedson, *Patients' Views of Medical Care* (New York: Russell Sage Foundation, 1961), provides some evidence on the first point. What separates the educated patient from the less educated is the former's critical attitude toward the services he is in a position to demand. The less educated do not make trouble for the doctor; they are typically docile and appreciative of what they get.

NEW FORMS OF PROFESSIONALISM

There is another way to view what is happening to professionalism: it is not that organizational revolution destroys professionalism, or that the newer forms of knowledge (vague human-relations skills at one extreme, programmed instructions at the other) provide a poor base for professionalism, but simply that all these developments lead to something new. The culture of bureaucracy invades the professions; the culture of professionalism invades organizations.[26]

To understand the future of professionalism we must grasp the diversity of orientations that now prevail among men with high levels of training and link these orientations to specific attributes of occupation and workplace. If interpenetration of various "bureaucratic" and "professional" cultures is taking place, the individual role orientations appropriate to each should also merge; mixed cultures should be reflected in mixed attitudes of professional people.

Data on the clash of professional careerist, missionary, and client orientations. —Several observers of occupational life—in contexts as diverse as the Wisconsin civil service, the Office of Naval Research, big national labor unions, general hospitals in Missouri, a liberal arts college, and university social science departments—have independently come to very similar conclusions about the types of orientations professional and executive personnel have toward their work (orientations variously labeled "career commitments," "role concepts," "role orientations," "job identifications," or "reference groups").[27] The

[26] Cf. T. H. Marshall, "The Recent History of Professionalism in Relation to Social Structure and Social Policy," *Canadian Journal of Economics and Political Science*, V (August, 1939), 325–40.

[27] L. Reissman, "A Study of Role Conceptions in Bureaucracy," *Social Forces*, XXVII (March, 1949), 305–10; D. Marvick, *Career Perspectives in a Bureaucratic Setting* (Ann Arbor: University of Michigan Press, 1954); Wilensky, *Intellectuals* . . . ; Habenstein and Christ, *op. cit.*; A. Gouldner, "Cosmopolitans and Locals," *Administrative Sci-*

diversity of these orientations suggests modifications of stereotypical portraits of the Organization Man or of the glad-handing, other-directed characters who people the pages of *The Lonely Crowd.* Chart 2, based on these previous studies, outlines the principal types and suggests hypotheses about how they develop.[28]

This research suggests that one of three major types emerging is the professional service expert, whose professional training and commitment to an outside colleague group at once give him motive and strength to resist the demands of the employing organization and, where his orientation is accommodated, make him useful as a source of flexibility, innovation, and reliable intelligence. However, each complex organization, having multiple functions, requires its own distribution of role orientations; and each man's biography is in some respects unique. We would therefore expect diverse workplaces to display central tend-

ence Quarterly, II (1957–58), 281–306, 444–80; and P. Lazarsfeld and W. Thielens, Jr., *The Academic Mind* (Glencoe, Ill.: Free Press, 1958).

[28] Cf. Wilensky, *Intellectuals . . .*, pp. 111–74, 313–17.

CHART 2

SOCIAL STRUCTURE, LIFE-HISTORY, AND ROLE ORIENTATION

Type of Role Orientation	Roots in Structure	Roots in Biography
Professional service (or discipline). Highly identified with profession; oriented toward outside colleague group; wants to give competent, objective, technical service of which outside colleagues would approve; accents full use of skills.	Role is technical, demands formal grad. training. Structure is managed by men with professional training and job histories. Org. interests impinge on large no. of outside groups, org. is public relations–sensitive, so it hires specialists in accommodative techniques who can deal with government agencies and others professionally staffed.	Origins: high status categories—e.g., upper-middle class, Protestant. Education: many years of college—esp. professional or graduate school built on undergraduate liberal arts degree. Orderly career. Participates in professional affairs.
Careerist ("Organization Man"). Highly identified with incumbent leadership of his org.; oriented toward career within workplace hierarchy. No ideological commitments, no dilemma-producing non-organizational goals; little professional identification. Wants chance for social mobility, rewards recognized in local community—money, promotions, security.	Role carries prestige in community. Structure provides opportunity for much job progression; career climb associated with residential mobility.*	Origins: middle mass; medium to low status ethnic-religious groups (esp. Catholics). Education: college dropout or 4-year grad. with low exposure to liberal arts; weak graduate training, if any. Less orderly career (e.g., several tries before got on present ladder). Little participation in professional affairs, more in local community.
Missionary. Oriented toward some abstract concept of a social movement; highly identified with an outside political or religious-political group. Sees organization as vehicle for social change fitting private goals—goals derived from past or present participation in social movement.	Role created as end product of social movement (e.g., labor movement→staff of unions; good govt. movement→city managers; political movements→staff of parties, govt. agencies; humanitarian reform movement→welfare occupations, correctional officers, nurses. Role not clearly defined (new because org. is new, new unit in established org., or org. has diffuse purposes), provides chance for innovator.	Origins: marginal (e.g., minority groups such as Jews and Negroes; families often entrepreneurial, broken, or unusually intellectual). Education: broad (via favored colleges, big-city colleges, or self teaching). Career: includes "ideological occupations" (e.g., journalism or adm. or organizational work in reform adm., political party, or little magazines; campus radical). Participation: professional and social action.

* H. L. Wilensky, "Work, Careers, and Social Integration," *International Social Science Journal*, XII (Fall, 1960), 555–56

encies toward one or another role orientation. Among individuals—recruitment, training, and indoctrination being everywhere imperfect, man and job never fitting precisely—we would further expect mixed orientations to be frequent.[29]

In order to test these ideas in three professional groups, I devised an index of *professional-discipline orientation* and another of *careerist orientation*.[30] The indexes are based on the following:

1. A ranking of reference groups on a card, with appropriate variations in phrasing for lawyers, engineers, and professors: "Here are some groups that inevitably judge the quality of professional performance (*show card*). Whose judgment should count most when your overall professional performance is assessed?" "Are there any others on this list whose judgments should count?" Table 2 shows the percentage mentioning each reference group as most important.

2. Free responses to the question, "Different people want different things out of their jobs. What are the things you yourself feel are most important in a job?"

3. A question concerning those professional journals read regularly: "Generally speaking, would you say you read your professional

[29] In a recent study of industrial scientists and engineers, Kornhauser (*op. cit.*) shows that professional orientations are most frequent among producers of research, "organizational" orientations are most common among men who administer the conditions under which research is produced, and mixed orientations typify men who apply and communicate research. The earlier studies mentioned above are consistent, although every one of them turned up large numbers who combine elements from each type.

[30] The analysis is based on detailed interviews with probability samples or universes of six professional groups stratified for various characteristics. The interviews took place in the first half of 1960. Only white males who were in the labor force and who were currently or previously married were interviewed. All had college degrees. The special selection criteria follow (an initial phone contact screened in the eligible lawyers and engineers): (1) *Lawyers*. Selected randomly from the Michigan State Bar Roster and the Martindale-Hubbell Law Directory. Age: 30–55. Family income: at least $8,000 in one of the past five years. All have law degrees and derive half or more of total income from law. (*a*) *Solo lawyers*. A pure type including only individual practitioners or two-man family partnerships in the Detroit area. May be associated with other lawyers but does not share clientele on any permanent basis. $N = 100$ (*b*) *Firm lawyers*. Selected from the nineteen Detroit firms with ten or more partners and associates. House counsel were excluded. $N = 107$. (2) *Professors*. Full-time faculty of arts and sciences colleges in two universities. Rank: assistant professor and up. Age: 29–55. Disciplines: physical sciences (including mathematics) and the humanities. All professors who fit these criteria were interviewed. Excluded: the social sciences and professional schools. Both institutions are large, but not

TABLE 2

Lawyers	Per Cent	Engineers	Per Cent	Professors	Per Cent
a) Clients	43	The consumer of the product or service	34	Students	9
b) Executives or heads of firms (if lawyer is not self-employed)	8 (of firm men)	Immediate superiors in one's company	47	The administration	2
				The department chairman	6
c) Colleagues—other lawyers familiar with one's work	51	Fellow engineers in one's specialty	16	Colleagues in one's own dept.	24
				Colleagues in one's discipline, whatever their affiliation	56
d) Community leaders	—	Community leaders	0	Community leaders active in educational affairs	0
e) Bar association leaders	0	Leaders of professional engineering associations	2		
				The college faculty as a whole	1

journals thoroughly, partially, or do you just glance through them?"

Responses were scored as shown in Table 3 (respondents' total scores ranged from a high of 8 to a low of 1 on professionalism and 5 to 1 on careerism).

As expected, the two indexes are negatively correlated ($r = -.27, p < .01$). Men who score very high on professionalism seldom score high on careerism; men who rate very high on careerism seldom rate high on professionalism. Taking account of the entire range, however, this inverse relationship is not strong, which suggests that mixed types of orientation are typical, consistent with the idea of the interpenetration of bureaucratic and professional cultures.

Where is professionalism most likely to flourish? Table 4 shows that Urban University professors lead (six in ten are high on professionalism), Church University professors and lawyers as groups are medium, and engineers are low (only one in ten comes through strong on professionalism).

These findings again emphasize that (1) bureaucracy is not a necessary bar to professional commitments (e.g., recruitment and administrative policies encourage professionalism at Urban University, not at Church University); (2) occupational training is generally more important than workplace indoctrination as a source of role orientation (e.g., despite contrasting organizational contexts, firm and solo lawyers are similar in professionalism; and engineers, the occupational group with least professional training, are by far the most careerist and least professional). Thus, organizational threats to colleague control, which have received so much attention in debate about the rise and fall of profes-

eminent: (a) *"Church U."* Church-controlled. $N =$ 31. (b) *"Urban U."* A fast-growing state university. $N = 68$. (3) *Engineers.* Had an engineering degree or its equivalent. Age: 30–55. Family income: at least $8,000 in one of the past five years. Generally research and development specialists, supervisors, or executives. Two large enterprises: (*a*) *"Unico."* A unit making one main product subject to great fluctuations in demand. Reputation: a dead end for engineers. $N = 91$. (*b*) *"Diversico."* A unit with diversified operations and a history of stable growth. Reputation: recruiting ground for top executives in central headquarters. $N = 93$.

TABLE 3

PROFESSIONAL-DISCIPLINE ORIENTATION	CAREERIST ORIENTATION
Whose Judgment Should Count	
3. One of the following is *most* important *and* mentions the second: colleagues in own department or discipline for professors, colleages or bar association leaders for lawyers; fellow engineers, leaders of engineering associations for engineers.	3. One of the following is *most* important: department chairman or administration for professors; heads of firms for lawyers, immediate superiors for engineers (Solo lawyers not scored here, score adjusted by weighing things most important in a job twice.)
2. Mentions one as most important, not the other.	2. Mentions one but not as most important.
1. One or both mentioned but not as most important	
0. None of above.	0. None of above.
Things Most Important in a Job	
3. Mentions technical tasks *and* autonomy or service *and* recognition from colleagues, profession, or discipline as things most important.	3. Mentions income or economic security, mobility and status, *and* recognition from superiors as things most important.
2. Technical tasks, *and either* autonomy, service, *or* recognition, etc.	2. Income or economic security *and either* of others but not both.
1. Technical task but neither of the others.	1. Income or economic security but neither of the others.
0. Mentions none of above.	0 Mentions none of above.

Thoroughness of Reading Professional Journals

3. Thorough.
2. Partial.
1. Glances through.

sionalism, are attenuated by occupational training and organizational purpose.

Perhaps more subversive of autonomy and the service ideal are pressures from the non-organizational users of service—where the client is not a boss but just a customer. "The quack," Everett Hughes suggests, "is the man who continues through time to please his customers but not his colleagues."[31] In any work context where the professional lacks strong colleague constraints, the customer's complaints, real or imaginary, are likely to receive prompt and costly attention; his real problems, if they require professional skill, may be overlooked. In the extreme case, the client-oriented practitioner makes a point of maligning the techniques and motives of his professional competitors and, like the proverbial ambulance chaser, solicits work where no work needs doing.

[31] *Op. cit.*, p. 98.

In order to pin down this clash between colleague control and client control, I constructed an index of client orientation, using the questions mentioned above. To receive a high score the respondent must spontaneously give client-oriented responses in "things most important in a job" (i.e., recognition or appreciation from clients, chance to serve clients, enjoy working with clients, or good quality of clientele) *and* rank clients as "most important" in judging the quality of professional performance. A medium score indicates one of the two, a low score, neither.

TABLE 4*

OCCUPATION COUNTS MORE THAN WORKPLACE AS A SOURCE OF PROFESSIONALISM AND CAREERISM; SOLO PRACTICE ENCOURAGES A CLIENT ORIENTATION

(Per Cent)

	Professors			Lawyers			Engineers			Grand Total (N=490)
	Urban U. (N=68)	Church U. (N=31)	Total (N=99)	Firm (N=107)	Solo (N=100)	Total (N=207)	Diversico (N=93)	Unico (N=91)	Total (N=184)	
Index of professional orientation:										
High (6–8)	60%	32	51	27	24	25	13	8	11	25
Medium (4–5)	36	48	39	50	57	53	57	52	55	51
Low (0–3)	4	19	9	24	19	22	30	39	35	24
Total	100%	99	99	101	100	100	100	99	101	100
Index of careerist orientation:										
High (4–5)	1	6	3	7	6	6	25	24	25	12
Medium (2–3)	67	51	62	39	41	40	62	59	60	52
Low (0–1)	31	42	34	55	53	54	13	17	15	35
Total	99	99	99	101	100	100	100	100	100	99
Index of client orientation:										
Medium-High (1–2)	31	29	30	41	61	51	30	39	35	41
Low (0)	69	71	70	59	39	49	70	60	65	59
Total	100	100	100	100	100	100	100	99	100	100

* See pp. 152–53 for measures and samples.

The results confirm the conflict between client and colleague orientations. The negative correlation between professionalism and client orientation ($r = -.33, p < .01$) is somewhat stronger than that between professionalism and organizational careerism. Eighty-four per cent of those who score high (6–8) in professionalism score low (0) in client orientation; 32 per cent

of those high in client orientation are low (0–3) in professional orientation and the rest are medium (4–5).

The variations by occupation and work setting are also consistent with the hypothesis. Table 2 shows that lawyers, the most "independent" of the three professional groups, are medium on professionalism but high on client orientation—a product perhaps of medium-long professional training but weak colleague constraints on the job. Professors rank high on professionalism, low on client orientation—a product perhaps of longer training and stronger colleague constraints. Most important, solo lawyers, while average in professionalism and low in careerism, have a strong edge in client orientation; six in ten score medium or high compared to about three or four in ten of other groups. Anchored in neither organization nor colleague group, the sensitivity of the solo professional gravitates naturally to the customer.

Will the clash of client and colleague diminish or increase in the future? One could argue that the increasing fraction of the labor force in the tertiary sector—service occupations in which client and customer contact is prominent—implies that a client orientation will be an increasing threat to professionalism. Insofar as self-employment encourages a client orientation, however, the professional segments of the service sector are becoming less vulnerable, as they become more salaried. On balance, the *organizational* threat to professionalism, to the extent that it is a threat, is the one that will grow in influence. The interpenetration of organizational controls and professional controls, careerist and professional orientations, remains the central problem for analysis in assessing the future of professionalism. The occasional rise of social movements spawning new occupational groups and missionary orientations is also worth further attention. I will return to these problems below.

Power structure and professional controls.—Just as the clash of professional, careerist, missionary, and client orientation results in a mixed picture so the interpenetration of professional and non-professional controls leads to new structural forms. A preview of these mixed forms of control may be seen in some of the newer, marginal, or would-be professions—first, in occupations which share control with social establishments where careers may lead to managerial positions (e.g., engineering, teaching, librarianship, social work); second, in occupations in which careers do not lead to management but where control is split among professionals, laymen, and administrators (e.g., the many occupations ancillary to medicine such as hospital administration, nursing, pharmacy). The crucial question concerns how much weight professionals, bosses, laymen, or clients carry in decisions regarding standards of entry, performance, reward, and promotion.

In librarianship and teaching, salaried professionals face powerful lay boards who appoint supervisors and administrators from professional ranks. The dominant voices are lay and managerial. Trade unions (the American Federation of Teachers AFL–CIO) have a small percentage of the membership; the leading occupational associations (the National Education Association) include administrators (school principals), emphasize public relations, and eschew collective bargaining. However, where trade unions share jurisdiction with professional associations, there is a tendency for each to copy the organizational forms and strategies of the other. Thus, the AFT has long officially stated that it is against strikes, and the NEA has recently set negotiating goals and indorsed tactics short of the strike—ranging from complaints to the press to national sanctions like withholding contracts for the coming school year.

Among engineers and foremen the same situation prevails, but here an overwhelmingly dominant managerial group, in active opposition to unionism and hostile to autonomous professional groups, has usually

prevented the full development of either.[32] Of course, management domination is sometimes attenuated and, as I have suggested above, a professional group in great demand, with a firm base of independent training, may acquire considerable autonomy even in command-conscious sectors such as industry and the military establishment.[33] And in Europe it is common for management to share control with white-collar unions, some of which have a semi-professional character.

Instead of facing a powerful lay board, or a powerful managerial group, an aspiring occupation may face an entrenched profession. A large number of the quasi-professions which have strived mightily to expand their professional status—pharmacists, nurses, hospital administrators—find themselves in the stultifying shadow of medicine. The doctor is still officially a guest of the hospital, but, like the Man Who Came to Dinner and stayed for several months, he carries heavy weight and indeed, upon occasion, has full charge of the management of daily affairs. In seeking autonomy in the technical performance of their work, hospital administrators, like pharmacists, must travel a rocky road. By legal and informal means medicine resists encroachment on its authority.

This fact reflects a common dilemma in organizational life: the simultaneous necessity of giving autonomy to highly trained specialists and yet giving some occupations the authority to co-ordinate the specialists. Two outcomes seem typical: the layman gets control, as in teaching and librarianship; or professionals administer, as social workers in welfare agencies, clergy in churches, or professors in universities. Hospital administration may be something new: professionals and laymen together hire administrators who then must struggle for the authority to do their co-ordinating job. If the hospital administrator decides to intervene in such touchy matters as the use and payment of salaried medical specialists, the control of the quality of surgery, a death from a new drug or anesthetic, or sometimes even in more routine matters such as scheduling of operations and admissions or the use of proper techniques of sterilization, he is likely to be lectured about interfering with the "sacred doctor-patient relationship."[34] It seems clear that ancillary medical occupations will arrive at an autonomy befitting professional status only at the expense of control now in the hands of physicians and board members who will not readily yield.

All these cases point up the importance of the pre-existing power structure as a barrier to full professionalization and as a determinant of the mixed forms of organization that professional aspirants adopt. To the degree that professionalization is the expedient adoption of professional forms in a struggle for prestige and income, the fact that someone—layman, manager, or professional—got there first is central.

CONCLUSIONS AND IMPLICATIONS

If the marks of a profession are a successful claim to exclusive technical competence and adherence to the service ideal, the idea that all occupations move toward professional authority—this notion of the professionalization of everyone—is a bit of sociological romance. Many occupations which aspire to become professional are in organizational contexts that threaten autonomy and the service ideal: some are overcrowded occupations of low rank where the work is supervised by men without professional training or sympathy; some are practiced

[32] See the symposium on professional workers in industry in *Industrial Relations*, Vol. II (May, 1963), especially G. Strauss, "Professionalism and Occupational Associations," and E. J. Dvorak, "Will Engineers Unionize?"

[33] Commenting on kickback practices in the buying operations of auto manufacturers a purchasing agent said, "Management at Chrysler was able to tell Purchasing from whom to buy and look at the scandal. I bet they couldn't get away with telling a professional accountant to falsify the books" (Strauss, *op. cit.*; cf. Janowitz, *op. cit.*).

[34] Cf. T. Burling, E. M. Lentz, and R. N. Wilson, *The Give and Take in Hospitals* (New York: G. P. Putnam's Sons, 1956).

in service establishments which, in their concern with the comfort and morale of their most valued personnel, lose sight of client needs; others are so commercialized that talk of the service ideal is nonsense. Many occupations rest on a base of knowledge or doctrine which is (1) too general and vague or (2) too narrow and specific for achievement of the exclusive jurisdiction and autonomy of a profession. The first is epitomized by the personnel men, salesmen, junior social workers, and other human-relations specialists who are products of American general education at the college level; the second, by the scores of engineering specialties in the Soviet Union where the regime finds it easy to train and control its technicians by continual narrowing and redivision of traditional engineering curriculums.[35]

Another clue to the obstacles to any marked growth of professionalism is in the difference between the process by which the established professions have achieved their position and the process pursued by occupations aspiring to professional status. In the recent history of professionalism, the organization push often comes before a solid technical and institutional base is formed; the professional association, for instance, typically precedes university-based training schools, and the whole effort seems more an opportunistic struggle for the rewards of monopoly than a "natural history of professionalism."

This is not to say that many occupations which fail to fit the professional model are not developing higher levels of training and performance, an increasingly sober, dutiful dedication to the task, and even some standards of honorable dealing—what T. H. Marshall calls "a modern type of semi-professionalism"[36] and what Shils sees as a happy integration of professional and civil culture.[37] It may also be true that the empirical, critical, rational spirit of science finds its way into an increasing number of occupations. This should not lead us to mistake the rhetoric of professionalism for its substance or to ignore the distinctive features of professional life that sort out the established professions from new and different organizational forms.

The occupational group of the future will combine elements from both the professional and bureaucratic models; the average professional man will combine professional and non-professional orientations; the typical occupational association may be neither a trade union nor a professional association. Mixed forms of control, hybrid organizations—not a straight-line "professionalization of labor"—are the likely outcomes.

Occupational structures now emerging have their individual correlates. As the data above suggest, the role orientations of many professionals reflect a resolution of the clash between the requirements of profession, organization, and social movement. Most obvious, professional orientations rooted in a colleague group will increasingly be found mixed with careerist orientations rooted in a workplace hierarchy. This is one of the costs of specialization and bureaucratization noted by Carr-Saunders as he laments, "no one speaks any more of the learned professions."[38] Less obvious but no less important, the neutral and objective

[35] N. De Witt (*Education and Professional Employment in the U.S.S.R.* [Washington, D.C.: Government Printing Office, 1961], pp. 228 ff.) reports that there are programs for metallurgical engineering specialists in copper and alloy, in lightweight metals or in ferrous metallurgy; for mining engineering specialists in the drilling of petroleum and gas wells or in the exploration of coal deposits; for civil engineering specialists in bridge design, in building large-scale hydrotechnical structures or in erecting industrial buildings. "This fragmentation," he says, "is characteristic of every field of engineering."

[36] Marshall, *op. cit.*, p. 338.

[37] E. Shils, "Demagogues and Cadres in the Political Development of the New States," in L. Pye (ed.), *Communications and Political Development*, (Princeton, N.J.: Princeton University Press, 1963), pp. 68-69, 76.

[38] A. M. Carr-Saunders, "Metropolitan Conditions and Traditional Professional Relationships," in R. M. Fisher (ed.), *The Metropolis in Modern Life* (Garden City, N.Y.: Doubleday & Co., 1955), p. 286.

advice of the "technician professional" will be mixed more and more with a sense of program, of long-run goals and possibilities. In many a corner of the bureaucratic machinery of modern society, one finds what I have elsewhere labeled the "program professional"—the specialist in depth (e.g., experts in social insurance, rehabilitation, public assistance, public finance, housing, race relations, labor disputes settlement) whose professional competence and commitment are beyond question, but whose commitment to particular programs and policies (e.g., health insurance) is just as strong. By virtue of his technical prowess, he makes himself indispensable as a policy adviser. In his job moves—between government and private agencies, civic organizations, foundations, universities—he follows the programs to which both his skills and his social philosophy are bound.[39] The labor staff expert striving to "keep Labor left" supplies ideology and programs for community relations and national political action, the caseworker or groupworker who becomes a supervisor or administrator broadens his ties to the larger community of pressure groups and politicians, and can thereby engage in social action on behalf of his profession. These have their counterparts among lawyers working for minority defense agencies and civil liberties organizations, social scientists working for government agencies, political parties, and congressional committees. End products of broad movements of social reform, these men combine professional standards of work with programmatic sense and constitute an important link between professional culture and civil culture, the man of knowledge and the man of power. As we assess the mixed organizational forms and mixed role orientations of the future, we must attend not only to the barriers to the professionalization of newer occupations, but to the emergence from existing professions of such policy-minded staff experts.

[39] Wilensky, *Intellectuals* . . . , pp. 129–43.

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Legal Evolution and Societal Complexity[1]

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ABSTRACT

Comparative cross-cultural data suggest that legal characteristics occur in a standard sequential order. Mediation, police, and counsel are scaled in a sample of fifty-one societies; and their occurrence bears a systematic relationship to other attributes of the folk-urban continuum. Theoretical implications include possible relationships between damages and mediation, a partial disconfirmation of Durkheim's Division of Labor hypothesis, and a suggestion of some factors affecting the rise of specialized counsel. Practical implications relate to developing societies and, with qualifications, to the evolution of international legal control.

The study of legal evolution has traditionally commended itself to scholars in a variety of fields. To mention only a few, it has been a concern in sociology of Weber[2] and Durkheim;[3] in jurisprudence of Dicey,[4] Holmes,[5] Pound,[6] and Llewellyn;[7] in anthropology of Maine[8] and Hoebel;[9] in legal history of Savigny[10] and Vinogradoff.[11]

There are theoretical and practical reasons for this interest. Legal evolution[12]

[1] The authors are indebted to Arnold S. Feldman, Raoul Naroll, Terrence Tatje, and Robert F. Winch for their helpful comments on this paper. A grant from the Graduate School of Northwestern University aided in the completion of the work.

[2] Max Weber, *Law in Economy and Society*, ed. Max Rheinstein (Cambridge, Mass.: Harvard University Press, 1954). For a discussion and development of Weber's thinking on legal evolution, see Talcott Parsons, "Evolutionary Universals in Society," *American Sociological Review*, XXIX (June, 1964), 350–53.

[3] Émile Durkheim, *The Division of Labor in Society*, trans. George Simpson (Glencoe, Ill.: Free Press, 1947).

[4] A. V. Dicey, *Lectures on the Relation between Law and Public Opinion in England during the Nineteenth Century* (London: Macmillan Co., 1905).

[5] Oliver Wendell Holmes, Jr., *The Common Law* (Boston: Little, Brown & Co., 1881). Holmes's discussion of the place and limitations of historical analysis provides an appropriate background for the present study. "The law embodies the story of a nation's development through many centuries, and it cannot be dealt with as if it contained only the axioms and corollaries of a book of mathematics. In order to know what it is, we must know what it has been, and what it tends to become. But the most difficult labor will be to understand the combination of the two into new products at every stage. The substance of the law at any given time pretty nearly corresponds, so far as it goes, with what is then understood to be convenient; but its form and machinery, and the degree to which it is able to work out desired results depend very much on its past" (pp. 1–2). In stressing history as providing an explanation for procedure rather than substance, Holmes points to those aspects of legal development that—in the present study at least—appear to follow highly uniform sequences of change.

[6] Roscoe Pound, "Limits of Effective Legal Action," *International Journal of Ethics*, XXVII (1917), 150–65; and *Outlines of Lectures on Jurisprudence* (5th ed.; Cambridge, Mass.: Harvard University Press, 1943). See also his *Interpretations of Legal History* (London: Macmillan Co., 1930).

[7] Karl N. Llewellyn, *The Common Law Tradition: Deciding Appeals* (Boston: Little, Brown & Co., 1960).

[8] Sir Henry Maine, *Ancient Law* (London: J. M. Dent, 1917).

[9] E. Adamson Hoebel, *The Law of Primitive Man* (Cambridge, Mass.: Harvard University Press, 1954).

[10] Frederick von Savigny, *Of the Vocation of Our Age for Legislation and Jurisprudence*, trans. Abraham Hayward (London: Littlewood & Co., 1831).

[11] Paul Vinogradoff, *Outlines of Historical Jurisprudence*, Vols. I and II (London: Oxford University Press, 1920–22).

[12] The term "evolution" is used here in the minimal sense of a regular sequence of changes over time

provides an opportunity to investigate the relations between law and other major aspects and institutions of society. Thus Maine explained the rise of contract in terms of the declining role of kinship as an exclusive basis of social organization. Durkheim saw restitutive sanctions replacing repressive ones as a result of the growth of the division of labor and the corresponding shift from mechanical to organic solidarity. Dicey traced the growth of statutory lawmaking in terms of the increasing articulateness and power of public opinion. Weber viewed the development of formal legal rationality as an expression of, and precondition for, the growth of modern capitalism.

For the most part, these writers were interested in the development of legal norms and not in the evolution of legal organization. The latter subject warrants attention for several reasons. As the mechanism through which substantive law is formulated, invoked, and administered, legal organization is of primary importance for understanding the process by which legal norms are evolved and implemented. Moreover, legal organization seems to develop with a degree of regularity that in itself invites attention and explanation. The present study suggests that elements of legal organization emerge in a sequence, such that each constitutes a necessary condition for the next. A second type of regularity appears in the relationship between changes in legal organization and other aspects of social organization, notably the division of labor.

By exploring such regularities intensively, it may be possible to learn more about the dynamics of institutional differentiation. Legal organization is a particularly promising subject from this point of view. It tends toward a unified, easily identifiable structure in any given society. Its form and procedures are likely to be explicitly stated. Its central function, legitimation, promotes crossculturally recurrent instances of conflict with, and adaptation to, other institutional systems such as religion, polity, economy, and family. Before these relationships can be adequately explored, however, certain gross regularities of development should be noted and it is with these that the present paper is primarily concerned.

This article reports preliminary findings from cross-cultural research that show a rather startling consistency in the pattern of legal evolution. In a sample of fifty-one societies, compensatory damages and mediation of disputes were found in every society having specialized legal counsel. In addition, a large majority (85 per cent) of societies that develop specialized police also employ damages and mediation. These findings suggest a variety of explanations. It may be necessary, for instance, for a society to accept the principles of mediation and compensation before formalized agencies of adjudication and control can be evolved. Alternatively or concurrently, non-legal changes may explain the results. A formalized means fo exchange, some degree of specialization, and writing appear almost universally to follow certain of these legal developments and to precede others. If such sequences are inevitable, they suggest theoretically interesting causative relationships and provide a possible basis for assigning priorities in stimulating the evolution of complex legal institutions in the contemporary world.

METHOD

This research employed a method used by Freeman and Winch in their analysis of societal complexity.[13] Studying a sample of forty-eight societies, they noted a Guttman-scale relationship among six items associated with the folk-urban continuum. The

in a given type of unit, in this case, societies. This usage neither implies nor precludes causal links among the items in the sequence. For a discussion of diverse uses of, and reactions to, the term "evolution," see Sol Tax (ed.), *Issues in Evolution* (Chicago: University of Chicago Press, 1960).

[13] Linton C. Freeman and Robert F. Winch, "Societal Complexity: An Empirical Test of a Typology of Societies," *American Journal of Sociology*, LXII (March, 1957), 461–66.

following items were found to fall in a single dimension ranging, the authors suggest, from simple to complex: a symbolic medium of exchange; punishment of crimes through government action; religious, educational, and goverment specialization; and writing.[14]

To permit the location of legal characteristics on the Freeman-Winch scale, substantially the same sample was used in this study. Three societies were dropped because of uncertainty as to date and source of description[15] or because of inadequate material on legal characteristics.[16] Six societies were added, three to cover the legally developed societies more adequately[17] and three to permit the inclusion of certain well-described control systems.[18]

Several characteristics of a fully developed legal system were isolated for purposes of study. These included counsel, mediation, and police. These three characteristics, which will constitute the focus of the present paper,[19] are defined as follows:

counsel: regular use of specialized non-kin advocates in the settlement of disputes

mediation: regular use of non-kin third party intervention in dispute settlement

police: specialized armed force used partially or wholly for norm enforcement.

[14] This ordering has not been reproduced in other studies that followed similar procedures. Freeman repeated the study on another sample and included four of the six items used in the first study. They scaled in a markedly different order, from simple to complex: government specialization, religious specialization, symbolic medium of exchange, writing. The marked change in position of the first and third items appears attributable to changes in definition for these terms (Linton C. Freeman, "An Empirical Test of Folk-Urbanism," [unpublished Ph.D. dissertation, Northwestern University, 1957], pp. 45, 49–50, 80–83). Young and Young studied all six items in a cross-cultural sample of communities, changing only the definition of punishment. Their ordering is somewhat closer to, but not identical with, that found by Freeman and Winch (*op. cit.*). From simple to complex, the items were ordered as follows: punishment, symbolic medium of exchange, governmental specialization, religious specialization, writing, educational specialization (Frank W. and Ruth C. Young, "The Sequence and Direction of Community Growth: A Cross-Cultural Generalization," *Rural Sociology*, XXVII [December, 1962], 374–86, esp. 378–79).

In the present study, we will rely on the Freeman-Winch ratings and orderings, since the samples overlap so heavily. The reader should bear in mind, however, that the order is tentative and contingent upon the specific definitions used in that study.

[15] Southeastern American Negroes and ancient Hebrews.

[16] Sanpoil.

[17] Three societies—Cambodian, Indonesian, and Syrian—were selected from the Human Relations Area Files to increase the number of societies with counsel. The procedure for selection consisted of a random ordering of the societies in the Human Relations Area Files until three with counsel were located in geographically separate regions. These were then examined to determine the presence or absence of other legal characteristics. The random search eliminated the possibility of a bias in favor of societies conforming to the scale type.

The three societies were quota sampled by region to represent a randomly determined three of the following six regions: Asia, Africa, the Middle East, North America, South America, and Oceania. Purposely omitted from the sample were Europe and Russia because they were already represented in the "counsel" type in the Freeman-Winch sample. Selection from different regions was designed to avoid the problem, first noted by Francis Galton, that cross-cultural regularities might be due to diffusion rather than to functional interrelationships. For a discussion of the problem and evidence of the importance of geographical separateness in sampling, see Raoul Naroll, "Two Solutions to Galton's Problem," *Philosophy of Science*, XXVIII (1961), 15–39; Raoul Naroll and Roy G. D'Andrade, "Two Further Solutions to Galton's Problem," *American Anthropologist*, LXV (October, 1963), 1053–67; and Raoul Naroll, "A Fifth Solution to Galton's Problem," *American Anthropologist*, Vol. LXVI (forthcoming).

[18] These three—Cheyenne, Comanche, and Trobrianders—were selected by James C. Miller before the hypothesis was known to him. Selection of both the Comanche and Cheyenne is subject to some criticism on the grounds that they were prone to diffusion, but this hardly seems a serious difficulty in view of the difference in their scale positions. At all events, the coefficients of reproducibility and scalability would not be seriously lowered by eliminating one of the two.

[19] The original study also included damages, imprisonment, and execution. These were dropped from the present analysis, even though this unfortunately limited the scale to three items, to permit focus on statuses rather than sanction. Data on damages will be introduced, however, where relevant to the discussion of restitution.

These three items, all referring to specialized roles relevant to dispute resolution, were found to fall in a near-perfect Guttman scale. Before the central findings are described and discussed, several methodological limitations should be noted.

First, despite efforts by Murdock[20] and others, no wholly satisfactory method has been devised for obtaining a representative sample of the world's societies. Since the universe of separate societies has not been adequately defined, much less enumerated, the representativeness of the sample cannot be ascertained. Nevertheless, an effort has been made to include societies drawn from the major culture areas and from diverse stages of technological development.

Second, societies have been selected in terms of the availability of adequate ethnographic reports. As a result, a bias may have entered the sample through the selection of societies that were particularly accessible—and hospitable—to anthropological observers. Such societies may differ in their patterns of development from societies that have been less well studied.

Third, despite the selection of relatively well-studied societies, the quality of reports varies widely. Like the preceding limitations, this problem is common to all cross-cultural comparisons. The difficulty is mitigated, however, by the fact that the results of this study are positive. The effect of poor reporting should generally be to randomize the apparent occurrence of the variables studied. Where systematic patterns of relationship emerge, as they do in the present research, it would seem to indicate considerable accuracy in the original reports.[21]

Fourth, this study deals with characteristics whose presence or absence can be determined with relative accuracy. In so doing, it may neglect elements of fundamental importance to the basic inquiry. Thus no effort is made to observe the presence of such important phenomena as respect for law, the use of generalized norms, and the pervasiveness of deviance-induced disturbance. Although all of these should be included in a comprehensive theory of legal evolution, they are omitted here in the interest of observational reliability.[22]

[20] George Peter Murdock, "World Ethnographic Sample," *American Anthropologist*, LIX (August, 1957), 664–87.

[21] On this point see Donald T. Campbell, "The Mutual Methodological Relevance of Anthropology and Psychology," in Francis L. K. Hsu (ed.), *Psychological Anthropology* (Homewood, Ill.: Dorsey Press, 1961), p. 347. This inference should be treated with caution, however, in light of Raoul Naroll's observation that systematic observer bias can lead to spurious correlations (*Data Quality Control: A New Research Technique* [New York: Free Press of Glencoe, 1962]).

[22] Determination of the presence of a characteristic was made after a detailed search by Miller of the materials on each society in the Human Relations Area Files. His search began with a thorough reading for all societies of the material filed under category 18, "total culture." (All categories used are described in detail in George P. Murdock *et al.*, *Outline of Cultural Materials* [4th rev. ed.; New Haven, Conn.: Human Relations Area Files, 1961].) This was followed by a search of the annotated bibliography (category 111) to locate any works specifically dealing with legal or dispute settling processes. When found, works of this kind were examined in detail. In addition, materials filed under the following categories were read: community structure (621), headmen (622), councils (623), police (625), informal in-group justice (627), intercommunity relations (628), territorial hierarchy (631), legal norms (671), liability (672), offenses and sanctions (68), litigation (691), judicial authority (692), legal and judicial personnel (693), initiation of judicial proceedings (694), trial procedure (695), execution of justice (696), prisons and jails (697), and special courts (698). If this search did not reveal the presence of the practice or status under investigation, it was assumed absent. The principal sources relied on for these determinations are given in a mimeographed bibliography which will be supplied by the authors on request.

A reliability check on Miller's judgments was provided by Robert C. Scholl, to whom the writers are indebted. Working independently and without knowledge of the hypotheses, Scholl examined a randomly selected third of the total sample. His judgments agreed with those of Miller 88 per cent, disagreed 4 per cent, and he was unable to reach conclusions on 8 per cent of the items. If the inconclusive judgments are excluded, the reliability reaches the remarkable level of 96 per cent.

The use of a single person to check reliability falls short of the desired standard. In a more detailed and extensive projected study of the relationships reported here, we plan to use a set of three independent naive judges. For discussion of the problems involved in judging cross-cultural

Fifth, the Guttman scale is here pressed into service beyond that for which it was developed. Originally conceived as a technique for the isolation of uni-dimensional attitudes, it has also been used as a means of studying the interrelationship of behavior patterns. It should be particularly valuable, however, in testing hypotheses concerning developmental sequences, whether in individuals or in societies.[23] Thus, if we hypothesize that A must precede B, supporting data should show three scale types: neither A nor B, A but not B, and A and B. All instances of B occurring without A represent errors which lower the reproducibility of the scale and, by the same token, throw doubt in measurable degree on the developmental hypothesis.[24] Although the occurrence of developmental sequences ultimately requires verification by the observation of historic changes in given units, substantiating evidence can be derived from the comparative study of units at varying stages of development. The Guttman scale seems an appropriate quantitative instrument for this purpose.

materials see John W. M. Whiting and Irvin L. Child, *Child Training and Personality* (New Haven, Conn.: Yale University Press, 1953), pp. 39–62; and Guy E. Swanson, *The Birth of the Gods* (Ann Arbor: Michigan University Press, 1960), pp. 32–54.

[23] The use of the Guttman scale is extensively treated by Robert L. Carneiro in "Scale Analysis as an Instrument for the Study of Cultural Evolution," *Southwestern Journal of Anthropology*, XVIII (1962), 149–69. In a sophisticated critique of the Carneiro paper, Ward L. Goodenough suggests that quasi-scales may be needed for charting general evolutionary trends and for treating the traits that develop and then fail to persist because they are superseded by functional equivalents ("Some Applications of Guttman Scale Analysis to Ethnography and Culture Theory," *Southwestern Journal of Anthropology*, XIX [Autumn, 1963], 235–50). While the quasi-scale is a desirable instrument for analyzing supersedence, Goodenough appears unduly pessimistic about the possible occurrence of approximately perfect scales, see p. 246. Studies that obtained such scales, in addition to the one reported here, include Freeman and Winch, *op. cit.*; Stanley H. Udy, "'Bureaucratic' Elements in Organizations: Some Research Findings," *American Sociological Review*, XXII (1958), 415–18; Frank W. and Ruth C. Young, "Social Integration and Change in Twenty-four Mexican Villages," *Economic Development and Cultural Change*, VIII (July, 1960), 366–77; and Robert L. Carneiro and Stephen L. Tobias, "The Application of Scale Analysis to the Study of Cultural Evolution," *Transactions of the New York Academy of Sciences*, Series II, XXVI (1963), 196–207.

The suggestion that Guttman scales could be used for discovering and testing temporal sequences was made earlier by Norman G. Hawkins and Joan K. Jackson in "Scale Analysis and the Prediction of Life Processes," *American Sociological Review*, XXII (1957), 579–81. Their proposal referred, however, to individuals rather than societies.

FINDINGS

In the fifty-one societies studied, as indicated in Table 1, four scale types emerged. Eleven societies showed none of the three characteristics; eighteen had only mediation; eleven had only mediation and police; and seven had mediation, police, and specialized counsel. Two societies departed from these patterns: the Crow and the Thonga had police, but showed no evidence of mediation. While these deviant cases merit detailed study, they reduce the reproducibility of the scale by less than 2 per cent, leaving the coefficient at the extraordinarily high level of better than .98.[25] Each characteristic of legal organi-

[24] The developmental inference does not preclude the possibility of reversal of the usual sequence. It merely indicates which item will be added if any is acquired. Cf. S. N. Eisenstadt, "Social Change, Differentiation and Evolution," *American Sociological Review*, XXIX (June, 1964), 378–81. The finding of a scale also does not rule out the possibility that two items may sometimes occur simultaneously, although the existence of all possible scale types indicates that no two items invariably occur simultaneously and that when they occur separately one regularly precedes the other.

[25] This coefficient of reproducibility far exceeds the .90 level suggested by Guttman as an "efficient approximation . . . of perfect scales" (Samuel Stouffer [ed.], *Measurement and Prediction* [Princeton, N.J.: Princeton University Press, 1950]). The coefficient of scalability, designed by Menzel to take account of extremeness in the distribution of items and individuals, far exceeds the .65 level that he generated from a scalability analysis of Guttman's American Soldier data. Herbert A. Menzel, "A New Coefficient for Scalogram Analysis," *Public Opinion Quarterly*, XVII (Summer, 1953), 268–80, esp. 276. The problem of determining goodness of fit for the

TABLE 1

SCALE OF LEGAL CHARACTERISTICS

Society	Counsel	Police	Mediation	Errors	Legal Scale Type	Freeman-Winch Scale Type
Cambodians	x	x	x		3	*
Czechs	x	x	x		3	6
Elizabethan English	x	x	x		3	6
Imperial Romans	x	x	x		3	6
Indonesians	x	x	x		3	*
Syrians	x	x	x		3	*
Ukrainians	x	x	x		3	6
Ashanti		x	x		2	5
Cheyenne		x	x		2	*
Creek		x	x		2	5
Cuna		x	x		2	4
Crow		x		1	2	0
Hopi		x	x		2	5
Iranians		x	x		2	6
Koreans		x	x		2	6
Lapps		x	x		2	6
Maori		x	x		2	4
Riffians		x	x		2	6
Thonga		x		1	2	2
Vietnamese		x	x		2	6
Andamanese			x		1	0
Azande			x		1	0
Balinese			x		1	4
Cayapa			x		1	2
Chagga			x		1	4
Formosan aborigines			x		1	0
Hottentot			x		1	0
Ifugao			x		1	0
Lakher			x		1	2
Lepcha			x		1	3
Menomini			x		1	0
Mbundu			x		1	3
Navaho			x		1	5
Ossett			x		1	1
Siwans			x		1	1
Trobrianders			x		1	*
Tupinamba			x		1	0
Venda			x		1	5
Woleaians			x		1	0
Yakut			x		1	1
Aranda					0	0
Buka					0	0
Chukchee					0	0
Comanche					0	*
Copper Eskimo					0	0
Jivaro					0	0
Kababish					0	1
Kazak					0	0
Siriono					0	0
Yaruro					0	0
Yurok					0	1

* Not included in Freeman-Winch sample.

Coefficient of reproducibility = 1 − 2/153 = 987; coefficient of scalability = 1 − 2/153-120 = .94; Kendall's tau = +.68.

zation may now be discussed in terms of the sociolegal conditions in which it is found.

MEDIATION

Societies that lack mediation, constituting less than a third of the entire sample, appear to be the simplest societies. None of them has writing or any substantial degree of specialization.[26] Only three of the thirteen (Yurok, Kababish, and Thonga) use money, whereas almost three-fourths of the societies with mediation have a symbolic means of exchange. We can only speculate at present on the reasons why mediation is absent in these societies. Data on size, using Naroll's definition of the social unit,[27] indicate that the maximum community size of societies without mediation is substantially smaller than that of societies with mediation.[28] Because of their small size, mediationless societies may have fewer disputes and thus have less opportunity to evolve regularized patterns of dispute settlement. Moreover, smaller societies may be better able to develop mores and informal controls which tend to prevent the occurrence of disputes. Also, the usually desperate struggle for existence of such societies may strengthen the common goal of survival and thus produce a lessening of intragroup hostility.

The lack of money and substantial property may also help to explain the absence of mediation in these societies. There is much evidence to support the hypothesis that property provides something to quarrel about. In addition, it seems to provide something to mediate with as well. Where private property is extremely limited, one would be less likely to find a concept of damages, that is, property payments in lieu of other sanctions. The development of a concept of damages should greatly increase the range of alternative settlements. This in turn might be expected to create a place for the mediator as a person charged with locating a settlement point satisfactory to the parties and the society.

This hypothesis derives support from the data in Table 2. The concept of dam-

TABLE 2

DAMAGES IN RELATION TO LEGAL FUNCTIONARIES

	No Mediation	Mediation Only	Mediation and Police	Mediation, Police, and Counsel	Total
Damages	7	17	10	7	41
No damages	6*	3	1	0	10
Total	13	20	11	7	51

* Includes Thonga, who have neither mediation nor damages, but have police

Guttman scale has still not been satisfactorily resolved (see W. S. Torgerson, *Theory and Methods of Scaling* [New York: John Wiley & Sons, 1958], esp. p. 324). A method utilizing χ^2 to test the hypothesis that observed scale frequencies deviate from a rectangular distribution no more than would be expected by chance is suggested by Karl F. Schuessler, "A Note on Statistical Significance of Scalogram," *Sociometry*, XXIV (September, 1961), 312–18. Applied to these data, Schuessler's Test II permits the rejection of the chance hypothesis at the .001 level. $\chi^2 = 60.985$ *(7df)*.

[26] Statements of this type are based on the ratings in the Freeman-Winch study, as noted in n. 14 above. For societies that did not appear in their sample, we have made our own ratings on the basis of their definitions.

[27] Raoul Naroll, "A Preliminary Index of Social Development," *American Anthropologist*, LVIII (August, 1956), 687–720.

[28] Data were obtained for thirty-nine of the fifty-one societies in the sample on the size of their largest settlement. Societies with mediation have a median largest settlement size of 1,000, while those without mediation have a median of 346. Even eliminating the societies with developed cities, the median largest settlement size remains above 500 for societies with mediation.

ages occurs in all but four of the thirty-eight societies that have mediation and thus appears to be virtually a precondition for mediation. It should be noted, however, that damages are also found in several (seven of thirteen) of the societies that lack mediation. The relationship that emerges is one of damages as a necessary but not sufficient condition for mediation. At present it is impossible to ascertain whether the absence of mediation in societies having the damage concept results from a simple time lag or whether some other factor, not considered in this study, distinguishes these societies from those that have developed mediation.

POLICE

Twenty societies in the sample had police—that is, a specialized armed force available for norm enforcement. As noted, all of these but the Crow and Thonga had the concept of damages and some kind of mediation as well. Nevertheless, the occurrence of twenty societies with mediation but without police makes it clear that mediation is not inevitably accompanied by the systematic enforcement of decisions. The separability of these two characteristics is graphically illustrated in ethnographic reports. A striking instance is found among the Albanian tribesmen whose elaborately developed code for settling disputes, Lek's Kanun, was used for centuries as a basis for mediation. But in the absence of mutual agreements by the disputants, feuds often began immediately after adjudication and continued unhampered by any constituted police.[29]

From the data it is possible to determine some of the characteristics of societies that develop police. Eighteen of the twenty in our sample are economically advanced enough to use money. They also have a substantial degree of specialization, with full-time priests and teachers found in all but three (Cheyenne, Thonga, and Crow), and full-time governmental officials, not mere relatives of the chief, present in all but four (Cuna, Maori, Thonga, and Crow).

Superficially at least, these findings seem directly contradictory to Durkheim's major thesis in *The Division of Labor in Society*. He hypothesized that penal law—the effort of the organized society to punish offenses against itself—occurs in societies with the simplest division of labor. As indicated, however, our data show that police are found only in association with a substantial degree of division of labor. Even the practice of governmental punishment for wrongs against the society (as noted by Freeman and Winch) does not appear in simpler societies. By contrast, restitutive sanctions—damages and mediation—which Durkheim believed to be associated with an increasing division of labor, are found in many societies that lack even rudimentary specialization. Thus Durkheim's hypothesis seems the reverse of the empirical situation in the range of societies studied here.[30]

[29] Margaret Hasluck, *The Unwritten Law in Albania* (Cambridge: Cambridge University Press, 1954).

[30] A basic difficulty in testing Durkheim's thesis arises from his manner of formulating it. His principal interest, as we understand it, was to show the relationship between division of labor and type of sanction (using type of solidarity as the intervening variable). However, in distinguishing systems of law, he added the criterion of organization. The difficulty is that he was very broad in his criterion of organization required for penal law, but quite narrow in describing the kind of organization needed for non-penal law. For the former, the "assembly of the whole people" sufficed (*op. cit.*, p. 76); for the latter, on the other hand, he suggested the following criteria: "restitutive law creates organs which are more and more specialized: consular tribunals, councils of arbitration, administrative tribunals of every sort. Even in its most general part, that which pertains to civil law, it is exercised only through particular functionaries: magistrates, lawyers, etc., who have become apt in this role because of very special training" (p. 113). In thus suggesting that restitutive law exists only with highly complex organizational forms, Durkheim virtually insured that his thesis would be proven—that restitutive law would be found only in complex societies.

Such a "proof," however, would miss the major point of his argument. In testing the main hy-

COUNSEL

Seven societies in the sample employ specialized advocates in the settlement of disputes. As noted, all of these societies also use mediation. There are, however, another thirty-one societies that have mediation but do not employ specialized counsel. It is a striking feature of the data that damages and mediation are characteristic of the simplest (as well as the most complex) societies, while legal counsel are found only in the most complex. The societies with counsel also have, without exception, not only damages, mediation, and police but, in addition, all of the complexity characteristics identified by Freeman and Winch.

It is not surprising that mediation is not universally associated with counsel. In many mediation systems the parties are expected to speak for themselves. The mediator tends to perform a variety of functions, questioning disputants as well as deciding on the facts and interpreting the law. Such a system is found even in complex societies, such as Imperial China. There the prefect acted as counsel, judge, and jury, using a whip to wring the truth from the parties who were assumed a priori to be lying.[31] To serve as counsel in that setting would have been painful as well as superfluous. Even where specialized counsel emerge, their role tends to be ambiguous. In ancient Greece, for instance, counsel acted principally as advisors on strategy. Upon appearance in court they sought to conceal the fact that they were specialists in legal matters, presenting themselves merely as friends of the parties or even on occasion assuming the identity of the parties themselves.[32]

At all events, lawyers are here found only in quite urbanized societies, all of which are based upon fully developed agricultural economies. The data suggest at least two possible explanations. First, all of the sample societies with counsel have a substantial division of labor, including priests, teachers, police, and government officials. This implies an economic base strong enough to support a variety of secondary and tertiary occupations as well as an understanding of the advantages of specialization. Eleven societies in the sample, however, have all of these specialized statuses but lack specialized counsel. What distinguishes the societies that develop counsel? Literacy would seem to be an important factor. Only five of the twelve literate societies in the sample do not have counsel. Writing, of course, makes possible the formulation of a legal code with its advantages of forewarning the violator and promoting uniformity in judicial administration. The need to interpret a legal code provides a niche for specialized counsel, especially where a substantial segment of the population is illiterate.[33]

pothesis it would seem preferable, therefore, to specify a common and minimal organizational criterion, such as public support. Then the key question might be phrased: Is there a tendency toward restitutive rather than repressive sanctions which develops as an increasing function of the division of labor? Although our present data are not conclusive, the finding of damages and mediation in societies with minimal division of labor implies a negative answer. This suggests that the restitutive principle is not contingent on social heterogeneity or that heterogeneity is not contingent on the division of labor.

[31] Sybille van der Sprenkel, *Legal Institutions in Manchu China* (London: Athlone Press, 1962). See also Ch'ü T'ung-tsu, *Law and Society in Traditional China* (Vancouver, B.C.: Institute of Pacific Relations, 1961).

[32] A. H. Chroust, "The Legal Profession in Ancient Athens," *Notre Dame Law Review*, XXIX (Spring, 1954), 339–89.

[33] Throughout the discussion, two sets of explanatory factors have been utilized. The observed pattern could be due to an internal process inherent in legal control systems, or it could be dependent upon the emergence of urban characteristics. It does seem clear, however, that the legal developments coincide to a considerable extent with increased "urbanism" as measured by Freeman and Winch. Evidence for this assertion is to be found in the correlation between the Freeman-Winch data and the legal scale types discerned. For the forty-five societies appearing in both samples, the rank correlation coefficient (Kendall's tau) between positions on the legal and urbanism scales is + .68. While

CONCLUSIONS

These data, taken as a whole, lend support to the belief that an evolutionary sequence occurs in the development of legal institutions. Alternative interpretations are, to be sure, not precluded. The scale analysis might fail to discern short-lived occurrences of items. For instance, counsel might regularly develop as a variation in simple societies even before police, only to drop out rapidly enough so that the sample picks up no such instances. Even though this is a possibility in principle, no cases of this kind have come to the authors' attention.

Another and more realistic possibility is that the sequence noted in this sample does not occur in societies in a state of rapid transition. Developing societies undergoing intensive cultural contact might provide an economic and social basis for specialized lawyers, even in the absence of police or dispute mediation. Until such societies are included in the sample, these findings must be limited to relatively isolated, slowly changing societies.

The study also raises but does not answer questions concerning the evolution of an international legal order. It would be foolhardy to generalize from the primitive world directly to the international scene and to assume that the same sequences must occur here as there. There is no certainty that subtribal units can be analogized to nations, because the latter tend to be so much more powerful, independent, and relatively deficient in common culture and interests. In other ways, the individual nations are farther along the path of legal development than subtribal units because all of them have their own domestic systems of mediation, police, and counsel. This state of affairs might well provide a basis for short-circuiting an evolutionary tendency operative in primitive societies. Then too, the emergent world order appears to lack the incentive of common interest against a hostile environment that gave primitive societies a motive for legal control. Even though the survival value of a legal system may be fully as great for today's world as for primitive societies, the existence of multiple units in the latter case permitted selection for survival of those societies that had developed the adaptive characteristic. The same principle cannot be expected to operate where the existence of "one world" permits no opportunity for variation and consequent selection.

Nonetheless, it is worth speculating that some of the same forces may operate in both situations.[54] We have seen that damages and mediation almost always precede police in the primitive world. This sequence could result from the need to build certain cultural foundations in the community before a central regime of control, as reflected in a police force, can develop. Hypothetically, this cultural founda-

this coefficient suggests a close relationship between the two processes, it does not justify the assertion that legal evolution is wholly determined by increasing urbanism. A scatter diagram of the interrelationship reveals that legal characteristics tend to straddle the regression line for five of the seven folk-urban scale positions, omitting only scale types 2 (punishment) and 3 (religious specialization). This suggests that some other factor might emerge upon further analysis that would explain why roughly half of the societies at each stage of urbanism appear to have gone on to the next stage of legal evolution while the others lag behind. A promising candidate for such a factor is the one located by Gouldner and Peterson in their cross-cultural factor analysis of Simmons' data and described by them as "Apollonianism" or "Norm-sending" (Alvin W. Gouldner and Richard A. Peterson, *Technology and the Moral Order* [Indianapolis: Bobbs-Merrill Co., 1962], pp. 30–53).

To test whether the legal sequence has a "dynamic of its own," it would seem necessary to examine the growth of legal systems independent of folk-urban changes, as in subsystems or in societies where the process of urbanization has already occurred. The data covered here do not permit such a test.

[54] For an interesting attempt to develop a general theory of legal control, applicable both to discrete societies and to the international order, see Kenneth S. Carlston, *Law and Organization in World Society* (Urbana: University of Illinois Press, 1962).

tion might include a determination to avoid disputes, an appreciation of the value of third-party intervention, and the development of a set of norms both for preventive purposes and as a basis for allocating blame and punishment when disputes arise. Compensation by damages and the use of mediators might well contribute to the development of such a cultural foundation, as well as reflecting its growth. If so, their occurrence prior to specialized police would be understandable. This raises the question as to whether the same kind of cultural foundation is not a necessary condition for the establishment of an effective world police force and whether, in the interest of that objective, it might not be appropriate to stress the principles of compensatory damages and mediation as preconditions for the growth of a world rule of law.

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Mathematical Methods for the Study of Systems of Groups[1]

Leo A. Goodman

ABSTRACT

This article presents mathematical methods for analyzing models pertaining to systems of groups. Applying these methods to the models proposed earlier by Coleman and James and by White, we find, for example, that their main conclusions require modification. Coleman and James claim to have derived the truncated-Poisson (T-P) formula for the equilibrium distribution of the sizes of groups in their model of a system of groups, and White claims that a wide variety of such models will have equilibrium size distributions that can be described by the T-P formula. We show here that this formula will *not* describe the equilibrium size distributions for the models given in these earlier articles, except under very special circumstances. Different formulas are derived herein. Although the models presented by Coleman and James and by White do pertain to systems of groups, we show that their mathematical manipulations did in effect replace these models by other models that do not pertain to systems of groups. In addition, we present a model for a system of groups which is different from those in the earlier literature and which is, in some respects, preferable to the earlier models. We also present statistical methods for analyzing data on the distribution of group sizes. We find, for example, that use of the χ^2 goodness-of-fit test and related tests can be justified when the model introduced herein is under study, but no such justification can be found for the earlier models.

Data on the frequency distribution of the sizes of "freely forming" groups in various settings (e.g., children's playgrounds, stores, public gatherings, sidewalks) have been collected and analyzed by James[2] and reanalyzed by Coleman and James.[3] In a number of different situations, these frequency distributions can be fitted by a simple formula, namely, the truncated-Poisson (T-P) formula, which we shall describe later herein. In an attempt to "explain why the distributions take the form they do," Coleman and James presented a stochastic model pertaining to the movement of individuals into and out of groups, and they claimed to have derived the T-P formula as a description of the equilibrium distribution of the sizes of groups in their model. In a subsequent article, White reanalyzed the model introduced by Coleman and James, and he too claimed to have derived the T-P formula as a description of the equilibrium size distribution for this model.[4] (White found fault with their derivation, but he actually arrived at the same formula as that given by Coleman and James.) In addition, he introduced a number of different stochastic models of the movement of individuals into and out of groups, and he claimed that these models also had equilibrium size distributions that could be described by the T-P formula. Consequently, he criticized the earlier work by Coleman and James on the grounds that these authors had assumed their model to be valid because their formula for the equilibrium size distribution of the model actually did fit the observed frequency distributions of group sizes, whereas they should have noted that the validity of their model was open to question because other quite different models would also have led, according to White, to the same kind of equilibrium size distribution formula. By a more detailed mathematical analysis of these models, we shall show in the present

[1] This research was supported in part by Research Grant No. NSF-G 10368 from the Division of Mathematical, Physical, and Engineering Sciences of the National Science Foundation.

[2] John James, "A Preliminary Study of the Size Determinant in Small Group Interaction," *American Sociological Review*, XVI (1951), 474–77; "The Distribution of Free-forming Small Group Size," *American Sociological Review*, XVIII (1953), 569–70.

[3] James S. Coleman and John James, "The Equilibrium Size Distribution of Freely-forming Groups," *Sociometry*, XXIV (1961), 36–45.

[4] Harrison White, "Chance Models of Systems of Casual Groups," *Sociometry*, XXV (1962), 153–72.

article that the claims made by Coleman and James and those made by White require modification.

The stochastic models introduced by Coleman and James and by White specify in probabilistic terms the possible movements, at any moment of time, of the individuals in the system of groups, that is, the movements of the individuals into and out of the groups in the system. (We shall describe two of these models later herein.) The probabilistic specification of the possible movements of individuals is different for the different models. For the Coleman-James model and also for White's models, the possible movements that can take place in the system in any particular small interval of time depend upon the actual situation in existence in the system at the beginning of that time interval. For example, in some of these models, the number of individuals who might possibly join one of the groups in the system in any particular small interval of time will depend upon the actual number of individuals in the system who, at the beginning of that time interval, are not members of any group. Thus, the probability that a group will be of a given size at the end of a particular small time interval will depend not only upon its size at the beginning of that time interval but also upon the actual situation in existence in the rest of the system (in particular, upon the number of individuals who are not members of any group) at that time. Stochastic models of this kind, where the state in which a group is at the end of any small time interval is contingent not only upon its state at the beginning of that time interval but also upon the actual state of the rest of the system at that time, have been referred to as "sociological" sorts of models.[5] We would say that models of this kind pertain to *systems* of interdependent groups, whereas models of the kind where the state in which a group is at the end of a particular small time interval is not contingent upon the state of the rest of the system, but only upon the state of that particular group at the beginning of the time period, are models that do *not* pertain to *systems* of groups.[6] Coleman and James's model and White's various models do indeed pertain to systems of groups, but we shall show in the present article that the mathematical manipulations they actually made, in order to obtain the T-P formula, did in fact ignore this aspect of their models. In effect, their mathematical manipulations implicitly replaced their stochastic models pertaining to systems of groups by other models which are mathematically simpler but which do not pertain to systems of groups; the formula they derived is suited to the latter models rather than to the former. Analyzing one of White's models in detail, we shall show that, to the extent that this model actually pertains to a system of groups, the T-P formula will not serve even as an approximation to its equilibrium size distribution. On the other hand, in some special situations the models referred to above may possibly be replaceable by certain other models that do not pertain to systems of groups, and in some of these cases the T-P formula may possibly be obtained as an approximation to the equilibrium size distribution of these models.[7]

Some of the mathematical methods of analysis presented herein can be applied to

[5] See James S. Coleman, "Comments on Harrison White, 'Chance Models of Systems of Casual Groups,'" *Sociometry*, XXV (1962), 172–76. Coleman distinguishes between "processes which are sociological in a strong sense" (i.e., stochastic models of this kind) and those which are not.

[6] The above remarks serve as a definition of what we shall mean when we say that a particular model does or does not "pertain to systems of groups." Other ways of expressing this concept, for example Coleman's use of the term "sociological," may in some respects be preferable and in other respects not. In any case, the concept we are defining is clear. When we say that a particular model does "pertain to systems of groups," we do *not* mean that any observed system of groups can be described by that model. We shall discuss later the problem of determining whether or not a particular observed system of groups can be described by a particular model.

[7] In some special situations, the dependence between groups in a system may be negligible. In describing these situations, the term "system" may be inappropriate, since we have in these cases a set of (approximately) independent groups rather than a "system" of interdependent groups.

various kinds of models in addition to those introduced in the earlier literature. (These methods are also applicable to the analysis of models pertaining to other kinds of social phenomena, not only to systems of groups.) In the present article we shall present and analyze a stochastic model (viz., the emigration-immigration [E-I] model) which is quite different from those introduced by Coleman and James and by White, although it also pertains to systems of groups. In contrast to the earlier stochastic models, we find, for example, that the E-I model will have, under certain circumstances (in which the interdependence between the groups in the system need not be negligible), a truncated-Poisson equilibrium size distribution.

Finally, statistical methods for analyzing data on the distribution of group sizes will be presented herein. The use of the χ^2 goodness-of-fit test will be discussed; and methods will be developed for fitting the T-P formula, when appropriate, to an observed distribution of group sizes. This test can be justified when the model introduced here (i.e., the E-I model) is under study, for no such justification can be found for the models presented in the earlier literature. In addition to the χ^2 test, we shall present other statistical tests particularly suited to the analysis of observed group-size distributions pertaining to the E-I model. We shall also derive different formula which are suited to the analysis of observed group-size distributions pertaining to the models presented by Coleman and James and by White.

THE TRUNCATED-POISSON FORMULA

The truncated-Poisson (T-P) distribution (truncated at zero) is a probability distribution associated with the integers 1, 2, ...[8] The probability associated with the integer i is

$$P_i = (\phi^i/i!)/(e^\phi - 1)\,, \quad \text{for } i = 1, 2, \ldots, \tag{1}$$

where the parameter ϕ is a positive constant. The T-P distribution can be defined by equation (1) or equivalently by the following two conditions:

$$P_{i+1} = P_i\phi/(i+1)\,, \quad \text{for } i = 1, 2, \ldots, \tag{2}$$

and

$$\sum_{i=1}^{\infty} P_i = 1\,. \tag{3}$$

We shall use both definitions of the T-P distribution. We note here that the mean of the T-P distribution is

$$\sum_{i=1}^{\infty} iP_i = \phi/[1 - e^{-\phi}]\,, \tag{4}$$

and that equations (2) and (4) shed light on the meaning of the parameter ϕ.

REANALYSIS OF THE ATTRACTOR MODEL

White introduced models for both closed and open systems of groups, whereas the Coleman-James model pertains only to a closed system. In a closed system, the individuals in the system cannot leave it, and no new individuals can enter it from the outside environment. In an open system, the individuals who are in it can leave, and individuals from the outside environment can enter it. White discussed the Coleman-James model (which he called the "transit pool *a* model for a closed system") and other models for closed systems after first discussing models for open systems. His methods of analyzing all of these models are similar to the methods he used first in his analysis of

[8] This distribution is a modification of the Poisson distribution, which is associated with the numbers 0, 1, 2, . . . and assigns positive probabilities to each of these numbers. The T-P distribution (truncated at zero) is the conditional distribution obtained from the Poisson distribution when positive probabilities are assigned only to the integers 1, 2, ... For an elementary discussion of the Poisson distribution, see, e.g., S. S. Wilks, *Elementary Statistical Analysis* (Princeton, N.J.: Princeton University Press, 1948), chap. vii; and for a somewhat more advanced discussion see William Feller, *An Introduction to Probability Theory and Its Applications*, I (2d ed.; John Wiley & Sons, 1957), chap. vi.

the "n-group attractor model," a model for an open system. This model and its analysis he discusses in some detail, and he states that the results for all other models, which he presents in brief, were arrived at by mathematical manipulations similar to those he made in his analysis of the attractor model. We shall now analyze this model in more detail and later discuss the Coleman-James model, which is in some respects similar to it.

The n-*group attractor model.*—This is a model for a system consisting of n groups of individuals, where an individual can depart from a group in which he was a member, and individuals from outside the system (i.e., individuals in the outside environment) can join groups in this system. Let $n_i(t)$ denote the number of groups of size i ($i = 1, 2, \ldots$) in the system at time t.[9] Then

$$\sum_{i=1}^{\infty} n_i(t) = n .$$

For this model the probabilistic specification of the possible movements, at any moment of time, of individuals into and out of groups in the system is as follows: With respect to a group of size i at time t, the probability is $i\mu h$ (for $i > 1$) that one individual will depart from it during the small time interval from time t to $t + h$; the probability is $ahn_1(t)$ that one individual (from the outside environment) will join it during this time interval; the probability that there will be neither a departure nor an arrival during this time interval is $1 - i\mu h - ahn_1(t)$ for $i > 1$, and $1 - ahn_1(t)$ for $i = 1$.[10] In this model, an individual who departs from the group in which he was a member also leaves the system, and the departure rate for a group of size i ($i > 1$) is proportional to i. Individuals from the outside environment join the system at time t at a rate proportional to $n_1(t)$, the number of single individuals in the system at time t. Note that $n_1(t)$ is a random variable in this stochastic model.

The expected number of groups of each size.—Let $E\{n_1(t)\}$ denote the expected value of the number $n_1(t)$ of groups of size one (i.e., the number of single individuals) in the system at time t. (Since $n_1(t)$ is a random variable and not a constant, its variance will be positive.) Similarly, let $E\{n_i(t)\}$ denote the expected value of $n_i(t)$. From the probabilistic specifications given above for the attractor model, we find that the expected value, $E\{n_i(t + h)\}$, of the number of groups of size i ($i = 1, 2, \ldots$) at time $t + h$ can be written as

$$E\{n_1(t + h)\} = 2\mu h E\{n_2(t)\} + E\{n_1(t)[1 - an_1(t)h]\} , \quad (5)$$

and for $i > 1$

$$E\{n_i(t + h)\} = (i + 1)\mu h E\{n_{i+1}(t)\} + E\{n_i(t)[1 - i\mu h - an_1(t)h]\} + E\{n_{i-1}(t)an_1(t)h\} . \quad (6)$$

From equations (5) and (6) we see that the derivative, $dE\{n_i(t)\}/dt$, of $E\{n_i(t)\}$ with respect to t can be written as

$$\frac{dE\{n_1(t)\}}{dt} = 2\mu E\{n_2(t)\} - aE\{n_1^2(t)\} , \quad (7)$$

and for $i > 1$

$$\frac{dE\{n_i(t)\}}{dt} = (i + 1)\mu E\{n_{i+1}(t)\} - i\mu E\{n_i(t)\} - aE\{n_i(t)n_1(t)\} + aE\{n_{i-1}(t)n_1(t)\} . \quad (8)$$

Equations (7) and (8), which reproduce formulas given by White, will now be used to show that the form of the $E\{n_i(t)\}$ (for $i = 1, 2, \ldots$) in equilibrium cannot be described by the T-P formula.

When there is stochastic equilibrium,

[9] A "group of size 1" is understood to be an individual in the system who has not joined with others in the system to form a group consisting of more than himself. We shall, for convenience, refer to such individuals as "single" individuals.

[10] These probabilities relating to the time interval from t to $t + h$ are assumed independent of the past history of the system.

then the left side of the equations (7) and (8) will be zero, which will yield

$$2\rho E\{n_2\} - E\{n_1^2\} = 0\,, \qquad (9)$$

and for $i > 1$

$$(i+1)\rho E\{n_{i+1}\} - E\{n_i n_1\} = i\rho E\{n_i\} - E\{n_{i-1} n_1\}\,, \qquad (10)$$

where $\rho = \mu/a$ and where $n_i(t)$ has been replaced by n_i. From equations (9) and (10) we find that

$$E\{n_{i+1}\} = E\{n_i n_1\}/[\rho(i+1)]\,, \quad \text{for } i = 1, 2, \ldots. \qquad (11)$$

(Note that the relationship [11] permits us to "predict" $E\{n_{i+1}\}$ from $E\{n_i n_1\}$, for $i = 1, 2, \ldots$. We shall make use of this later.) Comparing equation (11) with equation (2), we see that the form of the $E\{n_i\}$ (for $i = 1, 2, \ldots$) could be described by the form of relationship (2) if and only if

$$E\{n_i n_1\}/\rho = E\{n_i\}\phi\,, \quad \text{for } i = 1, 2, \ldots. \qquad (12)$$

Summing the equations represented by relationship (12) for $i = 1, 2, \ldots$, we obtain

$$nE\{n_1\}/\rho = n\phi\,; \qquad (13)$$

and thus

$$E\{n_1\} = \phi\rho\,. \qquad (14)$$

Applying equation (14) to equation (12) for $i = 1$, we have

$$E\{n_1^2\} = E\{n_1\}\phi\rho = [E\{n_1\}]^2\,. \qquad (15)$$

Thus, if equation (15) is not satisfied then certainly the form of the $E\{n_i\}$ (for $i = 1, 2, \ldots$) cannot be described by the *form* of the relationship (2). But equation (15) cannot be satisfied unless the variance of n_1 is zero, which is not the case for the stochastic model we have been considering. We have thus shown that for the attractor model the form of the $E\{n_i\}$ (for $i = 1, 2, \ldots$) in equilibrium cannot be described by the T-P formulas (2) and (1).

Applying equation (14) to equation (12), we see that the form of the $E\{n_i\}$ (for $i = 1, 2, \ldots$) could be described by the form of the relationship (2) if and only if

$$E\{n_i n_1\} = E\{n_i\}E\{n_1\}\,, \quad \text{for } i = 1, 2, \ldots. \qquad (16)$$

Since equation (16) will be true (for $i = 1, 2, \ldots$) if and only if the variance of n_1 is zero, we see that for the stochastic model under consideration the $E\{n_i n_1\}$ cannot be replaced by $E\{n_i\}E\{n_1\}$. In his attempt to derive the equilibrium size distribution for this stochastic model, White did replace the $E\{n_i n_1\}$ by $E\{n_i\}E\{n_1\}$ (for $i = 1, 2, \ldots$), thus ignoring the random variation of n_1.

The attractor model versus non-sociological models.—The central feature of the attractor model, which distinguishes it from models that do not pertain to systems of groups (i.e., from models that are not, according to Coleman, "sociological in the strong sense"), is the fact that the probability of an arrival to a group in the small time interval from t to $t + h$ is contingent upon the number $n_1(t)$ of single individuals in the system at time t, rather than upon $E\{n_1(t)\}$. Since $n_1(t)$ is a random variable, it cannot be replaced by $E\{n_1(t)\}$ without ignoring the random variation of n_1. Mathematical manipulations that ignore the random variation of n_1, by replacing n_1 by $E\{n_1\}$ or more generally by replacing $E\{n_i n_1\}$ by $E\{n_i\}E\{n_1\}$ (for $i = 1, 2, \ldots$), are in fact ignoring the central feature of this stochastic model, the feature that permits us to characterize it as a model pertaining to systems of groups rather than as one which does not. The mathematical manipulations made by White in his attempt to derive equilibrium size distributions, and those made by Coleman and James in their analysis, do in fact ignore the random variation of n_1, and they have thus in effect replaced their stochastic models which pertain to systems of groups with models which do not.

The preceding remarks will help to clarify why the same formula, namely, the T-P formula, was obtained by both Coleman and James and by White. We have shown above that, to the extent that the attractor model

actually pertains to a system of groups, the T-P formula will not serve as a description of the equilibrium size distribution of this model. The T-P formula is actually suited to a "deterministic model" (which does not pertain to systems of groups) rather than to the stochastic models given by Coleman and James and by White. Mathematical results obtained for stochastic models are often quite different from those obtained for the corresponding deterministic models, and quite misleading conclusions could be obtained if results for these deterministic models were taken as "approximations" to the corresponding results for the stochastic model; especially in the case considered here, where the central feature distinguishing models that pertain to systems of groups from those that do not is also one of the features that distinguishes between the stochastic and the deterministic model.[11]

The n-*group attractor model when* n *is large.*—White has claimed that the T-P formula will serve as an approximation to the equilibrium size distribution of the attractor model when the number, n, of groups in the model is large. We shall now re-examine this claim.

Let p_i denote the proportion of groups of size i in equilibrium; that is, $p_i = n_i/n$. From equation (9) we have

$$E\{p_1^2\} = 2\rho E\{p_2\}/n\,, \qquad (17)$$

which implies that $E\{p_1^2\}$ will approach zero when n approaches infinity (assuming that the parameter $\rho = \mu/a$ is fixed). In this case, the variance of p_1 will approach zero, and p_1 will actually converge in probability to zero. This indicates that the T-P formula (1), which has positive probabilities associated with $i = 1, 2, \ldots,$ will not yield the limiting values for $E\{p_i\}$ for $i = 1, 2, \ldots,$ in this case.[12]

The preceding discussion indicates that no situations have as yet been presented in which the T-P formula might serve as an approximation to the equilibrium size distribution of the stochastic model under consideration (not even when n is large), except when this model is replaced by a deterministic model which does not pertain to systems of groups and which ignores the random variation of n_1. Even in the case considered in the preceding paragraph, where the number, n, of groups in the model approaches infinity (assuming that ρ is fixed) and where the random variation of $n_1/n = p_1$ is found in fact to be negligible, the T-P formula still will not do. In the following section, we shall present a special situation in which the T-P formula may possibly serve as an approximation to the equilibrium size distribution of the stochastic model under consideration: a situation in which the stochastic model may possibly be replaced by a deterministic model, although the latter model does not pertain to systems of groups.

The n-*group attractor model when* n *is large and* $\rho = \gamma n$.—In the preceding section, we considered the situation where n approaches infinity and ρ is assumed fixed. This will be the case when the ratio of μ (the rate of departure, per individual in the group, from groups of size $i > 1$) and a (the rate of arrival to a given group, per number of single individuals in the system) is fixed, and n is approaching infinity. We shall now consider the situation where n approaches infinity, $\rho = \gamma n$, and γ is fixed, that is, where ρ increases proportionately with n. If both μ and a are fixed, if both μ and a are directly proportional to n, or if they are both inversely proportional to n, then we have the situation discussed in the preceding section. If μ is fixed and a is inversely proportional to n, or if a is fixed and μ is directly proportional

[11] For a discussion of the inadequacy of the deterministic description of certain processes, see, e.g., David G. Kendall, "Stochastic Processes and Population Growth," *Journal of the Royal Statistical Society*, Series B, XI (1949), 230-64, and the literature cited there. This article refers to a number of processes that do not show the simple relationship which might be expected between the development of the deterministic system and the mean development of the corresponding stochastic system.

[12] We use here the usual mathematical terminology: namely, the limit of $E\{p_i\}$ as $n \to \infty$ would yield the T-P distribution *if* this limit were equal to P_i given by equation (1), for $i = 1, 2, \ldots,$ where ϕ is a positive constant independent of n.

to n, then we have the situation which we shall now consider. We explicitly distinguish between the situation considered in the preceding section and the one which we shall now consider (although this was not done in the earlier literature), because we find that the limiting distributions of the p_i which are obtained in these two different situations are actually quite different.

From (17) we have

$$E\{p_1^2\} = 2\gamma E\{p_2\}\,, \qquad (18)$$

so that $E\{p_1^2\}$ need not approach zero (when $n \to \infty$) in the case under consideration now. From equation (11) we have

$$E\{p_{i+1}\} = E\{p_i p_1\}/[\gamma(i+1)]\,, \quad \text{for } i = 1, 2, \ldots . \qquad (19)$$

If p_1 converged in probability to $E\{p_1\}$, then $E\{p_i p_1\}$ in equation (19) could be replaced by $E\{p_i\}E\{p_1\}$ when $n \to \infty$,[13] thus obtaining

$$E\{p_{i+1}\} = E\{p_i\}\phi/(i+1)\,, \quad \text{for } i = 1, 2, \ldots , \qquad (20)$$

where

$$\phi = E\{p_1\}/\gamma\,. \qquad (21)$$

$E\{p_1\}$ is a positive quantity (see eq. [29]). Assuming that

$$\sum_{i=1}^{\infty} E\{p_i\} = 1\,, \qquad (22)$$

by comparing equation (20) with equation (2), the $E\{p_i\}$ in this case could be written as

$$E\{p_i\} = (\phi^i/i!)/(e^\phi - 1)\,, \quad \text{for } i = 1, 2, \ldots . \qquad (23)$$

[13] When p_1 converges in probability to a constant $E\{p_1\}$, and when the distribution function of p_i converges to a distribution function as $n \to \infty$ (or when p_i converges in probability to a constant as $n \to \infty$), then the convergence theorem in, for example, Harald Cramér, *Mathematical Methods of Statistics* (Princeton, N.J.: Princeton University Press, 1946), pp. 254–55, can be applied to show that the limiting distributions of $p_i p_1$ and $p_i E\{p_1\}$ are identical. In addition, when $n \to \infty$ the limiting value of $E\{p_i p_1\}$ will equal $E\{p_i\}E\{p_1\}$.

From equation (23) with $i = 1$, we have

$$\phi = (e^\phi - 1)E\{p_1\}\,, \qquad (24)$$

and thus, by comparing equation (24) with equation (21), we have

$$e^\phi - 1 = \gamma^{-1} \qquad (25)$$

which implies that

$$\phi = \ln(1 + \gamma^{-1})\,, \qquad (26)$$

where ln denotes the natural logarithm.

The preceding results indicate that the T-P formula would apply (with ϕ determined by eq. [26]), if p_1 converged in probability to a positive constant $E\{p_1\}$ and if equation (22) were true. In the preceding section we found that p_1 converged in probability to zero, so that the T-P formula did not apply there; but in the present context the convergence in probability of p_1 to a positive quantity seems plausible. Further research is needed to determine whether or not this is actually the case.

The expected number of individuals in the system.—We shall now derive some additional results relating to the attractor model. What is the expected value, $E\{N\}$, of the total number N of individuals who will be members of the system of n groups when the system is in stochastic equilibrium? What is the expected average group size in equilibrium? What is the ratio between the expected number $E\{n_1\}$ of single individuals in the system in equilibrium and the expected total number of individuals in the system? In order to answer these questions, it will not be necessary to obtain approximations assuming $n \to \infty$. The results we shall present now hold true for all values of n.

Multiplying each side of equation (11) by $\rho(i+1)$ and then summing each side for $i = 1, 2, \ldots$, we obtain

$$\rho E\{N - n_1\} = E\{nn_1\} = nE\{n_1\}\,, \qquad (27)$$

which implies that

$$E\{N\} = E\{n_1\}(1 + n/\rho)\,. \qquad (28)$$

(Here we assumed that the order of taking expectations and summing could be inter-

changed.) Thus $E\{N\}$ is proportional to $E\{n_1\}$, and the expected value $E\{N/n\}$ of the average group size can be written as

$$E\{N/n\} = E\{n_1\}(1+\gamma^{-1})/n = E\{p_1\}(1+\gamma^{-1}) . \quad (29)$$

From equation (28) we see that the ratio between the expected number of single individuals and the expected total number of individuals in the system is simply

$$E\{n_1\}/E\{N\} = \gamma/(1+\gamma) . \quad (30)$$

The relationships (28) through (30) hold true for all values of n. (Since $E\{p_1\}$ may possibly be approximated using eq. [23] with ϕ defined by eq. [26] in the situation considered in the preceding section, it may also be possible in this case to approximate $E\{N/n\}$ in terms of γ alone, using eq. [29] and the approximation for $E\{p_1\}$.)

The generating function for the attractor model.—The mathematical methods of analysis applied earlier herein were used to study the expected values of the random variables n_i (for $i = 1, 2, \ldots$), p_i, and N in the attractor model. It should be noted, however, that knowledge of these expected values could not in any case provide a complete description of the equilibrium size distribution (i.e., the joint distribution of the random variables n_i for $i = 1, 2, \ldots$) of this stochastic model.[14] The mathematical methods which were used could not provide any further knowledge (e.g., knowledge concerning the covariances) of the equilibrium size distribution. This is an inherent limitation of these methods. Because of this, we shall now introduce new mathematical methods which can be used sometimes to obtain a more complete description of distributions pertaining to stochastic models.

Let $x^{[j]}$ denote

$$\frac{x!}{(x-j)!} = x(x-1)\ldots(x-j+1) . \quad (31)$$

For any random variable, say $n_i(t)$, the jth factorial moment of $n_i(t)$ is defined as

$$E\{n_i(t)[n_i(t)-1]\ldots[n_i(t)-j+1]\} = E\{n_i(t)^{[j]}\} . \quad (32)$$

We define the factorial moment generating function, $G(a, t)$, to be the expected value of $a^{n_i(t)}$, that is,

$$G(a, t) = E\{a^{n_i(t)}\} .$$

$G(a, t)$ is a function of a, and it can be used sometimes to determine the factorial moments of the random variable.[15]

For the joint distribution of the random variables $n_i(t)$ (for $i = 1, 2, \ldots$), we define the joint factorial moment generating function, $G(a, t)$, to be the expected value of

$$\prod_{i=1}^{\infty} a_i^{n_i(t)} ;$$

that is,

$$G(a, t) = E\left\{\prod_{i=1}^{\infty} a_i^{n_i(t)}\right\} . \quad (33)$$

$G(a, t)$ is a function of $\{a_1, a_2, \ldots\} = a$, and it sometimes can be used to determine the joint factorial moments between $n_i(t)$ and $n_j(t)$.[16]

For the joint distribution of the $n_i(t)$ ($i = 1, 2, \ldots$), we shall now examine how

[14] This is so for the same kind of reason that a complete description of any random variable cannot be obtained from the expected value of the random variable alone. We would want to know, in addition, at least the variance of the random variable, and in studying a situation where there is more than one random variable (e.g., n_1 and n_2 in the present context) we would want to know at least the covariance between the random variables.

[15] For further discussion of moment generating functions and factorial moment generating functions, see, e.g., Alexander M. Mood and Frank A. Graybill, *Introduction to the Theory of Statistics* (2d ed.; New York: McGraw-Hill Book Co., 1963), Sections 5.3–5.4.

[16] For examples of the use of generating functions and other related techniques in the analysis of stochastic models which are somewhat different from those considered here, see, e.g., M. S. Bartlett, *An Introduction to Stochastic Processes* (Cambridge: Cambridge University Press, 1956), Section 3.5; William Feller, *op. cit.*, chap. xi, and Leo A. Goodman, "Population Growth of the Sexes," *Biometrics*, IX (1953), 212–25.

the generating function $G(a, t)$ changes with time. Let $z(t, h)$ denote the quantity

$$z(t, h) = \left[\prod_{i=1}^{\infty} a_i^{n_i(t+h)-n_i(t)} - 1\right] / h . \quad (34)$$

Then the generating function $G(a, t+h)$, for the joint distribution of the $n_i(t+h)$ (for $i = 1, 2, \ldots$), can be written as

$$\begin{aligned} G(a, t+h) &= E\left\{\prod_{i=1}^{\infty} a_i^{n_i(t+h)}\right\} \\ &= E\left\{z(t, h)\prod_{i=1}^{\infty} a_i^{n_i(t)}\right\} h + G(a, t). \end{aligned} \quad (35)$$

Equation (35) can be rewritten as

$$\begin{aligned} &\frac{G(a, t+h) - G(a, t)}{h} \\ &\quad = E\left\{z(t, h)\prod_{i=1}^{\infty} a_i^{n_i(t)}\right\}. \end{aligned} \quad (36)$$

Let $Z(t, h)$ denote the conditional expected value of $z(t, h)$ given the $n_i(t)$, that is,

$$Z(t, h) = E\{z(t, h) \mid n_i(t)\} . \quad (37)$$

From equations (36) and (37), we see that

$$\begin{aligned} &\frac{G(a, t+h) - G(a, t)}{h} \\ &\quad = E\left\{Z(t, h)\prod_{i=1}^{\infty} a_i^{n_i(t)}\right\}. \end{aligned} \quad (38)$$

From the probabilistic specifications given earlier for the attractor model, $Z(t, h)$ can be readily computed when h is small. Denoting by $Z(t, \cdot)$ the limit of $Z(t, h)$ as h approaches zero, we have

$$\begin{aligned} Z(t, \cdot) &= \sum_{i=1}^{\infty} (a_{i+1}a_i^{-1} - 1)\, a n_1(t) n_i(t) \\ &\quad + \sum_{i=2}^{\infty} (a_{i-1}a_i^{-1} - 1)\, i\mu n_i(t) \quad (39) \\ &= \sum_{i=2}^{\infty} (a_{i-1} - a_i) w_i(t), \end{aligned}$$

where

$$\begin{aligned} w_i(t) = [a_i^{-1} i\mu n_i(t) \\ - a_{i-1}^{-1} a n_1(t) n_{i-1}(t)] . \end{aligned} \quad (40)$$

Let $D_T^*(G)$ denote the derivative of $G(a, t)$ with respect to t. From equations (38) and (39) we note that when the order of the limiting processes can be interchanged, this derivative will be equal to

$$\begin{aligned} D_T^*(G) &= \frac{dG(a, t)}{dt} \\ &= E\left\{Z(t, \cdot)\prod_{i=1}^{\infty} a_i^{n_i(t)}\right\} \quad (41) \\ &= \sum_{i=2}^{\infty} (a_{i-1} - a_i) V_i , \end{aligned}$$

where

$$V_i = i\mu D_i(G) - a D_{i-1}[a_1 D_1(G)] , \quad (42)$$

where $D_i(G)$ denotes the derivative of $G(a, t)$ with respect to a_i, and $D_{i-1}[a_1D_1(G)]$ denotes the derivative of $[a_1D_1(G)]$ with respect to a_{i-1}.

Equation (41) is a fundamental equation which we have derived for the attractor model. It can be used to derive equations (5) and (6), which pertain solely to the expected values $E\{n_i(t)\}$, and in addition it can be used to derive results which could not possibly be derived from equations (5) and (6). Equation (41) does, in a sense, include or subsume equations (5) and (6).

To save space, we shall not show how to derive equations (5) and (6) from equation (41) but shall instead present a new result obtained from equation (41). Let $dE\{n_i^{[j]}\}/dt$ denote the derivative of the jth factorial moment of $n_i(t)$. (For the sake of brevity, we write n_i for $n_i(t)$.) Then, we find from equation (41) that

$$\begin{aligned} \frac{dE\{n_1^{[j]}\}}{dt} = j[\,2\mu E\{n_2 n_1^{[j-1]}\} \\ - aE\{n_1 n_1^{[j]}\}\,] , \end{aligned} \quad (43)$$

for $j = 1, 2, \ldots$. When there is stochastic equilibrium, then the left side of equation

(43) will be zero, which will yield

$$2\rho E\{n_2 n_1^{[j-1]}\} = E\{n_1 n_1^{[j]}\} \quad (\text{for } j = 1, 2 \ldots), \qquad (44)$$

a generalization of equation (9). In particular, when $j = 2$ we have

$$2\rho E\{n_2 n_1\} = E\{n_1^2(n_1 - 1)\} . \qquad (45)$$

From equation (45) we see that the covariance between n_1 and n_2 is

$$C\{n_1, n_2\} = E\{n_1 n_2\} - E\{n_1\}E\{n_2\} = [E\{n_1^2(n_1 - 1)\} - 2\rho E\{n_1\}E\{n_2\}]/2\rho . \qquad (46)$$

From equations (46) and (9), we have

$$C\{n_1, n_2\} = [E\{n_1^2(n_1 - 1)\} - E\{n_1\}E\{n_1^2\}]/2\rho = [E\{(n_1 - \nu_1)^2(\nu_1 + n_1)\} - E\{n_1^2\}]/2\rho , \qquad (47)$$

where $\nu_1 = E\{n_1\}$. Formula (47) provides us with an explicit expression for the covariance between n_1 and n_2 solely in terms of the moments of n_1.

In his attempt to derive the equilibrium size distribution for the attractor model, one of the assumptions made by White was that the difference between $E\{n_1 n_2\}$ and $E\{n_1\}E\{n_2\}$ would be negligible when n is large. In other words, White assumed that the covariance between n_1 and n_2 would be negligible when n is large. This would be the case if the statistical dependence between n_1 and n_2 were negligible. But the statistical dependence between n_1 and n_2 is actually not negligible for the attractor model and the covariance between n_1 and n_2 is also not negligible, as a further analysis of equation (47) would show.

Let us now return to the special situation introduced above where $n \to \infty$, $\rho = \gamma n$, and γ is fixed. Dividing the covariance $C\{n_1, n_2\}$ by n^2 in order to obtain the covariance, $C\{p_1, p_2\}$, between $p_1 = n_1/n$ and $p_2 = n_2/n$, formula (47) leads to

$$C\{p_1, p_2\} = [E\{(p_1 - \Pi_1)^2(\Pi_1 + p_1)\} - E\{p_1^2/n\}]/2\gamma , \qquad (48)$$

where $\Pi_1 = E\{p_1\}$. When $n \to \infty$ in this special case we see from equation (48) that $C\{p_1, p_2\}$ will approach zero if and only if p_1 converges in probability to Π_1. Thus, even when we study the behavior of $C\{p_1, p_2\}$, which is smaller than $C\{n_1, n_2\}$ by a factor of $1/n^2$, we find that whether or not $C\{p_1, p_2\}$ is negligible when n is large (and $\rho = \gamma n$) depends upon whether or not p_1 converges in probability to Π_1 in this special situation. As we had noted earlier, this convergence seems plausible in this special situation, but further research is needed to determine whether or not this is actually the case.

In closing this section, we note that equation (44), a generalization of equations (9) and (45), gives the higher joint factorial moments, $E\{n_2 n_1^{[j-1]}\}$ (for $j = 1, 2, \ldots$), between n_1 and n_2 solely in terms of the moments of n_1. Explicit expressions for the moments of n_1 in terms of the parameters of the model would, of course, increase the utility of equations (9), (44), and (45) in an analysis of this model. Other relations between the moments of the n_i can also be derived.

Further remarks.—In his article, White presents in some detail his analysis of the attractor model, but his analysis of the various other models he presents only briefly, noting that the results he obtained for these other models were arrived at by mathematical manipulations similar to those made in his analysis of the attractor model. In some of the preceding sections, we have presented results pertaining to these mathematical manipulations. Our comments concerning these manipulations apply, to some extent, not only to White's analysis of the attractor model but also to his analysis of the various other models he discusses.

The analysis of the attractor model, which we have presented in some detail in the preceding sections, provides a convenient illustration of some of the possible ways of analyzing various models pertaining to systems of groups. We shall see in the following section that these mathematical methods are also applicable to the analysis of the

Coleman-James model, though the details require modification. The attractor model provides a mathematically simpler example than does the Coleman-James model, and so for convenience of exposition we began with and concentrated upon the analysis of the former model. This does not mean that we think the former model is more generally applicable than the latter to the study of systems of groups. We shall comment later upon the applicability of these models.

The mathematical analysis in the preceding sections and the analysis of the Coleman-James model below do not provide a complete picture of the equilibrium size distributions of these models. At this writing a number of mathematical questions pertaining to these distributions remain unanswered.[17] What research methods might be applied in order to obtain a more complete picture? First, we might attempt to analyze these models in still more mathematical detail than we have done here, for example, by a more intensive analysis of the fundamental equations presented herein for these models. Second, we might simulate, with the aid of high-speed computers, the models under consideration.[18] This we leave to future research.

REANALYSIS OF THE COLEMAN-JAMES STOCHASTIC MODEL

The Coleman-James model.—This is a model for a system consisting of, say, N individuals, where no individuals in the system can leave it and no individuals outside the system can enter it. An individual in the system may depart from a group in which he was a member, and individuals in the system who were single can join groups in the system. In this model, an individual who departs from the group in which he was a member remains in the system (as a single individual, until he joins another group), and the single individuals in the system are the ones who can join groups.[19] Let $n_i(t)$ denote the number of groups of size i ($i = 1, 2, \ldots$) in the system at time t. Then

$$\sum_{i=1}^{\infty} i n_i(t) = N .$$

For this model, the probabilistic specification of the possible movements, at any moment of time, of individuals into and out of groups in the system is as follows: With respect to a group of size i (for $i > 1$) at time t, the probability is $i\mu h$ that one individual will depart from it during the small time interval from time t to $t + h$; the probability is $ahn_1(t)$ that one individual (who is in the system and is single) will join it during this time interval; the probability that there will be neither a departure nor an arrival during this time interval is $1 - i\mu h - ahn_1(t)$. With respect to the single individuals in the system, the probability is $i\mu h$ that an individual who is in a group of size i at time t will leave it and become a single individual in a small time interval from t to $t + h$; the probability is $ahn_i(t)$ that a given individual who is single at time t will join one of the groups of size i ($i > 1$) in this time interval; and the probability is $ah[n_1(t) - 1]$ that he will join one of the other single individuals in this time period.[20]

For this model, the departure rate for a group of size i ($i > 1$) is proportional to i.

[17] For the emigration-immigration model (a model which is quite different from those discussed by Coleman and James and by White) we shall provide a complete description of its equilibrium size distribution.

[18] In situations where the mathematical complexity of the stochastic model under consideration makes it difficult or impossible to derive formulas in closed form pertaining to the model and its equilibrium distribution (if it has one), then simulation of the model using high-speed computers (i.e., "Monte Carlo simulation") may sometimes help to provide a more adequate description. For an interesting example of the use of Monte Carlo methods in the analysis of a model, see H. A. Simon and T. A. Van Wormer, "Some Monte Carlo Estimates of the Yule Distribution," *Behavioral Science*, VIII (1963), 203–10.

[19] Note that in the attractor model an individual who departed from the group in which he was a member actually left the system, and the individuals who joined groups in the system came from outside the system.

[20] N. 10 applies here.

The arrival rate to a given group of size i ($i > 1$) at time t is proportional to $n_1(t)$, and the rate at which others join a given single individual at time t is proportional to $[n_1(t) - 1]$. The rate at which a given individual who is single at time t will join one of the groups of size i ($i > 1$) is proportional to $n_i(t)$ for $i > 1$, and it is proportional to $[n_1(t) - 1]$ for $i = 1$.[21]

The expected number of groups of each size.—As in the earlier model, the number $n_i(t)$ of groups of size i at time t is a random variable ($i = 1, 2, \ldots$), and we denote its expected value by $E\{n_i(t)\}$. The total number of groups in the system at t is now a random variable which we denote by $n(t)$, and its expected value we denote by $E\{n(t)\}$. We have

$$\sum_{i=1}^{\infty} n_i(t) = n(t).$$

In the n-group attractor model, n is a fixed constant; whereas in the Coleman-James model $n(t)$ is a random variable. From the specifications given above for the Coleman-James model, we find that

$$\begin{aligned}\frac{dE\{n_1(t)\}}{dt} &= \sum_{j=2}^{\infty} j\mu E\{n_j(t)\} + 2\mu E\{n_2(t)\} - aE\left\{n_1(t)\sum_{j=2}^{\infty} n_j(t)\right\} \\ &\quad - 2aE\{n_1(t)[n_1(t) - 1]\},\end{aligned} \tag{49}$$

$$\begin{aligned}\frac{dE\{n_2(t)\}}{dt} &= 3\mu E\{n_3(t)\} - 2\mu E\{n_2(t)\} - aE\{n_1(t)n_2(t)\} \\ &\quad + aE\{n_1(t)[n_1(t) - 1]\},\end{aligned} \tag{50}$$

[21] Note that in the attractor model the arrival rate to a given group at time t was proportional to $n_1(t)$, and this rate applied also to a given individual who was single at time t. A given individual in the system who was single could be joined in the attractor model by a second individual from outside the system, whereas in the Coleman-James model a single individual in the system can be joined by another single individual in the system or he can join any other group in the system.

and for $i > 2$

$$\begin{aligned}\frac{dE\{n_i(t)\}}{dt} &= (i+1)\mu E\{n_{i+1}(t)\} - i\mu E\{n_i(t)\} - aE\{n_1(t)n_i(t)\} \\ &\quad + aE\{n_1(t)n_{i-1}(t)\}.\end{aligned} \tag{51}$$

Equations (49) through (51) were obtained by methods similar to those used to obtain equations (5) and (6). We also note that

$$\frac{dE\{n(t)\}}{dt} = \sum_{j=2}^{\infty} j\mu E\{n_j(t)\} - aE\{n_1(t)[n(t) - 1]\}. \tag{52}$$

When there is stochastic equilibrium, then the left side of the equations (49) through (52) will be zero. From equation (52) we obtain

$$\begin{aligned}\rho[E\{N - n_1\}] &= \rho[N - E\{n_1\}] \\ &= E\{n_1[n - 1]\} = E\{n_1 n\} - E\{n_1\},\end{aligned} \tag{53}$$

where $\rho = \mu/a$ and where $n_i(t)$ and $n(t)$ have been replaced by n_i and n respectively. (Here the n and the n_i [for $i = 1, 2, \ldots$] are random variables, whereas in the attractor model the n_i were random variables but n was a fixed constant.) From equations (53) and (49) we obtain

$$2\rho E\{n_2\} - E\{n_1(n_1 - 1)\} = 0, \tag{54}$$

and from equations (54) and (50) we obtain

$$3\rho E\{n_3\} - E\{n_1 n_2\} = 0. \tag{55}$$

From equations (55) and (51) we obtain

$$E\{n_{i+1}\} = E\{n_i n_1\}/[\rho(i+1)], \quad \text{for } i = 2, 3, \ldots. \tag{56}$$

The relationship (54) permits us to "predict" $E\{n_2\}$ from $E\{n_1(n_1 - 1)\}$, which is equal to $E\{n_1^2\} - E\{n_1\}$. The relationship (56) permits us to predict $E\{n_{i+1}\}$ (for $i = 2, 3, \ldots$) from $E\{n_i n_1\}$. (We shall make use of this later.) Comparing equations (9) and (11) derived for the attractor model with equations (54) and (56) derived for the Coleman-James model, we note that $E\{n_2\}$ can be predicted from equation (54) when

$E\{n_1\}$ and $E\{n_1^2\}$ are known in the latter model, whereas we need know only $E\{n_1^2\}$ (and, of course, ρ) in order to predict $E\{n_2\}$ from equation (9) in the former model. The relationship (11) for $i = 2, 3, \ldots$ is identical to relationship (56). Thus the set of relationships described by equations (9) and (11), which pertain to the equilibrium size distribution of the attractor model, is similar to, but also different from, the corresponding set of relationships pertaining to the Coleman-James model. These relationships are different to the extent that $E\{n_1^2\} - E\{n_1\}$ is different from $E\{n_1^2\}$.

In addition, we note that the relationship (27), which pertains to the equilibrium size distribution of the attractor model, is similar to, but different from, the corresponding relationship (53) pertaining to the Coleman-James model. They are different in that in the former case n is a fixed constant, whereas in the latter case n is a random variable, and to the extent that $E\{n_1 n\} - E\{n_1\}$ in equation (53) is different from $E\{n_1 n\}$ in equation (27).

The similarities and differences described above, which pertain to the equilibrium size distributions, are actually reflections of similarities and differences between the two models themselves. These models are similar in that the departure rate from a group of size i is proportional to i, and the arrival rate to a given group of size i at time t is proportional to $n_1(t)$ for all groups of size $i > 1$ in both models. These models are different in that the rate at which others join a given single individual at time t is proportional to $[n_1(t) - 1]$ in the Coleman-James model and to $n_1(t)$ in the attractor model. In some of the other models introduced by White, the arrival rates to a given group at time t are very different from the corresponding rate in the attractor model or in the Coleman-James model. In contrast to the claims made by White, the results presented here indicate that these kinds of differences between the various models will in fact be reflected in differences in the form of the equations pertaining to the equilibrium size distributions. White's conclusion that the form of the equations pertaining to the equilibrium size distributions will be the same for the various stochastic models was arrived at by ignoring certain features of these models.

The Coleman-James model when N *is large.*—Let r_1 denote the proportion of individuals in the Coleman-James model who are single when the system is in equilibrium, that is, $r_1 = n_1/N$. More generally, let r_i denote n_i/N for $i = 1, 2, \ldots$. From equation (54) we have

$$E\{r_1(r_1 - N^{-1})\} = 2\rho E\{r_2\}/N\,, \quad (57)$$

which implies that $E\{r_1^2\}$ will approach zero when N approaches infinity (assuming that the parameter ρ is fixed). In this case, the variance of r_1 will approach zero, and r_1 will converge in probability to zero. This indicates that the form of the $E\{r_i\}$, for $i = 1, 2, \ldots$, will not correspond to the form of the T-P distribution, which has positive probabilities associated with $i = 1, 2, \ldots$, even when N approaches infinity.

The Coleman-James model when N *is large and* $\rho = \delta$N.—In the preceding section, we considered the situation where N approaches infinity and ρ is assumed fixed. We shall now consider the situation where N approaches infinity, $\rho = \delta N$, and δ is fixed, that is, where ρ increases proportionately with N.

From equation (57) we have

$$E\{r_1(r_1 - N^{-1})\} = 2\delta E\{r_2\}\,, \quad (58)$$

so that $E\{r_1^2\}$ need not approach zero (when $N \to \infty$) in the case under consideration now. From equation (56) we have

$$E\{r_{i+1}\} = E\{r_i r_1\}/[\delta(i+1)]\,, \quad \text{for } i = 2, 3, \ldots . \quad (59)$$

From equation (53) we have

$$1 - E\{r_1\} = E\{r_1(g - N^{-1})\}/\delta\,, \quad (60)$$

where $g = n/N$ is the reciprocal of the average group size in equilibrium. If r_1 converged in probability to $E\{r_1\}$, then from equations (58) and (59) we would have

$$E\{r_{i+1}\} = E\{r_i\}E\{r_1\}/[\delta(i+1)]\,, \quad \text{for } i = 1, 2, \ldots , \quad (61)$$

and from equation (60) we would have

$$E\{g\}/\delta = [1 - E\{r_1\}]/E\{r_1\} . \quad (62)$$

If g converged in probability to a positive quantity $E\{g\}$, then from equation (61) we would have

$$E\{p_{i+1}\} = E\{p_i\}E\{r_1\}/[\delta(i+1)] , \quad \text{for } i = 1, 2, \ldots . \quad (63)$$

Assuming that

$$\sum_{i=1}^{\infty} E\{p_i\} = 1 , \quad (64)$$

and that $E\{r_1\}$ is positive, by comparing equation (63) with equation (2), the $E\{p_i\}$ in this case could be written as

$$E\{p_i\} = (\phi^i/i!)/(e^\phi - 1) , \quad \text{for } i = 1, 2, \ldots , \quad (65)$$

where

$$\phi = E\{r_1\}/\delta . \quad (66)$$

Since $p_1 = r_1/g$, if g converged in probability to $E\{g\}$, from equation (65) for $i = 1$ we would have

$$E\{r_1\}/E\{g\} = \phi/(e^\phi - 1) . \quad (67)$$

From equations (65), (66), and (62), we have

$$e^\phi - 1 = E\{g\}/\delta = [1/E\{r_1\}] - 1 . \quad (68)$$

From equations (67) and (69), we have

$$\phi e^\phi = \delta^{-1} . \quad (69)$$

The preceding results indicate that the T-P formula would apply (with ϕ determined by eq. [69]) in the situation under consideration in this section if r_1 and g converged in probability to positive quantities and if equation (64) were true. In the preceding section we found that r_1 did not converge in probability to a positive quantity but converged instead to zero; in the present context the convergence in probability of r_1 and g to positive quantities seems plausible. Here, too, further research is needed.

The generating function for the Coleman-James model.—Let $H(a, t)$ denote the joint factorial moment generating function for the Coleman-James model, that is,

$$H(a, t) = E\left\{\prod_{i=1}^{\infty} a_i^{n_i(t)}\right\} , \quad (70)$$

where $n_i(t)$ is the number of groups in the Coleman-James model of size i at time t. Let $D_T^*(H)$ denote the derivative of $H(a, t)$ with respect to t. By methods similar to those used earlier herein, we find that the generating function $H(a, t)$ will satisfy the following equation:

$$D_T^*(H) = \sum_{i=2}^{\infty} (a_1 a_{i-1} - a_i) U_i , \quad (71)$$

where

$$U_i = i\mu D_i(H) - \alpha D_{i-1}[D_1(H)] , \quad (72)$$

where $D_i(H)$ denotes the derivative of $H(a, t)$ with respect to a_i, and $D_{i-1}[D_1(H)]$ denotes the derivative of $D_1(H)$ with respect to a_{i-1}.

Equation (71), a fundamental equation which we have derived for the Coleman-James model, can be used to derive equations (49) through (52), given above in our study of the expected values $E\{n_i(t)\}$. This equation does, in a sense, include or subsume equations (49) through (52). With equation (71) it is possible to derive results which could not be derived from equations (49) through (52). The methods for doing this are similar to those used above in our study of the attractor model.

Further remarks.—If the probabilistic specification of the attractor model had been altered, changing from $\alpha h n_1(t)$ to $\alpha h[n_1(t) - 1]$ the probability that an individual from the outside environment would join, in the time interval from t to $t + h$, a given individual in the system who is single at time t, then the arrival rates and the departure rates for this model would have been identical to the corresponding rates for the Coleman-James model, and the equations given here pertaining to the equilibrium size distributions would also have been identical (except for the fact that n is a fixed constant and N a random variable in one case, and in the

other case N is a fixed constant and n a random variable). However, as we noted earlier, for stochastic models having arrival rates and/or departure rates which are quite different from the corresponding rates for these two models, the equations pertaining to their equilibrium size distributions would also be different.

THE EMIGRATION-IMMIGRATION MODEL

We shall now introduce a model which is quite different from the two we have considered thus far. While the departure rate from a group of size i was the same for each group of this size (namely, $i\mu h$ in the time interval from t to $t + h$, for $i > 1$) in the earlier models, the model which we shall now consider (the E-I model) will not preclude the possibility that different groups in the system might have different departure rates even though they might be of the same size. One group in the system may be able to hold its members to a greater or lesser extent than can another group; this possibility will be taken into account in the E-I model, whereas the earlier models assumed that each group of a given size would be able to hold its members to the same extent. While the rate of arrivals to a group at time t in the earlier models depended only on the number of single individuals in the system at that time, the E-I model will permit arrivals to a group to come directly from other groups of any size in the system, or from the environment outside the system, and the rates will vary accordingly. In the attractor model, only individuals from the outside environment may join a given group in the system, and in the Coleman-James model only individuals in the system who are single at time t may join a given group in the system in the time interval from t to $t + h$; whereas the E-I model will permit both individuals from the outside environment and also individuals who are in the system, both individuals who are single at time t, and also individuals who are members of groups, to join a given group in the system in the time interval from t to $t + h$. In the earlier models, the arrival rate is the same for all or almost all groups in the system at time t (i.e., this rate is proportional to $n_1(t)$ for all groups in the attractor model, and for all groups of size $i > 1$ at time t in the Coleman-James model), whereas the E-I model will not preclude the possibility that arrival rates may be different for different groups in the system. One group in the system may be able to attract new members to a greater or lesser extent than can another group; this possibility will be taken into account in the E-I model, whereas the earlier models assumed that each group would be able to attract new members to the same extent.

In the Coleman-James model, the N individuals in the system have fixed identities and they remain continuously in the system. In the attractor model, the n groups in the system have fixed identities, even though the entire membership of a given group may be different at one time from another, and all of these n groups remain continuously in the system, each of them having at all times at least one member. In the E-I model, the n groups in the system will also have fixed identities, even though the entire membership of a given group may be different at one time from another. However, in contrast to the attractor model, the E-I model will not preclude the possibility that the number of groups with at least one member may vary with time, that some of the groups may lose their entire membership and then later be reactivated, that is, the E-I model will permit groups in the system to "disappear" and then "reappear" again. In the attractor model, no individual in the system who is single at time t may leave the system in the time interval from t to $t + h$, although individuals in groups of size $i > 1$ may leave the system. The E-I model will not preclude the possibility that any individual in the system, individuals who are single at time t and also individuals who are group members at that time, may leave the system.

The n-*group emigration-immigration model*.—Let $x_j(t)$ be the number of individuals in the jth group in the system at time t. (For convenience, we label or number the n groups in the system from 1 to n.) With re-

spect to the jth group at time t, the probability is $\lambda_{jk}hx_j(t)$ that one individual will move from it to the kth group ($k \neq j$) in the small time interval from t to $t+h$; the probability is $\lambda_j hx_j(t)$ that one individual will depart from this group (and leave the system) during this time interval; the probability is $\lambda_{kj}hx_k(t)$ that one individual from the kth group will move to the jth group during this time interval; the probability is $\theta_j h$ that an individual from outside the system will join the jth group during this time interval; and the probability of no change in the jth group during this time interval is

$$1-\Big(\sum_{k\neq j}\lambda_{jk}+\lambda_j\Big)hx_j(t) - \sum_{k\neq j}\lambda_{kj}hx_k(t)-\theta_j h.$$ [22]

Note that λ_{jk} is the rate of movement (per individual in the jth group) of individuals from the jth group to the kth group ($j \neq k$), λ_j is the rate at which individuals from the jth group leave the system (per individual in the jth group), and θ_j is the rate of arrival of individuals to the jth group. In the special case where $\lambda_{jk} = \lambda^*$ for all j and k, $\lambda_j = \lambda$ for all j, and $\theta_j = \theta$ for all j, then $\lambda^*(n-1)$ is the rate of movement of individuals between groups (per individual in the system), λ is the rate at which individuals leave the system (per individuals in the system), and θn is the rate of arrival of individuals to the system.

In the E-I model each group is associated with a particular label (the jth group has the label j) which we may regard as denoting either the location or purpose of the group or some other identification for the group. This identification is not affected by the size of the group at any given time, by the particular membership at that time, or by the possible temporary "disappearance" of the group. As we noted earlier, the groups in the attractor model also have fixed identities and therefore can be labeled. There, too, a group's identity has permanence independent of the particular membership of the group at any given time and independent of the size of the group. (Because of the possible temporary "disappearance" and "reappearance" of groups in the E-I model, a phenomenon the possibility of which is precluded in the attractor model, the labeling of groups in the E-I model may denote, in a sense, a more abstract identification than does the labeling in the attractor model.) For situations where this kind of labeling is not possible, these models are, of course, not to be recommended.

The equilibrium distribution for the emigration-immigration model.—The rate of entry into the jth group in the E-I model would be equal to the rate of exit from this group if

$$\theta_j+\sum_{k\neq j}\lambda_{kj}x_k(t) = \Big[\lambda_j+\sum_{k\neq j}\lambda_{jk}\Big]x_j(t), \tag{73}$$

which can be rewritten in the form

$$\theta_j=\sum_{k=1}^{n}\Lambda_{kj}x_k(t), \tag{74}$$

where

$$\Lambda_{jj}=\lambda_j+\sum_{k\neq j}\lambda_{jk} \quad \text{and} \quad \Lambda_{kj}=-\lambda_{kj} \quad \text{for } k\neq j.$$

Equation (74) provides us with a system of n equations ($j = 1, 2, \ldots, n$) in (so to speak) n unknowns $x_k(t)$ (for $k = 1, 2, \ldots, n$). The solution to this system of equations we denote by ϕ_k, that is, ϕ_k is such that

$$\theta_j=\sum_{k=1}^{n}\Lambda_{kj}\phi_k \quad \text{for } j=1,2,\ldots,n. \tag{75}$$

When the system of equations (75) has a unique solution, then $\phi_k = \phi$ for all k if and only if

$$\theta_j\Big/\sum_{k=1}^{n}\Lambda_{kj}=\phi \text{ for all } j. \tag{76}$$

[22] N. 10 applies here.

The condition (76) states that θ_j is proportional to

$$\Big[\lambda_j + \sum_{k\neq j}\lambda_{jk} - \sum_{k\neq j}\lambda_{kj}\Big].$$

In particular, if

$$\lambda_{jk} = \lambda_{kj} \quad (\text{for all } k \neq j)$$

and if (77)

$$\theta_j = C\lambda_j \quad (\text{for all } j),$$

then $\phi_k = C$ for all k. This indicates that if the rate of movement of individuals from group j to k (per individual in j) is equal to the rate of movement of individuals from group k to j (per individual in k) and if the rate of arrival to group j from outside the system is proportional to the rate at which individuals from group j leave the system (per individual in j), then the ϕ_k will all be equal. Since the conditions of equation (77) are only a special case of (76), we note that the ϕ_k will all be equal (for $k = 1, 2, \ldots, n$) even in some situations where equation (77) is not satisfied, provided that equation (76) is satisfied.

It can be proved for this model that

$$\lim_{t\to\infty} E\{x_j(t)\} = \phi_j, \tag{78}$$

that stochastic equilibrium is achieved when $t \to \infty$, that $x_j(t)$ will then have a Poisson distribution with mean value ϕ_j, and that $x_j(t)$ will then be statistically independent of $x_k(t)$ for $j \neq k$.[23] Thus, if the λ's and the θ's are such that equation (76) is satisfied, then the observed size distribution of the n groups of the system will be similar (when $t \to \infty$) to an observed sample of n observations from a Poisson distribution with mean value ϕ. The expected value of the group sizes will be ϕ, and the variance of the group sizes will also be ϕ (when $t \to \infty$).

Let n_i denote the observed number of groups of size i in the system (when $t \to \infty$), and let $p_i^* = n_i/n$. When equation (76) is satisfied, we note that

$$E\{n_i\}/n = E\{p_i^*\} = (\phi^i/i!)/e^{\phi} = \psi_i\,, \quad \text{for } i = 0, 1, \ldots, \tag{79}$$

and that the p_i^* converge in probability to ψ_i when $n \to \infty$. In this case, the distribution of n_i will be the binomial distribution with mean value $n\psi_i$.

We now consider the situation where equation (76) need not be satisfied. Then ϕ_k, which is by equation (78) the expected size of the kth group (when $t \to \infty$), can be determined from equation (75). Writing

$$\psi_{ik} = (\phi_k^i/i!)/e^{\phi_k},$$

we then have that

$$E\{n_i\} = \sum_{k=1}^{n}\psi_{ik}, \tag{80}$$

and the variance of n_i is

$$\sigma^2_{n_i} = \sum_{k=1}^{n}\psi_{ik}[1-\psi_{ik}]. \tag{81}$$

Writing

$$n_i = \sum_{k=1}^{n} y_{ik},$$

where $y_{ik} = 1$ if the kth group is of size i and $y_{ik} = 0$ if the kth group is not of size i, we see that the distribution of n_i is the same as the distribution of the sum of n binomial variates y_{ik} ($k = 1, 2, \ldots, n$) where

$$Pr\{y_{ik} = 1\} = \psi_{ik}, \qquad Pr\{y_{ik} = 0\} = 1 - \psi_{ik}. \tag{82}$$

(The joint distribution of n_i and n_j ($i \neq j$) can be similarly derived.) Writing

$$\sum_{k=1}^{n}\psi_{ik}/n = \bar{\psi}_i,$$

we note that the variance of the n_i will be smaller when

$$\psi_{ik} = \bar{\psi}_i \text{ for all } k, \tag{83}$$

[23] See Harold Ruben, "Some Aspects of the Emigration-Immigration Process," *Annals of Mathematical Statistics*, XXXIII (1962), 119–29.

than when the value of the ψ_{ik} is different for different k values. The condition (83) will be satisfied if equation (76) is satisfied.

Let $\bar{x}$ and s^2 denote the observed average and variance, respectively, of the n group sizes (when $t \to \infty$). We find that the expected value of $\bar{x}$ will be

$$E\{N/n\} = \bar{\phi}, \tag{84}$$

and the expected value of s^2 will be

$$E\{s^2\} = \bar{\phi} + \sigma^2_{\phi_k}, \tag{85}$$

where

$$\bar{\phi} = \sum_{k=1}^{n} \phi_k/n, \qquad \sigma^2_{\phi_k} = \sum_{k=1}^{n} (\phi_k - \bar{\phi})^2/(n-1). \tag{86}$$

Thus the expected variance of the n group sizes will be larger when ϕ_k differs for different values of k than when $\phi_k = \phi$ for all k. When condition (76) is satisfied, the expected values of s^2 and $\bar{x}$ will be equal, while when it is not satisfied then $E\{s^2\}$ will be greater than $E\{\bar{x}\}$. We shall make use of this point later.

For the distributions obtained above, there is a positive probability that some groups may temporarily disappear. Since the observed size distribution of the n groups of the system will be similar when $t \to \infty$ and equation (76) is satisfied to a sample from a Poisson distribution with mean value ϕ, the observed distribution of the groups with at least one member in them will be similar in this case to a sample from a truncated-Poisson distribution. Thus, when equation (76) is satisfied and $t \to \infty$, we have

$$E\{p_i\} = (\phi^i/i!)/(e^{\phi} - 1), \quad \text{for } i = 1, 2, \ldots, \tag{87}$$

where p_i is the observed proportion of groups of size i among groups with at least one member. When equation (76) is not satisfied, the T-P distribution will not be obtained.

Further remarks.—The reader will note that we have been able to say more about the distribution of group sizes relating to the E-I model than was possible for the attractor model or for the Coleman-James model. For the attractor model we were able, when there was stochastic equilibrium, to compute the relation between the expected average group size and $E\{n_1\}$ (eq. [29]) and some other relations between the moments of the n_i (e.g., eqs. [11] and [44]), and to suggest that the limiting values of $E\{n_i\}$ would be given by the T-P distribution (23) in the special situation where $\rho = \gamma n$, γ is fixed, and $n \to \infty$; but to obtain this limiting distribution we had to assume (and were unable to prove rigorously) that p_1 converged in probability to $E\{p_1\}$ and to make other assumptions that require further mathematical scrutiny. For the Coleman-James model, we were able to obtain some similar kinds of results, but we were not able in this case to determine an explicit expression for the expected average group size, not even in terms of $E\{n_1\}$.[24] On the other hand, for the E-I model we are able, when there is stochastic equilibrium, to compute explicit expressions for the expected average group size (eq. [84]) and for moments of the n_i (e.g., eqs. [80] and [81]), to describe in full the distribution of the n_i (see eq. [82] and related comments), to prove rigorously that the limiting values of $E\{p_i^*\}$ are given by the Poisson distribution (79) and the limiting $E\{p_i\}$ are given by the T-P distribution (87) when equation (76) is satisfied and $t \to \infty$, to determine other aspects of the observed distribution of group sizes (e.g., eqs. [84] and [85]), and to give an explicit statement of the conditions under which the observed group size distribution is related to the Poisson distribution (eq. [76]) and the conditions under which it is not.

Of course, this does not mean that the E-I model should replace the attractor

[24] Equation (62), which pertains to the expected value of the reciprocal of the average group size, is an approximation which we derived for a special situation.

model and the Coleman-James model in all situations relating to the analysis of the distribution of group size. None of these models will be of universal value. Different models provide different specifications for the possible movements of individuals into and out of a system and their possible movements within it. The particular model that will be appropriate will depend upon the particular situation under investigation. For some systems of groups, the E-I model may provide an adequate description of the movement of individuals. To determine which models are appropriate for which situations, further empirical research is needed concerning the dynamics of individual movements in different situations. This is an important point which should not be overlooked. Detailed empirical analysis of the actual process relating to the movements of individuals in a particular situation can shed further light on the kinds of models that might describe the process.[25]

In some situations where it may be impossible to carry out an empirical analysis of the actual process relating to the movements of individuals, it may nevertheless be possible to determine estimates of the supposed parameters of the process (e.g., estimates of arrival and departure rates pertaining to, say, the parameters a and μ in the attractor model) and then compare the observed distribution of group sizes with the equilibrium size distributions predicted by the various models under consideration on the basis of the estimates of the relevant parameter values. Although the parameters pertain to the dynamics of the process, their estimates can be used to predict the equilibrium size distributions pertaining to various models, and we could then use the observed distributions to see which predictions best fit the data. Even when estimates of the relevant parameters are not available, a comparison of the form of the observed distributions with the forms of the equilibrium size distributions of the various models will help us to determine which models may provide adequate descriptions of the process under study.

[25] For the statistical analysis of processes of change that are somewhat similar to, and to some extent different from, the movement of individuals among groups of the kind discussed here, see Isadore Blumen, Marvin Kogan, and Philip J. McCarthy, *The Industrial Mobility of Labor as a Probability Process* (Ithaca, N.Y.: Cornell University Press, 1955); Leo A. Goodman, "Statistical Methods for the Mover-Stayer Model," *Journal of the American Statistical Association*, LVI (1961), 841–68; and his "Statistical Methods for Analyzing Processes of Change," *American Journal of Sociology*, LXVIII (1962), 57–78.

STATISTICAL METHODS FOR ANALYZING DATA ON GROUP SIZES

The χ^2 goodness-of-fit test.—With respect to the E-I model, we noted above that the groups in the system can be considered to be (when eq. [76] is satisfied and $t \rightarrow \infty$) a random sample from a Poisson distribution, and the groups with at least one member in them can be considered to be in this case a random sample from a truncated-Poisson distribution. Therefore, the χ^2 goodness-of-fit test can be used in this case to see how well the observed size distribution of groups with at least one member in them can be fitted by the T-P formula.

With respect to the attractor model and the Coleman-James model, we noted earlier that, in general, the T-P formula does not apply. Even in the special cases described above (e.g., where $\rho = \gamma n$, γ is fixed, and $n \rightarrow \infty$ for the attractor model) where the T-P formula may be applicable, there is still no justification for treating the observed size distribution of the groups as a random sample from a T-P distribution, and hence no justification for the application of the usual χ^2 sampling theory. On the other hand, as we noted above, χ^2 sampling theory can be applied to the χ^2 goodness-of-fit statistic when the E-I model is under consideration.

Calculation of the goodness-of-fit test.—To justify the use of χ^2 sampling theory the "best" estimate of the unknown parameter of the Poisson distribution, or an estimate asymptotically equivalent to it, must be used in fitting the Poisson formula to an

observed set of data.[26] Similarly, in fitting the T-P formula to an observed set of data, the "best" estimate of the unknown parameter of the T-P formula (i.e., the ϕ in eq. [87]), or an estimate asymptotically equivalent to it, must be used. With respect to the T-P formula, it is possible to derive the "best" estimate (based on the observed cell frequencies) of ϕ and to show that it can be approximated by setting the mean of the T-P fitted distribution (see eq. [4]) equal to the observed average $\bar{y}$ of the group sizes among groups having at least one member, that is, by determining the estimate ϕ^* of ϕ from

$$\bar{y} = \phi^*/[1 - e^{-\phi^*}]. \qquad (88)$$

To facilitate the calculation of ϕ^*, tables and charts are available.[27]

In fitting their observed group size distributions, Coleman and James estimated ϕ by methods which differed from those referred to above. In view of the fact that (*a*) to justify the use of the χ^2 sampling theory the "best" estimate referred to in the preceding paragraph, or an estimate equivalent to it, must be used in fitting the T-P distribution, and that (*b*) tables and charts are available to facilitate the calculation of ϕ^*, which approximates the "best" estimate, we would recommend that the methods of estimation used by Coleman and James be replaced by the methods presented here.

The number of degrees of freedom of the χ^2 test.—What is the correct number of degrees of freedom to be applied in determining the probability level associated with the goodness-of-fit statistic measuring the fit of the observed frequencies in, say, r cells to the corresponding expected frequencies based upon the T-P formula? Coleman and James thought that r-1 is the correct number, and Coleman later stated that this had been an error and that r-2 is correct.[28] If the parameter ϕ in the T-P formula is assumed known (which was not the case in the Coleman-James paper), then r-1 is correct. If ϕ is unknown and must be estimated from the data (which was the case in the Coleman-James paper), then r-1 is not correct. In this case, the correct number of degrees of freedom will actually depend upon the method used to estimate ϕ.

The correct number of degrees of freedom will be r-2 *if* the "best" estimate referred to above, or an estimate equivalent to it, is used; but if other estimates are used, the goodness-of-fit test with r-2 degrees of freedom will not be valid.[29] The "best" estimate referred to above is the maximum likelihood estimate based on the frequencies observed in the r cells, and it is approximated by ϕ^* defined in the preceding section. The estimate ϕ^* is actually the maximum-likelihood estimate based on the entire sample of observations (not grouped in r cells), that is, it is the "fully efficient" maximum-likelihood estimate. But it is not, strictly speaking, asymptotically equivalent to the "best" estimate based on the r cell frequencies (when r is fixed). If the "fully efficient" estimate ϕ^* is used in fitting the T-P distribution to the observed data, and if the goodness-of-fit test with r-2 degrees of freedom is applied to the r cell frequencies obtained, this would actually result in a test having a greater probability of type I error (asymptotically) than that calculated from the corresponding table of the χ^2 distribution with r-2 degrees of freedom, though the discrepancy will usually not be serious.[30] In other words, in testing the hypothesis that a sample of group sizes could be fitted by a T-P distribution, rejection of this hypothesis at, say, the presumed 5 per cent level of significance as computed from the tabulated χ^2 distribution

[26] See, e.g., H. Cramér, *op. cit.*, pp. 434–35, and A. Hald, *Statistical Theory with Engineering Applications* (New York: John Wiley & Sons, 1952), pp. 742-43.

[27] A. Clifford Cohen, Jr., "Estimating the Parameter in a Conditional Poisson Distribution," *Biometrics*, XVI (1960), 203–11.

[28] James S. Coleman, *op. cit.*, p. 176.

[29] See Maurice G. Kendall and Alan Stuart, *The Advanced Theory of Statistics*, Vol. II (London: Charles Griffin & Co., 1961), Secs. 30.15–30.19.

[30] See Herman Chernoff and E. L. Lehmann, "The Use of Maximum Likelihood Estimation in χ^2 Tests for Goodness of Fit," *Annals of Mathematical Statistics*, XXV (1954), 579–86.

with r-2 degrees of freedom would be in fact rejection at a somewhat higher level of significance, the actual level depending upon the value of r and ϕ. Thus the test based on r-2 degrees of freedom would erroneously reject the hypothesis that the fit is a good one somewhat more frequently than is indicated by the presumed level of significance. The inadequacy of the fit would be exaggerated to some extent in a test based on r-2 degrees of freedom, while these differences would be understated in a test based on r-1 degrees of freedom. (The fit obtained by Coleman and James, based on r-1 degrees of freedom, seemed less inadequate than it would have had they used the r-2 degrees of freedom.)

In situations where it is preferable to understate rather than to exaggerate the inadequacy of the fit, where type I errors must be guarded against, even at the expense of increasing the probability of type II errors, then the test should be based on r-1 degrees of freedom. In situations where it is preferable to exaggerate rather than to understate the inadequacy of the fit, where type II errors must be guarded against even at the expense of increasing the probability of type I errors, then the test should be based on r-2 degrees of freedom. To indicate the extent of the possible exaggeration or understatement, after computing the goodness-of-fit statistic the corresponding probability levels associated with both r-1 and r-2 degrees of freedom should be reported.

The variance test and other related tests.—We noted earlier that the goodness-of-fit test, if properly applied, could be used to test the fit of the Poisson distribution, or of the T-P distribution, when the E-I model is under study. However, an analysis of observed distribution of group sizes should not rest solely on this test. We shall now present some additional methods which are also suited to the analysis of these kinds of data, when the E-I model is under study.

From equations (84) and (85) we noted earlier that the expected value of the observed variance s^2 of the sizes of the n groups in the E-I model will be equal to the expected value of the observed average size $\bar{x}$ of these groups, when condition (76) is satisfied, and that the expected value of s^2 will be larger than the expected value of $\bar{x}$ when condition (76) is not satisfied. It is therefore of particular interest to compare the magnitudes of s^2 and $\bar{x}$ when analyzing data on the observed sizes of groups relating to the E-I model. This comparison can be made by calculating

$$X^2 = \frac{s^2}{\bar{x}} \qquad (89)$$

and noting whether it is statistically significant when referred to the χ^2 table with (n-1) degrees of freedom.[31] This is the variance test for condition (76). It tests whether the observed magnitude of the variability of the group sizes is in accordance with the magnitude predicted on the basis of the Poisson distribution. More specifically, this test is concerned with whether the relationship between the observed sample variance and sample mean can be fitted by the corresponding relationship determined for the Poisson distribution; and it will usually provide a more powerful test for detecting actual deviations from this relationship than will the χ^2 goodness-of-fit test, which is concerned more generally with whether the over all shape of the observed sample distribution can be fitted by the corresponding shape of the Poisson distribution.

For a more thorough analysis of an observed distribution of group sizes, the χ^2 test and the variance test described above can be supplemented by a number of additional tests. The runs in the series of deviations between the observed frequencies in each category (i = 0, 1, 2, . . .) and the frequencies expected from the fitted Poisson (or truncated-Poisson) formula could be analyzed in more detail.[32] In addition, the (n-1) degrees of freedom for the variance test given here could be subdivided in various ways to provide additional variance tests (different from

[31] See, e.g., William G. Cochran, "Some Methods of Strengthening the Common χ^2 Tests," *Biometrics*, X (1954), 417–51.

[32] See, e.g., A. Hald, *op. cit.*, p. 742.

the one we have presented) specific for given kinds of deviations from the Poisson distribution, and the pattern of deviations observed with the usual χ^2 goodness-of-fit test could be analyzed further by additional single-degree goodness-of-fit tests specific for given patterns of deviations.[23] Furthermore, since the Poisson distribution is a limiting case of the negative binomial distribution, the observed data could first by fitted by the negative binomial distribution and then the values obtained for the fitted parameters could be used to determine whether the Poisson fit is adequate or whether it should be replaced by the negative binomial.[24]

Use of the expected relationships in equilibrium.—For the E-I model we noted earlier that the expected number $E\{n_i\}$ of groups of size i ($i = 0, 1, 2, \ldots$) in equilibrium could be described by the Poisson formula (79) when condition (76) is satisfied. Although this formula is not applicable to the attractor model or to the Coleman-James model, somewhat related formulas (eq. [11] for the former model; eqs. [54] and [56] for the latter model), which describe expected relationships between the $E\{n_i\}$, are applicable. The observed data could be studied to determine whether these expected relationships actually fit. We shall now describe how this might be done when the attractor model formula (11) is of interest. Similar methods can be developed for the situation where formulas (54) and (56) pertaining to the Coleman-James model are of interest.

Formula (11) can be interpreted as follows. If we observed the distribution of the sizes of the n groups in the attractor model in equilibrium a large number of different (independent) times, say m times, then the average of the m observed values of n_{i+1} will be approximately equal to the corresponding average of the m values of the product $n_i n_1$ divided by $\rho(i + 1)$ for $i = 1, 2, \ldots$. Thus, in order to determine whether an observed system of groups can be fitted by equation (11), we would observe the distribution of the sizes of the groups in this system, say, m times, and then average the m values of n_{i+1} and compare this average with the corresponding average of the m values of $n_i n_1/(i + 1)$, for $i = 1, 2, \ldots$. On graph paper, we might plot points corresponding to the pairs of values (X_i, Y_i), where the abscissa X_i is the average of the m values of $n_i n_1/(i + 1)$ and the ordinate Y_i is the corresponding average of the m values of n_{i+1} for $i = 1, 2, \ldots$. If these points can be fitted by a straight line through the origin, then we would say that the expected relationships in equilibrium given by equation (11) for the attractor model actually do fit the observed data. In this case, the slope of the straight line would provide an estimate of the unknown parameter $1/\rho$.[25]

In view of the fact that the average of the m values ($m > 1$) of the product $n_i n_1$ will (for at least some values of i) not be equal to the corresponding product of the average of the m values of n_i and the average of m values of n_1, the method described above for fitting equation (11) will be quite different from procedures which fit the average of the m values of n_i to the T-P formula. When the

[23] Some of these tests have been described by W. G. Cochran, *op. cit.* See also William G. Cochran, "A Test of a Linear Function of the Deviations between Observed and Expected Numbers," *Journal of the American Statistical Association*, L (1955), 377–97.

[24] We noted earlier that the Poisson distribution will be suited to the E-I model when condition (76) is satisfied, i.e., when the ϕ_k ($k = 1, 2, \ldots, n$) determined by (75) are all equal. If the ϕ_k ($k = 1, 2, \ldots, n$) are not all equal, the Poisson distribution will not apply. If the n values of ϕ_k ($k = 1, 2, \ldots, n$) can be approximated by a gamma type distribution (i.e., a Pearson type III distribution), then the distribution of group sizes will be approximated by a negative binomial distribution. For a discussion of the negative binomial distribution see, e.g., A. Hald, *op. cit.*, pp. 728–30; and F. J. Anscombe, "Sampling Theory of the Negative Binomial and Logarithmic Series Distributions," *Biometrika*, XXXVII (1950), 358–82.

[25] The fitting of the straight line can be done by inspection of the graph. Although the usual statistical methods for fitting straight lines and for testing whether or not the fitted line should go through the origin make assumptions that do not apply in this case, these methods might be used as a rough-and-ready guide to supplement a careful inspection of the graph.

distribution of the sizes of the n groups in the attractor model is observed m different (independent) times, where m is large, then the method for fitting equation (11) given here will lead to a close fit and also an accurate estimate of $1/\rho = \alpha/\mu$. In this situation, the usual methods for fitting the T-P formula will not lead to a close fit for the observed data. Although the method described herein for fitting equation (11) is not to be recommended in the case where $m = 1$,[36] we note that in this special case this method is somewhat related to (though not identical with) the usual procedures for fitting the T-P formula.

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[36] This case corresponds, in a sense, to the drawing of a single observation (i.e., a sample consisting of only one observation) from a population, a situation which is obviously undesirable if inferences based upon the observed sample are to be made about the population.

Institutional Completeness of Ethnic Communities and the Personal Relations of Immigrants[1]

Raymond Breton

ABSTRACT

This study focuses on the direction of the interpersonal relations of immigrants. The immigrant can become interpersonally integrated within the "native" community, within his ethnic community, or within a group of immigrants of an ethnicity other than his own.

The direction of the integration may be determined in part by some of the characteristics of the immigrant himself, for example, his educational background, his age, or his motivation for migrating. In this study attention was focused not on the individual characteristics but on the ability of the ethnic community in the receiving society to attract the immigrant into its social boundaries. It is found that this ability is largely dependent on the degree of institutional completeness of the ethnic community, but other characteristics of the community are also important. The findings indicate that more attention should be given to the social organization of ethnic communities, particularly to the wide variation which exists among them in this respect.

INTRODUCTION

Many researchers, in attempting to explain the integration of immigrants, have stressed the factors pertaining to the social background, the motivation, and the primary group affiliations of the immigrant.[2] In the present study the view was adopted that some of the most crucial factors bearing on the absorption of immigrants would be found in the social organization of the communities which the immigrant contacts in the receiving country. There are three communities which are relevant: the community of his ethnicity, the native (i.e., receiving) community, and the other ethnic communities.

It was also felt that the integration of the immigrant should not be seen from a purely assimilationist point of view in which integration is said to have taken place when the immigrant is absorbed in the receiving society. Integration was rather conceived of as taking place in any one of the communities mentioned above or in two or three directions at the same time. That is, an immigrant can establish a network of social affiliations extending beyond the boundaries of any one community. Finally, it is also possible for the immigrant to be unintegrated.

It is argued that the direction of the immigrant's integration will to a large extent result from the forces of attraction (positive or negative) stemming from the various communities. These forces are generated by the social organization of the communities. In the present paper the influence of the institutional completeness of the immigrant's own ethnic community on

[1] The material for this article has been taken from the writer's unpublished dissertation, "Ethnic Communities and the Personal Relations of Immigrants" (Baltimore: Johns Hopkins University, 1961). I wish to thank Professors James S. Coleman and Arthur Stinchcombe for their most helpful assistance throughout the study.

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[2] See, e.g., C. W. Mills *et al.*, *The Puerto Rican Journey* (New York: Harper & Bros., 1950); W. L. Warner *et al.*, *The Social System of American Ethnic Groups* (New Haven, Conn.: Yale University Press, 1945); S. N. Eisenstadt, "Social Mobility and Intergroup Leadership," *Transactions of the Second World Congress of Sociology*, II (1954), 218-30; and his *The Absorption of Immigrants* (Glencoe, Ill.: Free Press, 1955); and L. G. Reynolds, *The British Immigrant* (Toronto: Oxford University Press, 1935).

the direction of his social integration is examined. In other words, I will examine the extent to which the institutional completeness of the ethnic community is related to its capacity to attract the immigrant within its social boundaries.

INSTITUTIONAL COMPLETENESS OF THE ETHNIC GROUP

Ethnic communities can vary enormously in their social organization. At one extreme, there is the community which consists essentially in a network of interpersonal relations: members of a certain ethnic group seek each other's companionship; friendship groups and cliques are formed. But beyond this informal network, no formal organization may exist. The immigrant who is a member of such a group will establish all his institutional affiliations in the native community since his ethnic group has little or no organization of its own.

Most ethnic groups probably were at one time—and some still are—of this informal type. Many, however, have developed a more formal structure and contain organizations of various sorts: religious, educational, political, recreational, national, and even professional. Some have organized welfare and mutual aid societies. Some operate their own radio station or publish their own newspapers and periodicals. The community may also sustain a number of commercial and service organizations. Finally, it may have its own churches and sometimes its own schools. Between the two extremes much variation can be observed in the amount and complexity of community organizations; the degree of institutional completeness in fact shows variations from one ethnic group to another.

Institutional completeness would be at its extreme whenever the ethnic community could perform all the services required by its members. Members would never have to make use of native institutions for the satisfaction of any of their needs, such as education, work, food and clothing, medical care, or social assistance. Of course, in contemporary North American cities very few, if any, ethnic communities showing full institutional completeness can be found.

DIRECTION OF PERSONAL AFFILIATIONS

When the immigrant is transplanted from one country to another, he has to reconstruct his interpersonal "field." He will rebuild in a new community a network of personal affiliations.[8] Such a reconstruction is accomplished through his activities to satisfy his immediate needs: making a living, learning the new language, participating in social life, going to church. To satisfy these needs he will use a certain institutional setup; he will introduce himself into a social group. Which one? The native community? His own ethnic community? Another immigrant community of an ethnicity different from his own? Will he integrate himself primarily in one of these communities, or will he split his affiliations among them?

In the present paper two questions will be discussed: (1) Does the interpersonal integration of the immigrant in fact take place in different directions? In other words, to what extent are immigrants integrated in the native, their own, or another immigrant community? (2) To what extent does the institutional completeness of the ethnic community determine the direction of the interpersonal integration of the immigrant?

DATA AND METHODS

The analysis of the effect of the community characteristics on the direction of integration was carried out through a comparison of the personal relations of immigrants from thirty different ethnic groups. The findings are based on data from interviews with 230 male immigrants in Montreal, Canada, 163 of whom were reinter-

[8] S. N. Eisenstadt, "The Process of Absorption of New Immigrants in Israel," *Human Relations*, V (1952), 222–31.

viewed fourteen months later.[4] In drawing the sample, no restriction was put either on the length of residence in Canada or on the ethnicity of the immigrant.

The sample was drawn in two stages. The first stage was an area sample in which thirteen census tracts were randomly selected. The census tracts in which the number of immigrants was extremely small had first been eliminated. Of the 18,415 households in the thirteen census tracts, 75 per cent were visited in order to enumerate their foreign-born population. From this enumeration was drawn a list of 1,689 male immigrants eighteen years of age or older who had been at least fifteen years of age when they came to Canada. In the second stage, a sample of 350 was drawn at random from the list.

Questionnaires were then administered in personal interviews. Approximately 14 per cent refused to be interviewed; others were not interviewed because they were "never at home," because no one was found to conduct the interview in their own language, or because they had moved during the four to five months between the enumeration and the interviewing. Losses in this last category probably constitute the most serious bias in the sample. Finally, some of the returns of those interviewed had to be rejected because of their poor quality.

The measure used to rate the ethnic communities according to their degree of institutional completeness is constructed from information on the number of churches, welfare organizations, newspapers, and periodicals in each ethnic community. Because all spheres of social activity are not covered it is possible only to approximate the level of institutional completeness of each ethnic group. However, the error is probably consistent for all ethnic groups and consequently does not affect their ranking along that dimension, particularly in view of the fact that the ranking is a very gross one: "Low" or "High."[5]

The direction of the affiliations of the immigrant was determined by the ethnic character of the persons with whom he was in contact. The respondent was first asked whether during the week preceding the interview he had visited any people in their homes or had received any visitor himself. He was also asked whether he went to the movies, theater, night clubs, dance halls, taverns, bowling alleys, or other similar places; whether he attended concerts, sporting events, or other entertainments of some sort; and if he did, whether he went alone or with other people. Finally, he was asked if he met socially some of his co-workers, if he had any. Information was obtained on the national origin and birthplace of each person met (identified by first name or some other symbol). Then the respondents were classified, according to the proportion of the people met who were from each group, as having a *majority* or a *minority* of their personal relations in their own ethnic group, among other im-

[4] The first wave was done during April, May, and June, 1958; the second during July, August, and September, 1959.

[5] Ethnic communities classified as "high" in institutional completeness are Greek, German, French, Hungarian, Italian, Lithuanian, Polish, Russian, Ukrainian. Those classified as "low" are Albanian, American, Austrian, Belgian, Bulgarian, Czechoslovak, Danish, Dutch, English, Indian, Irish, Latvian, Portuguese, Romanian, Scotch, Spanish, Swedish, Swiss, South African, Yugoslavian, West Indian.

In referring to degree of institutional completeness, what is meant is the *relative degree*, not the absolute degree. First of all, not all the organizations are included. The schools, for example, are left out and—what is even more important—so are voluntary associations, which are not only numerous but also very significant in the social life of any ethnic community. However, it seems reasonable to assume that the distribution of these associations among the various groups would be about the same as the distribution of the organizations presented above. Second, there are probably minor errors in the above list. Some ethnic groups may have a welfare organization, for example, which was not listed in the sources used.

migrants, or among members of the receiving community.[6]

ATTACHMENTS TO NATIVE AND TO ETHNIC COMMUNITIES

Most immigrants become absorbed into the new social system in the sense that they establish some personal attachment to it. But in which direction? Table 1 presents data bearing on the extent to which immigrants are interpersonally integrated inside or outside their own ethnic community.

TABLE 1

ETHNIC COMPOSITION OF INTERPERSONAL RELATIONS FOR TOTAL SAMPLE AND BY LENGTH OF RESIDENCE IN RECEIVING COUNTRY

Interpersonal Relations	Total ($N=173$)*	Years of Residence		
		0–6 ($N=73$)	7–12 ($N=57$)	13+ ($N=43$)
Proportion with majority of their personal relations:				
In own ethnic group	0.59	0.70	0.58	0.42
In another ethnic group	.14	.15	.07	.21
With members of native group	.19	.10	.25	.28
In none of the above groups	0.08	0.05	0.10	0.09

* Respondents with no personal relations and those who did not give the necessary information are excluded.

The relations of the immigrants with their ethnic group seem quite strong as far as social ties are concerned. Indeed, nearly 60 per cent have a majority of their personal ties with members of that group, and comparatively few—approximately 20 per cent—have most of their relations with members of the native community.

If we examine the changes that take place as the immigrant spends more time in his new society, we find that the idea of the immigrant segregated from the native community fits more closely, although far from perfectly, the situation of the recent immigration. But this situation changes more and more as the number of years spent in the receiving society increases. It is partly true that at one time the immigrant is drawn into the ethnic subsystem, but it does not take too long before he begins to break these ties and to form new attachments outside his ethnic community. Indeed, it is after six years in the host country that the ties with the native community show a substantial increase.

INSTITUTIONAL COMPLETENESS OF THE ETHNIC COMMUNITY

The presence of formal organizations in the ethnic community sets out forces that have the effect of keeping the social relations of the immigrants within its boundaries. It tends to minimize out-group contacts. This is what Table 2 shows. The communities showing the highest degree of institutional completeness have a much greater proportion of their members with most of their personal relations within the ethnic group: 89 per cent of the members of highly institutionalized communities, as compared with 21 per cent of those from ethnic groups with few or no formal organizations. The relationship between institutional completeness and in-group relations is about the same when the groups

[6] No attention is given here to the difference between those with affiliations in two or three communities and those whose affiliations are concentrated in a single one. Consideration of such differences would be revealing as regards both patterns of change over time and the social and psychological impact of the ethnic homogeneity or heterogeneity of affiliations on the immigrant personality and behavior.

themselves, rather than the individual immigrants, are classified on these dimensions (Part B of Table 2).[7]

The institutions of an ethnic community are the origin of much social life in which the people of that community get involved and as a consequence become tied together in a cohesive interpersonal network. We may note, incidentally, that the same result would obtain in the case of ethnic organizations whose stated purpose is to help the immigrant to "adjust" to the conditions of life and cultural habits in the country of adoption. Through such organizations immigrants may become partly acculturated; but at the same time they would become more strongly integrated into an interpersonal network of their own group. In other words, the existence of an institution in the group would tend to have the observed effect on the cohesiveness of the ethnic group irrespective of its orientation toward the native and its own national culture.

TABLE 2

INSTITUTIONAL COMPLETENESS AND IN-GROUP RELATIONS

	Degree of Institutional Completeness of Ethnic Group		
	Low	Medium	High
A. Proportion of *individuals* with majority of relations within ethnic group.	0 21	0 54	0 89
Number of individuals	(62)	(28)	(83)
B. No. of *ethnic groups* with majority of all relations with in group	5 out of 21	4 out of 5	4 out of 4

[7] In order to test the relationship between institutional completeness and the composition of personal relations, several factors were held constant: the size of the group, its residential concentration, the proportion of professionals in the group, the similarity and difference of the language of the group with those of the natives (English and French), the proportion in the group who are ignorant of the native languages, years of residence in Canada, and occupational status. The relationship held under all these controls. The strength of the relationship was reduced by as much as one-third in only one case: when the proportion ignorant of the native languages was held constant.

SOCIAL ORGANIZATION OR INDIVIDUAL PARTICIPATION?

Do the institutions of an ethnic community affect the social relations only of those who use them, or do they generate a social life that extends beyond the realm of the participants? Table 3 shows that the presence of churches in a community is related to more in-group relations even among those who do not attend the ethnic church.

The number of ethnic publications also affects the composition of interpersonal networks, but only among non-readers (Table 4). The non-readers are much more likely to associate within their ethnic group if the latter has several publications rather than a single one ($d = .31$), but the difference in the case of readers is not only much weaker, but in the opposite direction ($d = .09$). This is perhaps due to the difference in the distribution of the members of each category of ethnic groups in terms of education, language read, or other relevant attributes. The size of the sample, however, does not allow a more detailed analysis.

There is further evidence in Table 4 concerning the effect of publications in the ethnic community on in-group relations. This is found by comparing the difference between readers and non-readers of ethnic publications in communities having only one publication with the corresponding difference for communities having more than one publication. These differences are .48

and .08, respectively. If the immigrant is a member of a community with many publications, it does not seem to make much difference whether he reads them himself or not. If, on the other hand, there is only one publication, then it seems important that he be directly exposed to its influence if it is to affect the composition of his personal relations. We may note also that the respondents from communities with no publications at all are the most likely to associate outside their ethnic group.

Through what processes does the institutional completeness of the community affect the composition of the interpersonal networks of the members of an ethnic group? There are at least four such processes that we can speculate about:

Substitution.—The ethnic group succeeds in holding its members' allegiance by pre-

TABLE 3

IN-GROUP RELATIONS, BY NUMBER OF CHURCHES IN ETHNIC COMMUNITY AND CHURCH ATTENDANCE*

	No. of Churches in Community				
		1 or 2		3 or More	
	None	Non-national Attendance	National Attendance	Non-national Attendance	National Attendance
Proportion of respondents with majority of relations within ethnic group	0.12	0.62	0 85	0.82	0 98
N†	....	....	.. . .	....	....

* a (effect of number of churches) = 0 16, P ($a \leq 0$) < 0.05; b (effect of attendance at a national church) = 0.19, $P(b \leq 0) < 0.02$.

† Respondents from communities without churches are not considered in the calculation of the index.

This measure of the effect of an independent attribute X on a dichotomized dependent attribute Y is the mean of the percentage difference in each pair of comparisons when controlling for other independent attributes. The sampling variance of this statistic is obtained by summing the variances of each percentage difference and dividing by the square of the number of paired comparisons through which these differences were obtained. This estimate of the variance and a table of the standardized cumulative normal distribution are used to find the level of significance. For the basis of this statistic see James S Coleman, *Introduction to Mathematical Sociology* (New York: Free Press of Glencoe, forthcoming).

TABLE 4

IN-GROUP RELATIONS, BY NUMBER OF PUBLICATIONS IN COMMUNITY AND READING OF PUBLICATIONS*

	No. of Publications in Community				
		1 Only		2 or More	
	None	Not Read Every Month	Read at Least Every Month	Not Read Every Month	Read at Least Every Month
Proportion of respondents with majority of relations within ethnic group	0.35	0 43	0.91	0.74	0.82
N†	....	....	... .	. ...	.. ....

* a (effect of number of publications) = 0 11, $P(a \leq 0)$ = 0 27; b (effect of reading of publications) = 0.28, $P(b \leq 0) < 0.01$.

† Respondents from communities with no newspapers and periodicals are not considered in the calculation of the index.

venting their contact with the native community. This is achieved by a process of substitution whereby ethnic institutions rather than those of the native community take hold in the immigrant's social life. Immigrants have to find jobs; if they belong to a religion, they will want to go to church; being members of an occupational group, they may wish to join a work association; like other members of a modern society, they will read newspapers, listen to the radio, and watch television; they will send their children to school, etc. The immigrant and his family establish a wide range of institutional attachments, inevitably or nearly so, through the performance of their work. If these attachments are established within the native community, they will constitute channels for the formation of personal relationships with the members of that community.

If, however, the ethnic group develops its own institutions and forms its own associations, it will in this way control to a large extent the interpersonal integration of at least those of its members who become participants in the ethnic instead of the native organization.

Indeed, the findings presented in Table 3 show that, to the extent that there is actual substitution, that is to say, to the extent that individual immigrants do attend ethnic religious institutions instead of others, the expected effect on the composition of interpersonal ties is observed. But this effect through the process of substitution is fairly weak. In fact, if we take as a measure of this substitution effect the average difference between those who attend a national church and the others (the value of "B" in Table 3), we find that it accounts for less than one-fifth of the total variation in the composition of personal ties. If there is at least one church in the community, a large proportion of the immigrant's personal relations are contained within the social boundaries of the community. The increment in that proportion stemming from the individual's actual attendance at the ethnic church is not very large.

Extension within the community of the personal networks of the participants in institutions.—The immigrants who belong to ethnic organizations value highly their nationality and, as we have seen, form associations within it. The organizations sustain and perhaps increase this group of "attached" individuals, who in turn nourish the national sentiments of non-participants and include them among their associates. Since we do not know if the non-members have among their personal relations other individuals who are members, we can only suggest that this may be one of the processes taking place.

Organizations and associations in a community also raise new issues or activate old ones for public debate.—The arousal of public interest in the life of the group probably results in greater cohesiveness of the group. For instance, the group may become united in some action against outside elements such as over an immigration bill which would be detrimental to this particular group, or the expropriation of houses which would force a large number of families of that ethnic group to move away, or again the attempts of a non-national parish to have the national church closed down in order to increase its own membership.

Of course, issues which divide subgroups within the ethnic community will also have the effect of keeping the personal relations within the ethnic boundaries. The attachments of the members of each subgroup will be strengthened. To the extent that they become polarized on the issues facing their group, association with individuals who are not members of the ethnic group will be less appealing, unless they could become allies in the conflict. An important controversy among Italians, for example, will make Italians associate among themselves in cliques fighting each other.[8]

Leaders of organizations actively attempt to maintain or enlarge the clientele.—This is particularly true if the rate of immigration is decreasing, and because of this the survival of some of the organizations comes

[8] On the unifying role of social conflict see G. Simmel, *Conflict* (Glencoe, Ill.: Free Press, 1955), pp. 13 ff.

to be in danger. An illustration of this is found, for example, in Wittke's history of the German press in America. He notes that "at various times efforts were made to organize German press clubs on a local, state, or national basis." He reports that in 1863, "a number of German publishers and editors met at Reading, Pennsylvania, to work out plans to solve the problem of the rising cost of paper during the Civil War. The agenda was expanded to include a discussion of ways and means to keep the German press alive by supporting German Sunday Schools, reading clubs, and libraries, and by insisting that German be taught in the schools. From this meeting stemmed an organization."[9] After about twenty-five years, the organization died.

This example suggests that some ethnic organizations are dependent on other ethnic organizations for their survival and expansion. In the case quoted above, there was even a direct attempt to create new organizations in the ethnic community for the purpose of keeping alive a particular one. Such direct attempts, however, may occur only when the leaders (in this case the publishers) have some personal interest in the survival of the organization. But this is perhaps only a question of degree in the sense that leaders always have some personal interest in the survival of the organizations for which they are responsible.

TYPE AND NUMBER OF INSTITUTIONS

EFFECT OF DIFFERENT TYPES OF INSTITUTIONS

Of the three types of institutions included in the index of institutional completeness—churches, welfare organizations, and newspapers and periodicals—religious institutions have the greatest effect in keeping the immigrant's personal associations within the boundaries of the ethnic community. This is shown in Table 5 where the effect of the other types of institutions is controlled. In both cases, the presence and number of religious institutions explain about the same proportion of the variation in the dependent phenomenon (40 per cent and 45 per cent).[10]

TABLE 5

CHURCHES, WELFARE ORGANIZATIONS, PUBLICATIONS, AND EXTENT OF IN-GROUP RELATIONS

	No. of Churches in Community							
	2 or Less		3 or More		2 or Less		3 or More	
	Welfare Organizations				Publications			
	None	Some	None	Some	None	Some	None	Some
Proportion of individuals with majority of relations within ethnic group	0 26	0 54	0 75	0 95	0.23	0 67	0 73	0 98
N	(61)	(48)	(4)	(60)	(70)	(39)	(11)	(53)

NOTE: a (effect of welfare organizations on in group relations) = 0 24, $P(a \leq 0) \simeq 0.05$; b^1 (effect of churches holding welfare organizations constant) = 0.45, $P(b_1 \leq 0) < 0.01$; b^2 (effect of churches holding publications constant) = 0 40, $P(b_2 \leq 0) < 0.01$; c (effect of publications) = 0.35, $P(c \leq 0) < 0.01$.

The weight of the religious institutions can be attributed to the dominant role they hold in the community. Churches are very frequently the center of a number of activ-

[9] C. Wittke, *The German-Language Press in America* (Lexington: University of Kentucky Press, 1957), pp. 215–16.

[10] The number of cases in certain cross-classifications did not allow an identical break for each type of organization. It was done as a check, and the results were in the same direction.

ities; associations are formed and collective activities are organized under their influence and support. Also, the national sentiments of the immigrant find support in having experiences in church very similar to those in the country of origin—the language is the same; the images used in preaching are the same; the saints worshipped are also those the immigrant has known from early childhood. Moreover, religious leaders frequently become advocates and preachers of a national ideology, providing a *raison d'être* for the ethnic community and a motivation for identification with it.

Publications have the second most important effect on the immigrant's interpersonal network. The presence of publications in the ethnic community explains 35 per cent of the variation in the proportion of in-group relations.[11]

Newspapers have a role in promoting the national ideology and keeping alive the national symbols and values, national heroes, and their historical achievements. Moreover, they interpret many of the events occurring in the country of adoption in terms of the survival or interests of the ethnic community. It is the very business of the national periodical or newspaper to be concerned with the events and personalities of the ethnic group.

Finally, the effect of the presence of welfare organizations is the least. It explains only 24 per cent of the variation. This, we presume, is due to the fact that these organizations are not concerned with the national interests of the community. They deal with people in need of food, shelter, clothing, or medical attention. They have to support "human" feelings rather than "national" feelings; nevertheless, the existence of such organizations points to the presence of an active elite in the community which perhaps has its influence on the community's cohesiveness through channels other than the welfare organizations themselves. Also, their existence has a negative effect on contact with the host community: immigrants in need who address themselves to ethnic organizations miss a chance of contact with the "native" community.

VARIATION WITH INCREASING NUMBER OF FORMAL ORGANIZATIONS

What most differentiates one community from another in its capacity to control the social integration of its members is not so much its having many formal organizations as having some as opposed to none at all. This is shown in Table 6 where the extent of in-group relations is presented for immigrants whose group has no organization of a certain type, for those whose group has one organization of that type, and finally, for those who belong to groups with two or more such organizations.

All comparisons show that it is the existence of the first organization which makes the greatest difference. Second, third, or further additions have an effect on the extent of in-group relations, but much smaller than the effect of the first organization, or in the opposite direction. A drastic expansion occurs in the ethnic interpersonal network when the ethnic community ceases to be an informal system and acquires some first elements of a formal structure. This expansion continues as the formal structure develops but in increments smaller than the initial one.

CHANGE IN COMPOSITION OF INTERPERSONAL NETWORKS

If the ethnic community to which he belongs determines to a certain degree the composition of an immigrant's interpersonal network, it should also show some effect on the rate at which the immigrant changes his personal associates as well as on the kind of people he includes among, and abandons from, his personal relations.

Changes took place in both directions: some immigrants associated more within their ethnic group, some less. Of those who were not members of an ethnic interpersonal network at the time of the first interview, 22 per cent were included in one

[11] The estimate of the effect of the presence of publications remains the same when it is computed while holding constant the presence of welfare organizations.

year after; on the other hand, 17 per cent of those who associated mostly in their ethnic group had become interpersonally integrated in the native group.[12]

The changes in the composition of the immigrants' personal relations were then approximately as likely to be in the direction of the ethnic community as in that of the native community. This fact is of crucial importance for this study indeed; it serves to validate the scheme used in the analysis. It supports the idea that the integration of immigrants cannot really be studied without taking into account the fact that it can be achieved in at least two directions, that is, within the native or within the ethnic community; and that the integration can take place as frequently in one as in the other direction. Integration within the ethnic community cannot be ignored under the assumption that it is relatively unimportant or that it occurs relatively unfrequently.[13]

The degree of institutional completeness of an immigrant's ethnic community is one *of the main factors determining* the direction of the change in the composition of his personal relations. As Table 7 shows, the immigrant who is a member of an institutionalized ethnic community is more likely to have shifted to a high proportion of in-group relations than the one who belongs to a less institutionalized one (29 per cent as compared to 18 per cent).

Also, Table 7 shows that the immigrants who had no personal relations at the time of the first interview and acquired some between the interviews acquired different kinds of associates depending on the degree of institutional completeness of their ethnic group. If the immigrant is a member of a group with organizations of its own, he is three times more likely to have acquired associates within his own group than if he is a part of a group with a more informal social organization (62 per cent as against 20 per cent).

The degree of institutional completeness and the magnitude of the ethnic interpersonal network are interdependent phenomena. It can be argued that the existence of an informal structure in an ethnic community is a prerequisite for the appearance of formal organizations. But it is also true that once a formal structure has developed it has the effect of reinforcing the cohesiveness of already existing networks and of expanding these networks. This expansion is achieved mostly by attracting within the ethnic community the new immigrants. A community with a high degree of institutional completeness has a greater absorbing capacity than those with a more informal social organization. The present findings on the changes in the composition of personal relations show the difference between the two types of ethnic communities in their ability to exert influence on the direction of the interpersonal integration of the immigrant.

On the other hand, the ethnic community does not seem to have much effect in preventing some of its members from establishing relations outside its boundaries. The proportion who had most of their relations within their own ethnic group and who now have most of them outside of it is the same for members of communities with either formal or informal social organization. The proportions, however, are based on a very small number of cases.

[12] If there had been no panel dropouts, the proportions changing in each direction would probably not have been different. Indeed, if we compare those who changed address with those who did not, under the assumption that the dropouts were mostly movers, we find that the movers were more likely to change the composition of their personal ties than the non-movers during the period between the two panel waves, but in both directions: among those who had mostly in-group relationships at the time of the first panel wave, 13 per cent more of movers than of non-movers had changed to a majority of out-group contacts by the time of the second wave; among those with a majority of out-group contacts, 11 per cent more of movers than of non-movers changed to a majority of in-group relationships.

[13] There are two other possibilities: the immigrant may become interpersonally integrated in another immigrant group of an ethnicity different from his own, or he may remain an isolate. These alternatives will be examined elsewhere.

TABLE 6

Various Community Organizations and Extent of In-Group Relations

Community Organization	Proportion of Individuals and No. of Ethnic Groups with Majority of In-Group Relations				
	Individual			Group	
	Proportion	d*	N	No.	Total
Number of churches:					
2 or more	.80		(108)	7	8
		.13			
1 only	67		(15)	4	6
		.55†			
None	.12		(50)	2	16
Number of welfare organizations:					
2 or more	.58		(36)	3	4
		− 28†			
1 only	.86		(72)	3	5
		− 57†			
None	29		(65)	7	21
Number of newspapers and periodicals:					
2 or more	93		(62)	3	3
		.26†			
1 only	67		(30)	3	5
		37†			
None	30		(81)	7	22

* Difference between the proportion in the first column and the proportion immediately below it in the same column The variance of the differences was estimated by using the method suggested by Leo Goodman in "Modifications of the Dorn Stouffer Tibbitts Method for Testing the Significance of Comparisons in Sociological Data," *American Journal of Sociology*, LXVI (January, 1961), 353–63

† This difference is significant at least at the 01 level

TABLE 7

Institutional Completeness and Change in Ethnic Composition of Personal Relations

Proportion of Relations within Ethnic Group	High Degree of Institutional Completeness			Low Degree of Institutional Completeness		
	Wave II			Wave II		
	Minority	Majority	N	Minority	Majority	N
Wave I:						
Minority	0 71	0 29	17	0 82	0 18	34
Majority	15	85	60	14	86	7
No personal relations	0 38	0 62*	13	0 80	0 20*	10

* d (0 62 − 0 20) = 0.42 is significant at the 03 level.

CONCLUSION

Having found that the institutional completeness of the ethnic community is an important factor in the direction of the social integration of immigrants, it is interesting to speculate about the determinants of this property of an ethnic community. There seem to be at least three sets of factors related to the formation of a public for ethnic organizations.

First, the ethnic group may possess some differentiating social or cultural attribute which can set it apart from the native community. Language, color, and religion are prominent among these features; but other traits and customs of a group can bring about the same result. The more different the people of a certain ethnicity are from the members of the native community, the easier it will be for them to develop their own institutions to satisfy their needs.

In the present study, it was found that a difference in language was associated with a high degree of institutional completeness. It was also found among ethnic groups with a different language that the higher the proportion in the ethnic group who are ignorant of the native languages (French and English), the higher the degree of institutional completeness of the group.[14]

The differentiating characteristics of an ethnic group constitute the basis for the formation of a clientele or a public for ethnic organizations. The mobility potential of the immigrant is reduced by such factors; he is more confined to his ethnic group. This is particularly true—or perhaps only true—when the differentiating features are negatively evaluated by the native community.

The second set of factors related to the degree of institutional completeness has to do with the level of resources among the members of the ethnic group. If a large proportion of the members of an ethnic group have few resources of their own, as indicated for instance in rural origin and lack of occupational skills, then there is in this ethnic group an important "clientele" to support welfare and mutual benefit organizations. Such a situation is likely to incite a "social entrepreneur" within the ethnic group to try to organize something for the new immigrants in need, seeing there an interesting opportunity for himself. His rewards would be either monetary profit, prestige in the community, more members for his church, or more buyers for his newspaper. In the present study, a strong positive relationship was in fact found between the proportion of manual workers in an ethnic group and the degree of institutional completeness of that group.[15]

A third set of factors relates to the pattern of migration. The number of immigrants of a given ethnicity and the rate at which they arrive are revelant factors in the formation of an ethnic public. Perhaps a more important factor is whether the migration is an individual or a group phenomenon. Immigration during certain periods of time or for a given ethnic group can be the result of discrete decisions by individuals experiencing similar conditions in their country of origin; but it can also be a more or less organized group response, ranging from the migration of a group (such as a sect) under a leader to the pattern in which a migratory chain is established over a few years within a kinship network or a village and funds are collected in the new country to pay the passage of

[14] Institutional completeness is high in one group out of eight whose language is also spoken by the native community and in twelve out of twenty-two whose language is different. Also, it is high in six of the thirteen groups with less than 20 per cent ignorant of the native languages and in six of the seven groups with 20 per cent or more not knowing these languages. (No data were available for two of the ethnic groups with a language other than French or English.)

[15] Of thirteen ethnic groups with less than 50 per cent of their workers in manual occupations, two are high on institutional completeness. Of the fourteen with 50 per cent or more of their workers in manual occupations, seven have a high degree of institutional completeness. (No occupational information was available for three of the ethnic groups.)

others from the native village or city.[16] Such factors provide very different sets of conditions for the development of ethnic institutions.

Ethnic communities are formed, grow, and disappear; they go through a life-cycle.[17] They probably begin either as simple aggregates of individuals, as amorphous informal groups, or as fairly well-structured informal groups. This group can constitute a public for ethnic organizations, a set of opportunities for social entrepreneurs. The organizations established by these entrepreneurs will maintain themselves as long as a public exists to use their services, or as long as the ethnic identity of the organization is important for the members of the ethnic group. The very existence of such organizations, as the findings of this paper show, act to strengthen this identity. But other mechanisms also operate, such as the fact that the leaders of the organizations have a vested interest in these organizations and will attempt in various ways to strengthen the ethnic identity so as to keep their public as large as possible.

If the rate of migration is low or nil, the ethnic public will progressively decrease, because even a high degree of institutional completeness will not prevent some integration into the native community. With time —and it may be quite long—the ethnic organizations will themselves disappear or lose their ethnic identity, completing the life-cycle of the community.

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[16] See W. Peterson, "A General Typology of Migration," *American Sociological Review*, XXIII (1958), 256–66; and C. A. Price, "Immigration and Group Settlement," in W. D. Borrie, *The Cultural Integration of Immigrants* (New York: UNESCO, 1959).

[17] The community life-cycle approach to the study of the integration and acculturation of immigrants is well represented by W. I. Thomas and F. Znaniecki, *The Polish Peasant in Europe and America* (Boston, 1920); and E. A. Galitzi, "A Study of Assimilation among the Roumanians in the United States" (unpublished Ph.D. dissertation, Columbia University, 1929).

Police Encounters with Juveniles[1]

Irving Piliavin and Scott Briar

ABSTRACT

In an observational study of police officers' contacts with juveniles the authors reached these conclusions: (1) Wide discretion was exercised by policemen in dealing with youthful offenders. (2) The exercise of this discretion was affected by a few readily observable criteria, including boys' prior offense records, race, grooming, and demeanor. Among first offenders particularly, but to some degree among all offenders, a youth's demeanor was a major criterion for determining what police disposition he would be given. Officers estimated that 50–60 per cent of first offense dispositions were based on this criterion. (3) The differential in arrest and apprehension rates between Negroes and whites was not simply a consequence of a greater offense rate among the former or police bias. To some extent this differential was due to the fact that Negroes more often than Caucasians exhibited those aspects of demeanor associated by officers with "true" delinquent boys.

As the first of a series of decisions made in the channeling of youthful offenders through the agencies concerned with juvenile justice and corrections, the disposition decisions made by police officers have potentially profound consequences for apprehended juveniles. Thus arrest, the most severe of the dispositions available to police, may not only lead to confinement of the suspected offender but also bring him loss of social status, restriction of educational and employment opportunities, and future harassment by law-enforcement personnel.[2] According to some criminologists, the stigmatization resulting from police apprehension, arrest, and detention actually reinforces deviant behavior.[3] Other authorities have suggested, in fact, that this stigmatization serves as the catalytic agent initiating delinquent careers.[4] Despite their presumed significance, however, little empirical analysis has been reported regarding the factors influencing, or consequences resulting from, police actions with juvenile offenders. Furthermore, while some studies of police encounters with adult offenders have been reported, the extent to which the findings of these investigations pertain to law-enforcement practices with youthful offenders is not known.[5]

The above considerations have led the writers to undertake a longitudinal study of the conditions influencing, and consequences flowing from, police actions with juveniles. In the present paper findings

[1] This study was supported by Grant MH-06328-02, National Institute of Mental Health, United States Public Health Service.

[2] Richard D. Schwartz and Jerome H. Skolnick, "Two Studies of Legal Stigma," *Social Problems*, X (April, 1962), 133–42; Sol Rubin, *Crime and Juvenile Delinquency* (New York: Oceana Publications, 1958); B. F. McSally, "Finding Jobs for Released Offenders," *Federal Probation*, XXIV (June, 1960), 12–17; Harold D. Lasswell and Richard C. Donnelly, "The Continuing Debate over Responsibility: An Introduction to Isolating the Condemnation Sanction," *Yale Law Journal*, LXVIII (April, 1959), 869–99.

[3] Richard A. Cloward and Lloyd E. Ohlin, *Delinquency and Opportunity* (Glencoe, Ill.: Free Press, 1960), pp. 124–30.

[4] Frank Tannenbaum, *Crime and the Community* (New York: Columbia University Press, 1936), pp. 17–20; Howard S. Becker, *Outsiders: Studies in the Sociology of Deviance* (New York: Free Press of Glencoe, 1963), chaps. i and ii.

[5] For a detailed accounting of police discretionary practices, see Joseph Goldstein, "Police Discretion Not To Invoke the Criminal Process: Low Visibility Decisions in the Administration of Justice," *Yale Law Journal*, LXIX (1960), 543–94; Wayne R. LaFave, "The Police and Non-enforcement of the Law—Part I," *Wisconsin Law Review*, January, 1962, pp. 104–37; S. H. Kadish, "Legal Norms and Discretion in the Police and Sentencing Processes," *Harvard Law Review*, LXXV (March, 1962), 904–31.

will be presented indicating the influence of certain factors on police actions. Research data consist primarily of notes and records based on nine months' observation of all juvenile officers in one police department.[6] The officers were observed in the course of their regular tours of duty.[7] While these data do not lend themselves to quantitative assessments of reliability and validity, the candor shown by the officers in their interviews with the investigators and their use of officially frowned-upon practices while under observation provide some assurance that the materials presented below accurately reflect the typical operations and attitudes of the law-enforcement personnel studied.

The setting for the research, a metropolitan police department serving an industrial city with approximately 450,000 inhabitants, was noted within the community it served and among law-enforcement officials elsewhere for the honesty and superior quality of its personnel. Incidents involving criminal activity or brutality by members of the department had been extremely rare during the ten years preceding this study; personnel standards were comparatively high; and an extensive training program was provided to both new and experienced personnel. Juvenile Bureau members, the primary subjects of this investigation, differed somewhat from other members of the department in that they were responsible for delinquency prevention as well as law enforcement, that is, juvenile officers were expected to be knowledgeable about conditions leading to crime and delinquency and to be able to work with community agencies serving known or potential juvenile offenders. Accordingly, in the assignment of personnel to the Juvenile Bureau, consideration was given not only to an officer's devotion to and reliability in law enforcement but also to his commitment to delinquency prevention. Assignment to the Bureau was of advantage to policemen seeking promotions. Consequently, many officers requested transfer to this unit, and its personnel comprised a highly select group of officers.

In the field, juvenile officers operated essentially as patrol officers. They cruised assigned beats and, although concerned primarily with juvenile offenders, frequently had occasion to apprehend and arrest adults. Confrontations between the officers and juveniles occurred in one of the following three ways, in order of increasing frequency: (1) encounters resulting from officers' spotting officially "wanted" youths; (2) encounters taking place at or near the scene of offenses reported to police headquarters; and (3) encounters occurring as the result of officers' directly observing youths either committing offenses or in "suspicious circumstances." However, the probability that a confrontation would take place between officer and juvenile, or that a particular disposition of an identified offender would be made, was only in part determined by the knowledge that an offense had occurred or that a particular juvenile had committed an offense. The bases for and utilization of non-offenses related criteria by police in accosting and disposing of juveniles are the focuses of the following discussion.

SANCTIONS FOR DISCRETION

In each encounter with juveniles, with the minor exception of officially "wanted" youths,[8] a central task confronting the officer was to decide what official action to take against the boys involved. In making

[6] Approximately thirty officers were assigned to the Juvenile Bureau in the department studied. While we had an opportunity to observe all officers in the Bureau during the study, our observations were concentrated on those who had been working in the Bureau for one or two years at least. Although two of the officers in the Juvenile Bureau were Negro, we observed these officers on only a few occasions.

[7] Although observations were not confined to specific days or work shifts, more observations were made during evenings and weekends because police activity was greatest during these periods.

[8] "Wanted" juveniles usually were placed under arrest or in protective custody, a practice which in effect relieved officers of the responsibility for deciding what to do with these youths.

these disposition decisions, officers could select any one of five discrete alternatives:

1. outright release
2. release and submission of a "field interrogation report" briefly describing the circumstances initiating the police-juvenile confrontation
3. "official reprimand" and release to parents or guardian
4. citation to juvenile court
5. arrest and confinement in juvenile hall.

Dispositions 3, 4, and 5 differed from the others in two basic respects. First, with rare exceptions, when an officer chose to reprimand, cite, or arrest a boy, he took the youth to the police station. Second, the reprimanded, cited, or arrested boy acquired an official police "record," that is, his name was officially recorded in Bureau files as a juvenile violator.

Analysis of the distribution of police disposition decisions about juveniles revealed that in virtually every category of offense the full range of official disposition alternatives available to officers was employed. This wide range of discretion resulted primarily from two conditions. First, it reflected the reluctance of officers to expose certain youths to the stigmatization presumed to be associated with official police action. Few juvenile officers believed that correctional agencies serving the community could effectively help delinquents. For some officers this attitude reflected a lack of confidence in rehabilitation techniques; for others, a belief that high case loads and lack of professional training among correctional workers vitiated their efforts at treatment. All officers were agreed, however, that juvenile justice and correctional processes were essentially concerned with apprehension and punishment rather than treatment. Furthermore, all officers believed that some aspects of these processes (e.g., judicial definition of youths as delinquents and removal of delinquents from the community), as well as some of the possible consequences of these processes (e.g., intimate institutional contact with "hard-core" delinquents, as well as parental, school, and conventional peer disapproval or rejection), could reinforce what previously might have been only a tentative proclivity toward delinquent values and behavior. Consequently, when officers found reason to doubt that a youth being confronted was highly committed toward deviance, they were inclined to treat him with leniency.

Second, and more important, the practice of discretion was sanctioned by police-department policy. Training manuals and departmental bulletins stressed that the disposition of each juvenile offender was not to be based solely on the type of infraction he committed. Thus, while it was departmental policy to "arrest and confine all juveniles who have committed a felony or misdemeanor involving theft, sex offense, battery, possession of dangerous weapons, prowling, peeping, intoxication, incorrigibility, and disturbance of the peace," it was acknowledged that "such considerations as age, attitude and prior criminal record might indicate that a different disposition would be more appropriate."[9] The official justification for discretion in processing juvenile offenders, based on the preventive aims of the Juvenile Bureau, was that each juvenile violator should be dealt with solely on the basis of what was best for him.[10] Unofficially, administrative legitimation of discretion was further justified on the grounds that strict enforcement practices would overcrowd court calendars and detention facilities, as well as dramatically increase juvenile crime rates—consequences to be avoided because they would expose the police department to community criticism.[11]

In practice, the official policy justifying use of discretion served as a demand that discretion be exercised. As such, it posed

[9] Quoted from a training manual issued by the police department studied in this research.

[10] Presumably this also implied that police action with juveniles was to be determined partly by the offenders' need for correctional services.

[11] This was reported by beat officers as well as supervisory and administrative personnel of the juvenile bureau.

three problems for juvenile officers. First, it represented a departure from the traditional police practice with which the juvenile officers themselves were identified, in the sense that they were expected to justify their juvenile disposition decisions not simply by evidence proving a youth had committed a crime—grounds on which police were officially expected to base their dispositions of non-juvenile offenders[12]—but in the *character* of the youth. Second, in disposing of juvenile offenders, officers were expected, in effect, to make judicial rather than ministerial decisions.[13] Third, the shift from the offense to the offender as the basis for determining the appropriate disposition substantially increased the uncertainty and ambiguity for officers in the situation of apprehension because no explicit rules existed for determining which disposition different types of youths should receive. Despite these problems, officers were constrained to base disposition decisions on the character of the apprehended youth, not only because they wanted to be fair, but because persistent failure to do so could result in judicial criticism, departmental censure, and, they believed, loss of authority with juveniles.[14]

DISPOSITION CRITERIA

Assessing the character of apprehended offenders posed relatively few difficulties for officers in the case of youths who had committed serious crimes such as robbery, homicide, aggravated assault, grand theft, auto theft, rape, and arson. Officials generally regarded these juveniles as confirmed delinquents simply by virtue of their involvement in offenses of this magnitude.[15] However, the infraction committed did not always suffice to determine the appropriate disposition for some serious offenders;[16] and, in the case of minor offenders, who comprised over 90 per cent of the youths against whom police took action, the violation per se generally played an insignificant role in the choice of disposition. While a number of minor offenders were seen as serious delinquents deserving arrest, many others were perceived either as "good" boys whose offenses were atypical of their customary behavior, as pawns of undesirable associates or, in any case, as boys for whom arrest was regarded as an unwarranted and possibly harmful punishment. Thus, for nearly all minor violators and for some serious delinquents, the assessment of character—the distinction between serious delinquents, "good" boys, misguided youths, and so on—and the dispositions which followed from these assessments were based on youths' personal characteristics and not their offenses.

Despite this dependence of disposition decisions on the personal characteristics of these youths, however, police officers actually had access only to very limited information about boys at the time they had to decide what to do with them. In the field, officers typically had no data concerning the past offense records, school performance, family situation, or personal adjustment of apprehended youths.[17] Fur-

[12] In actual practice, of course, disposition decisions regarding adult offenders also were influenced by many factors extraneous to the offense per se.

[13] For example, in dealing with adult violators, officers had no disposition alternative comparable to the reprimand-and-release category, a disposition which contained elements of punishment but did not involve mediation by the court.

[14] The concern of officers over possible loss of authority stemmed from their belief that court failure to support arrests by appropriate action would cause policemen to "lose face" in the eyes of juveniles.

[15] It is also likely that the possibility of negative publicity resulting from the failure to arrest such violators—particularly if they became involved in further serious crime—brought about strong administrative pressure for their arrest.

[16] For example, in the year preceding this research, over 30 per cent of the juveniles involved in burglaries and 12 per cent of the juveniles committing auto theft received dispositions other than arrest.

[17] On occasion, officers apprehended youths whom they personally knew to be prior offenders. This did not occur frequently, however, for several reasons. First, approximately 75 per cent of appre-

thermore, files at police headquarters provided data only about each boy's prior offense record. Thus both the decision made in the field—whether or not to bring the boy in—and the decision made at the station—which disposition to invoke—were based largely on cues which emerged from the interaction between the officer and the youth, cues from which the officer inferred the youth's character. These cues included the youth's group affiliations, age, race, grooming, dress, and demeanor. Older juveniles, members of known delinquent gangs, Negroes, youths with well-oiled hair, black jackets, and soiled denims or jeans (the presumed uniform of "tough" boys), and boys who in their interactions with officers did not manifest what were considered to be appropriate signs of respect tended to receive the more severe dispositions.

TABLE 1

SEVERITY OF POLICE DISPOSITION BY YOUTH'S DEMEANOR

Severity of Police Disposition	Youth's Demeanor		Total
	Co-operative	Unco-operative	
Arrest (most severe)	2	14	16
Citation or official reprimand	4	5	9
Informal reprimand	15	1	16
Admonish and release (least severe)	24	1	25
Total	45	21	66

Other than prior record, the most important of the above clues was a youth's *demeanor*. In the opinion of juvenile patrolmen themselves the demeanor of apprehended juveniles was a major determinant of their decisions for 50–60 per cent of the juvenile cases they processed.[18] A less subjective indication of the association between a youth's demeanor and police disposition is provided by Table 1, which presents the police dispositions for sixty-six youths whose encounters with police were observed in the course of this study.[19] For purposes of this analysis, each youth's demeanor in the encounter was classified as either co-operative or unco-operative.[20] The results clearly reveal a marked association between youth demeanor and the severity of police dispositions.

The cues used by police to assess demeanor were fairly simple. Juveniles who were contrite about their infractions, respectful to officers, and fearful of the sanctions that might be employed against them tended to be viewed by patrolmen as basically law-abiding or at least "salvageable." For these youths it was usually assumed that informal or formal reprimand would suffice to guarantee their future conformity. In contrast, youthful offenders who were fractious, obdurate, or who appeared nonchalant in their encounters with patrolmen

hended youths had no prior official records; second, officers periodically exchanged patrol areas, thus limiting their exposure to, and knowledge about, these areas; and third, patrolmen seldom spent more than three or four years in the juvenile division.

[18] While reliable subgroup estimates were impossible to obtain through observation because of the relatively small number of incidents observed, the importance of demeanor in disposition decisions appeared to be much less significant with known prior offenders.

[19] Systematic data were collected on police encounters with seventy-six juveniles. In ten of these encounters the police concluded that their suspicions were groundless, and consequently the juveniles involved were exonerated; these ten cases were eliminated from this analysis of demeanor. (The total number of encounters observed was considerably more than seventy-six, but systematic data-collection procedures were not instituted until several months after observations began.)

[20] The data used for the classification of demeanor were the written records of observations made by the authors. The classifications were made by an independent judge not associated with this study. In classifying a youth's demeanor as co-operative or unco-operative, particular attention was paid to: (1) the youth's responses to police officers' questions and requests; (2) the respect and deference—or lack of these qualities—shown by the youth toward police officers; and (3) police officers' assessments of the youth's demeanor.

were likely to be viewed as "would-be tough guys" or "punks" who fully deserved the most severe sanction: arrest. The following excerpts from observation notes illustrate the importance attached to demeanor by police in making disposition decisions.

1. The interrogation of "A" (an 18-year-old upper-lower-class white male accused of statutory rape) was assigned to a police sergeant with long experience on the force. As I sat in his office while we waited for the youth to arrive for questioning, the sergeant expressed his uncertainty as to what he should do with this young man. On the one hand, he could not ignore the fact that an offense had been committed; he had been informed, in fact, that the youth was prepared to confess to the offense. Nor could he overlook the continued pressure from the girl's father (an important political figure) for the police to take severe action against the youth. On the other hand, the sergeant had formed a low opinion of the girl's moral character, and he considered it unfair to charge "A" with statutory rape when the girl was a willing partner to the offense and might even have been the instigator of it. However, his sense of injustice concerning "A" was tempered by his image of the youth as a "punk," based, he explained, on information he had received that the youth belonged to a certain gang, the members of which were well known to, and disliked by, the police. Nevertheless, as we prepared to leave his office to interview "A," the sergeant was still in doubt as to what he should do with him.

As we walked down the corridor to the interrogation room, the sergeant was stopped by a reporter from the local newspaper. In an excited tone of voice, the reporter explained that his editor was pressing him to get further information about this case. The newspaper had printed some of the facts about the girl's disappearance, and as a consequence the girl's father was threatening suit against the paper for defamation of the girl's character. It would strengthen the newspaper's position, the reporter explained, if the police had information indicating that the girl's associates, particularly the youth the sergeant was about to interrogate, were persons of disreputable character. This stimulus seemed to resolve the sergeant's uncertainty. He told the reporter, "unofficially," that the youth was known to be an undesirable person, citing as evidence his membership in the delinquent gang. Furthermore, the sergeant added that he had evidence that this youth had been intimate with the girl over a period of many months. When the reporter asked if the police were planning to do anything to the youth, the sergeant answered that he intended to charge the youth with statutory rape.

In the interrogation, however, three points quickly emerged which profoundly affected the sergeant's judgment of the youth. First, the youth was polite and co-operative; he consistently addressed the officer as "sir," answered all questions quietly, and signed a statement implicating himself in numerous counts of statutory rape. Second, the youth's intentions toward the girl appeared to have been honorable; for example, he said that he wanted to marry her eventually. Third, the youth was not in fact a member of the gang in question. The sergeant's attitude became increasingly sympathetic, and after we left the interrogation room he announced his intention to "get 'A' off the hook," meaning that he wanted to have the charges against "A" reduced or, if possible, dropped.

2. Officers "X" and "Y" brought into the police station a seventeen-year-old white boy who, along with two older companions, had been found in a home having sex relations with a fifteen-year-old girl. The boy responded to police officers' queries slowly and with obvious disregard. It was apparent that his lack of deference toward the officers and his failure to evidence concern about his situation were irritating his questioners. Finally, one of the officers turned to me and, obviously angry, commented that in his view the boy was simply a "stud" interested only in sex, eating, and sleeping. The policemen conjectured that the boy "probably already had knocked up half a dozen girls." The boy ignored these remarks, except for an occasional impassive stare at the patrolmen. Turning to the boy, the officer remarked, "What the hell am I going to do with you?" And again the boy simply returned the officer's gaze. The latter then said, "Well, I guess we'll just have to put you away for a while." An arrest report was then made out and the boy was taken to Juvenile Hall.

Although anger and disgust frequently characterized officers' attitudes toward

recalcitrant and impassive juvenile offenders, their manner while processing these youths was typically routine, restrained, and without rancor. While the officers' restraint may have been due in part to their desire to avoid accusation and censure, it also seemed to reflect their inurement to a frequent experience. By and large, only their occasional "needling" or insulting of a boy gave any hint of the underlying resentment and dislike they felt toward many of these youths.[21]

PREJUDICE IN APPREHENSION AND DISPOSITION DECISIONS

Compared to other youths, Negroes and boys whose appearance matched the delinquent stereotype were more frequently stopped and interrogated by patrolmen—often even in the absence of evidence that an offense had been committed[22]—and usually were given more severe dispositions for the same violations. Our data suggest, however, that these selective apprehension and disposition practices resulted not only from the intrusion of long-held prejudices of individual police officers but also from certain job-related experiences of law-enforcement personnel. First, the tendency for police to give more severe dispositions to Negroes and to youths whose appearance corresponded to that which police associated with delinquents partly reflected the fact, observed in this study, that these youths also were much more likely than were other types of boys to exhibit the sort of recalcitrant demeanor which police construed as a sign of the confirmed delinquent. Further, officers assumed, partly on the basis of departmental statistics, that Negroes and juveniles who "look tough" (e.g., who wear chinos, leather jackets, boots, etc.) commit crimes more frequently than do other types of youths.[23] In this sense, the police justified their selective treatment of these youths along epidemiological lines: that is, they were concentrating their attention on those youths whom they believed were most likely to commit delinquent acts. In the words of one highly placed official in the department:

> If you know that the bulk of your delinquent problem comes from kids who, say, are from 12 to 14 years of age, when you're out on patrol you are much more likely to be sensitive to the activities of juveniles in this age bracket than older or younger groups. This would be good law enforcement practice. The logic in our case is the same except that our delinquency problem is largely found in the Negro community and it is these youths toward whom we are sensitized.

As regards prejudice per se, eighteen of twenty-seven officers interviewed openly admitted a dislike for Negroes. However, they attributed their dislike to experiences they had, as policemen, with youths from this minority group. The officers reported that

[21] Officers' animosity toward recalcitrant or aloof offenders appeared to stem from two sources: moral indignation that these juveniles were self-righteous and indifferent about their transgressions, and resentment that these youths failed to accord police the respect they believed they deserved. Since the patrolmen perceived themselves as honestly and impartially performing a vital community function warranting respect and deference from the community at large, they attributed the lack of respect shown them by these juveniles to the latters' immorality.

[22] The clearest evidence for this assertion is provided by the overrepresentation of Negroes among "innocent" juveniles accosted by the police. As noted, of the seventy-six juveniles on whom systematic data were collected, ten were exonerated and released without suspicion. Seven, or two-thirds of these ten "innocent" juveniles were Negro, in contrast to the allegedly "guilty" youths, less than one-third of whom were Negro. The following incident illustrates the operation of this bias: One officer, observing a youth walking along the street, commented that the youth "looks suspicious" and promptly stopped and questioned him. Asked later to explain what aroused his suspicion, the officer explained, "He was a Negro wearing dark glasses at midnight."

[23] While police statistics did not permit an analysis of crime rates by appearance, they strongly supported officers' contentions concerning the delinquency rate among Negroes. Of all male juveniles processed by the police department in 1961, for example, 40.2 per cent were Negro and 33.9 per cent were white. These two groups comprised at that time, respectively, about 22.7 per cent and 73.6 per cent of the population in the community studied.

Negro boys were much more likely than non-Negroes to "give us a hard time," be unco-operative, and show no remorse for their transgressions. Recurrent exposure to such attitudes among Negro youth, the officers claimed, generated their antipathy toward Negroes. The following excerpt is typical of the views expressed by these officers:

They (Negroes) have no regard for the law or for the police. They just don't seem to give a damn. Few of them are interested in school or getting ahead. The girls start having illegitimate kids before they are 16 years old and the boys are always "out for kicks." Furthermore, many of these kids try to run you down. They say the damnedest things to you and they seem to have absolutely no respect for you as an adult. I admit I am prejudiced now, but frankly I don't think I was when I began police work.

IMPLICATIONS

It is apparent from the findings presented above that the police officers studied in this research were permitted and even encouraged to exercise immense latitude in disposing of the juveniles they encountered. That is, it was within the officers' discretionary authority, except in extreme limiting cases, to decide which juveniles were to come to the attention of the courts and correctional agencies and thereby be identified officially as delinquents. In exercising this discretion policemen were strongly guided by the demeanor of those who were apprehended, a practice which ultimately led, as seen above, to certain youths, (particularly Negroes[24] and boys dressed in the style of "toughs") being treated more severly than other juveniles for comparable offenses.

But the relevance of demeanor was not limited only to police disposition practices. Thus, for example, in conjunction with police crime statistics the criterion of demeanor led police to concentrate their surveillance activities in areas frequented or inhabited by Negroes. Furthermore, these youths were accosted more often than others by officers on patrol simply because their skin color identified them as potential troublemakers. These discriminatory practices—and it is important to note that they are discriminatory, even if based on accurate statistical information—may well have self-fulfilling consequences. Thus it is not unlikely that frequent encounters with police, particularly those involving youths innocent of wrongdoing, will increase the hostility of these juveniles toward law-enforcement personnel. It is also not unlikely that the frequency of such encounters will in time reduce their significance in the eyes of apprehended juveniles, thereby leading these youths to regard them as "routine." Such responses to police encounters, however, are those which law-enforcement personnel perceive as indicators of the serious delinquent. They thus serve to vindicate and reinforce officers' prejudices, leading to closer surveillance of Negro districts, more frequent encounters with Negro youths, and so on in a vicious circle. Moreover, the consequences of this chain of events are reflected in police statistics showing a disproportionately high percentage of Negroes among juvenile offenders, thereby providing "objective" justification for concentrating police attention on Negro youths.

To a substantial extent, as we have implied earlier, the discretion practiced by juvenile officers is simply an extension of the juvenile-court philosophy, which holds that in making legal decisions regarding juveniles, more weight should be given to the juvenile's character and life-situation than to his actual offending behavior. The juvenile officer's disposition decisions—and the information he uses as a basis for them—are more akin to the discriminations made by probation officers and other correctional workers than they are to decisions of police officers dealing with non-juvenile offenders. The problem is that such clinical-type decisions are not restrained by mechanisms comparable to the principles of due process

[24] An unco-operative demeanor was presented by more than one-third of the Negro youths but by only one-sixth of the white youths encountered by the police in the course of our observations

and the rules of procedure governing police decisions regarding adult offenders. Consequently, prejudicial practices by police officers can escape notice more easily in their dealings with juveniles than with adults.

The observations made in this study serve to underscore the fact that the official delinquent, as distinguished from the juvenile who simply commits a delinquent act, is the product of a social judgment, in this case a judgment made by the police. He is a delinquent because someone in authority has defined him as one, often on the basis of the public face he has presented to officials rather than of the kind of offense he has committed.

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COMMENTARY AND DEBATES

Organizational Size and Managerial Succession: A Re-examination[1]

The relationship between managerial succession and organizational size has been examined by both sociologists and economists. However, the conclusions arrived at by sociologists Grusky and Kriesberg, and by the economist Roberts, are contradictory.[2] Roberts studied the rates of executive succession over a three-year period in the largest companies in the country and found no relationship between organizational size and succession, holding constant age of executive, compensation, and other possibly related factors. Grusky, on the other hand, concluded that there is a direct relationship between organizational size and succession over a ten-year period for the 500 largest companies in the United States. Kriesberg, in a study of state and local mental health agencies and public health departments, reported findings similar to Grusky's, although he did not control age of the officials as did Grusky. As knowledge of a relation between organizational size and succession could contribute to an understanding of organizational dynamics, the contradiction between Roberts' data on the one hand and those of Grusky and Kriesberg on the other warrants further investigation. To this end, the first step taken was to re-examine the methodology and inferences of the three researchers.

No substantive weakness in methodology and in inferences were found in Roberts' study, while several factors in the other two studies might lead one to question both Grusky's and Kriesberg's conclusions.

Grusky examined succession in five positions: chairman of the board, president, secretary, comptroller, and treasurer. For each of the five positions he concluded that "frequency of administrative succession is directly related to size of the organization."[3] Initially Grusky examined succession rates in the twenty-five largest and twenty-five smallest companies as ranked by sales volume in the 1959 *Fortune* magazine list of 500 largest companies in the United States, that is, companies ranked 1 through 26 and companies 474 through 500 (omitting companies for which data were not available). He then "repeated" the initial study by comparing the twenty-five next largest firms on which data could be obtained, companies 28–53, with the twenty-five next smallest firms, companies ranked 446 through 473 inclusive. Grusky stated that the data dealing with succession were derived from two sources:

> "Moody's Industrial Manual," 1959, and "Poor's Register of Directors and Executives," 1949. *By taking as evidence of succession a change in names listed for key job titles* in these two volumes over the ten-year period 1949–59, we were able to secure data for most of the firms selected for study. This technique did not permit us to ascertain how many successions occurred within each office, but we could compare *firms which were qualitatively different, namely, those in which succession had occurred and those in which the incumbent remained in office.*[4] [Italics ours.]

[1] We wish to thank Sue Marquis and Ross Archibald for their aid in collecting and processing data.

[2] Oscar Grusky, "Organizational Size and Managerial Succession," *American Journal of Sociology*, LXVII (1961), 261–69; Louis Kriesberg, "Careers, Organization Size, and Succession," *American Journal of Sociology*, LXVIII (1962), 355–59; David Roberts, *Executive Compensation* (Glencoe, Ill.: Free Press, 1959). Roberts' book was first pointed out to us by Thomas L. Whisler.

[3] Grusky, *op. cit.*, p. 269.

[4] *Ibid.*, pp. 262–63.

Grusky thus examined four groups of twenty-five companies, but rather than plotting succession rate against the four groups by company size, he compared group 1, the largest companies, to group 4, the smallest; and he also compared group 2 (the next largest) to group 3 (the next smallest). He treated the two comparisons as independent tests of his hypothesis. As a consequence of this treatment, Grusky was able to conclude that there is a direct relationship between size and succession. However, when the rate of succession for each position is examined simultaneously for all four groups (see Table 1, cols. [1], [4], [7], and [10]), it can be seen that there is no direct relationship between size of organization and succession for four of the five positions.

Kriesberg, following Grusky, studied the relationship between organizational size and succession in mental health and public health agencies. For both types of agency he compared state-wide agencies having less than 500 employees with those having more than 500 employees and then compared local agencies having less than 30 employees with those having more than 30 employees. He concluded that generally tenure is inversely related to organizational size and thus, by inference, that succession is directly related to organizational size. Kriesberg noted, however, the following exceptions: "Among local public health officials, there is no relationship between organization size and tenure, and among local mental health heads, the relationship appears to be inverse from what we have seen at the state level; now tenure is somewhat longer in large organizations."[5] If one treats state and local agencies as differing only in size, then an examination of Kriesberg's data reveals that the greatest succession in mental health agencies is associated with both the largest *and smallest groups* and that in public health agencies *least succession occurs in the second largest group.*

While an examination of data presented in support of the hypothesis that there is a direct relationship between size and succession leaves one somewhat skeptical, it does not allow one categorically to reject such a relationship. For instance, Roberts reported yearly succession rates for a period of three years; Grusky, succession rates for one period of ten years. It is possible that the relationship between size and succession does not become manifest within a short period. Second, there may be factors about local and state agencies which cause them to have different rates of succession. Finally, while the relationship through the four groups of companies studied by Grusky was not direct, there did appear a tendency for over-all succession to be greater in the two larger groups combined than in the two smaller groups combined. The last fact is an important one, but the fact that Grusky considered only the extreme groups of companies on the *Fortune* list, omitting companies ranked 54 through 445, also is important. Without the "middle"-ranked companies it is difficult to determine whether the existence of differences in extreme groups stems from some non-linear relationship or from a direct relationship or, given the small number of points utilized by Grusky, from chance variation.

Since reanalysis of the evidence presented by Grusky and Kriesberg does not allow us either to reject or accept their conclusions, we decided to determine rates of executive succession for the middle-ranked companies not included in the study by Grusky. It was hoped that these data would provide enough additional information to reject or accept the hypothesis that there is a direct relationship between organizational size and succession.

Succession rates for all companies ranked 54 through 445 in the 1959 *Fortune* list of 500, grouped by size into categories of twenty-five, were determined by following the procedures described by Grusky. As a check on our use of his procedures, we replicated Grusky's study and found what

[5] Kriesberg, *op. cit.*, p. 357.

TABLE 1

REPLICATION OF GRUSKY'S STUDY OF RATE OF SUCCESSION AS A FUNCTION OF COMPANY SIZE
(Per Cent)

Position	Rank of Company											
	1-26			28-53*			446-73			474-500		
	Grusky (1)	Replication† w Grusky Code (2)	Replication‡ w/Grusky Definition (3)	Grusky (4)	Replication† w/Grusky Code (5)	Replication‡ w Grusky Definition (6)	Grusky (7)	Replication† w Grusky Code (8)	Replication‡ w Grusky Definition (9)	Grusky (10)	Replication† w Grusky Code (11)	Replication‡ w/Grusky Definition (12)
Chairman . . .	75	74	78	52	48	61	40	50	80	56	56	60
President	76	76	76	92	92	92	64	60	60	60	60	60
Treasurer .	76	80	80	88	84	84	76	80	80	56	58	58
Comptroller	67	88	94	71	75	78 9	17	45 5	71	50	50	80
Secretary. . .	83	88	88	71	68	68	68	68	68	57	54	57

* Company No 27 was omitted because Grusky apparently did, although he did not mention it as one for which there was insufficient information.

† This replication duplicated Grusky's procedures as he specified them in our personal communication

‡ This replication represents an operationalization of Grusky's conceptual definition of no succession as he stated it in his article.

appeared to us to be a number of errors in his work.

Subsequent personal communication with Grusky, in which he made available to us his code, revealed that the majority of these differences arose from discrepancies in our definitions of no succession. Grusky coded as instances of no succession situations where a position was listed in 1949 but not in 1959. Grusky's results and ours, using his code, are presented in Table 1 under the columns headed "Grusky" and "Replication with Grusky Code." If our replication is accurate[6] the discrepancies between the two studies, while small, have the effect of weakening his conclusions. Where Grusky found greater succession for chairmen and for secretaries in companies 28–53 than in companies 446–73, we find no differences for secretaries and a reversal of the differences for chairmen. We also found an error in succession rates for comptrollers in companies 446-73.

The data in columns headed "Replication with Grusky Definition" in Table 1 and all other data presented, except where otherwise noted, arise from the use of Grusky's definition of no succession, that is, "where the incumbent remained in office," rather than his actual coding of it. Grusky coded as no succession the case where a position was listed in 1949 but not in 1959. We believe the stated definition is the more valid one for the following reasons. (1) Where a position is listed in 1949 but not in 1959, succession could have occurred and so such a case should not be treated as no succession. (2) The absence of the listing in 1959 might be due to a clerical error. Comparing Moody's and Poor's for the same year we found, for companies ranked 446-73, four instances out of a possible eleven in which a comptroller was not listed in one of the sources but was in the other. (3) Coding a position listed in 1949 but not in 1959 as no succession, while ignoring positions not listed in 1949 and listed in 1959, severely biases the data in favor of the hypothesis that size and succession are positively related. The bias arises from the fact that there is a greater variability in the use of position titles by small firms. We found, for example, that in twelve instances the smallest twenty-five companies did not list a particular position in 1949 but did in 1959; but this was true for the largest companies only eight times. Counting that situation as no succession, as Grusky did, increases the no succession rate for the small companies. This bias could be offset by counting as no succession a listing in 1949 but not in 1959, which occurred eight times in the smallest companies and twice in the largest. For these three reasons we coded "listed in 1949 but not 1959" and "not listed in 1949 but listed in 1959" as insufficient data to determine whether or not there was any succession.

When one examines the rates of succession for the five positions across the entire 500 companies (less those for which data were unavailable—giving us a total of nineteen rather than twenty groups of companies),[7] it can be seen that in only one position, secretary, is the highest rate of succession associated with the group of largest companies. For three of the five positions, highest rates of succession are found in companies ranked 251–500. Moreover, as can be seen from Table 2, for no position does there appear to be a direct relationship between organizational size and succession.

Despite the fact that there is no direct relationship between size and succession for any of the five positions, the possibility

[6] Aside from the discrepancies noted in the text, a number of other discrepancies can also be seen in Table 2. The existence of these discrepancies led us to recode the rates of succession. Initially two persons coded all the data separately, and their data were compared and errors corrected. This procedure was replicated by two different coders and the results of this coding were compared with those of the first coding.

[7] The succession rates for the following ranked companies were not included in the analysis due to lack of information: 20, 44, 59, 73, 108, 135, 200, 225, 228, 248, 269, 286, 360, 382, 385, 395, 407, 429, 438, 448, 450, 462, 492, 499.

TABLE 2

RATE OF EXECUTIVE SUCCESSION BY POSITION AND BY POSITIONS COMBINED FOR 476 OF THE 500 LARGEST COMPANIES AS LISTED BY *Fortune*

(Per Cent)

Position	Rank of Organization																		
	1–26	27–52	53–79	80–104	105–30	131–56	157–81	182–207	208–34	235–61*	262–88	289–313	314–38	339–64	365–91	392–418	419–45	446–73	474–500
Chairman....	77.8	66.7	86.7	72.7	72.7	64.3	63.6	71.4	72.7	46.1	69.2	60	55.5	57.1	71.4	83.3	87.5	80	60
President....	76	92	88	65.2	92	80	70.8	76	68	76	75	75	66.7	75	80	88	64	60	60
Treasurer....	80	84	84	91.7	84	75	76	68	84	76.9	73.9	80	56	95.8	79.2	66.7	79.2	80	58.3
Comptroller..	93.7	78.9	90	76.5	88.9	64.7	77.8	73.3	76.9	80	81.8	100	40	68.7	60	75	92.9	71.4	80
Secretary.....	88	72	62.5	64	69.6	68	52	60.9	80	64	65.2	76	84	80	73.9	87.5	79.2	68	56.5
Over-all....	82.5	79.5	81.6	74	82.3	71.4	67.9	69.6	76.7	70.1	72.3	78.1	63.3	79.2	75.3	80	77.9	70.7	60.8

* In this group are twenty-six companies unlike all other groups which are made up of tweny five companies

existed that the relationship between size and succession was not of sufficient magnitude to be measurable within a given position. Therefore succession rates for the five positions were combined and the over-all succession rates were plotted against size of organization. Although the over-all succession rate is lowest in the group of smallest companies, there does not appear to be direct relationship between size and succession. The mean rate of succession over the nineteen groups is 74 per cent; the mean rate for the smallest nine groups is 73 per cent, and for the largest nine groups 76 per cent. Despite these small differences a conclusion of no direct relationship between size and succession leaves one slightly uncomfortable because of the low rate found in the group of twenty-five smallest companies. It could be possible that Grusky's conclusion is valid but that the effect is discernible only when companies smaller than those in the *Fortune* list of 500 are contrasted with those on the list.

TABLE 3

SUCCESSION RATES FOR 50 RANDOMLY SAMPLED COMPANIES RANKED 501–1,000 BY *Fortune*

(Per Cent)

Position	Organization Size	
	2,500 or Fewer Employees	More than 2,500 Employees
Chairman	70.0	66.7
President	76.0	68.0
Treasurer	82.6	68 0
Comptroller	88.9	90.0
Secretary	75.0	50 0
Over-all	78.0	65.6

Accordingly, a list of companies comprising the next largest 500 companies was obtained from *Fortune* magazine.[8] Although we knew that these companies ranked from 501 to 1,000 in terms of sales for 1959, the rank associated with each company was not available. We therefore examined the rates of excutive succession in a randomly selected sample of 10 per cent of these 500 companies. The mean rate of succession for this group of fifty companies was 71.7 per cent, a rate considerably higher than that found for companies ranked 474–500. Data of this sort lend strength to the feeling that the low rate of succession in the group 474–500 probably was due to factors other than size.

These fifty companies then were ranked by number of employees and split into two groups of twenty-five companies, one group having more than 2,500 employees, the other 2,500 or fewer employees. While this measure of company size (number of employees) differs from that used by Grusky (sales volume), it is the one used by Kriesberg in his study. Although sales volume is related to size, that relationship is far from perfect. For instance, an art dealer may have a relatively small staff and sell paintings worth more than a million dollars while a candy manufacturer may require a staff many times larger which produces a lower volume of sales than an art dealer. Procedures for determining succession were unchanged.

From Table 3 it can be seen that in four of the five positions succession is greater among the smaller companies. Over-all succession rates also follow the same pattern: 78 per cent succession in the group of smaller companies and 65.6 per cent succession in the group of larger companies. When the rate of succession for this group of companies is viewed with the other nineteen groups (see Fig. 1) the low rate of succession in companies ranked 474–500 assumes less importance.

To compare the combined effect of both sales volume and number of employees on succession a multiple regression of the following form was performed:

$$y = a + b_1x_1 + b_2x_2 + b_3x_1x_2 ,$$

[8] We wish to thank Mr. Edward Gunther, Director of Research, *Fortune* magazine, for making this information available to us.

where y is the percentage[9] of succession in each of the 480[10] of the 500 largest firms on which data were available, x_1 is the sales volume of the firm, and x_2 the number of employees.[11] The following results were obtained:

$$\begin{aligned} y = &\underset{(.015)}{.726} + \underset{(.00004)}{.00005}(x_1) + \underset{(.00092)}{.00036}(x_2) \\ &+ \underset{(.00000009)}{(-.00000012)}x_1x_2 . \end{aligned}$$

From the values of the coefficients it can be seen that the slopes of the regression lines are virtually non-existent; the lines are nearly parallel to the abscissa. The coefficient of multiple correlation, $R = .1074$ so that $R^2 = .011$, from which it can be concluded that the independent variables, sales volume and number of employees, together explain only about 1 per cent of the movement in the dependent variable, percentage rate of succession. Whether concluding on the basis of the grouped data or that of the regression analysis, of sales volume, number of employees, or both, Roberts' conclusion of no relation between organization size and succession is substantiated.

Hypotheses relating any organizational variable to size are difficult to test because no good definition of size exists as yet. Sales volume obviously is inadequate since it does not take into consideration such perhaps important differences as plant size or number of employees. Obviously a firm

[9] Percentage succession rate rather than the number of successions was used as the dependent variable because larger firms have a greater number of positions.

[10] Insufficient data were available for the companies ranked 20, 59, 73, 108, 135, 200, 225, 228, 248, 269, 286, 360, 382, 385, 395, 407, 429, 438, 448, 492. Companies ranked 44, 450, 462, and 499 were included in this analysis although they were omitted in the analysis summarized in Table 2 because Grusky originally excluded them in his analysis.

[11] Number of employees was obtained from *Moody's Industrial Manual*, 1959.

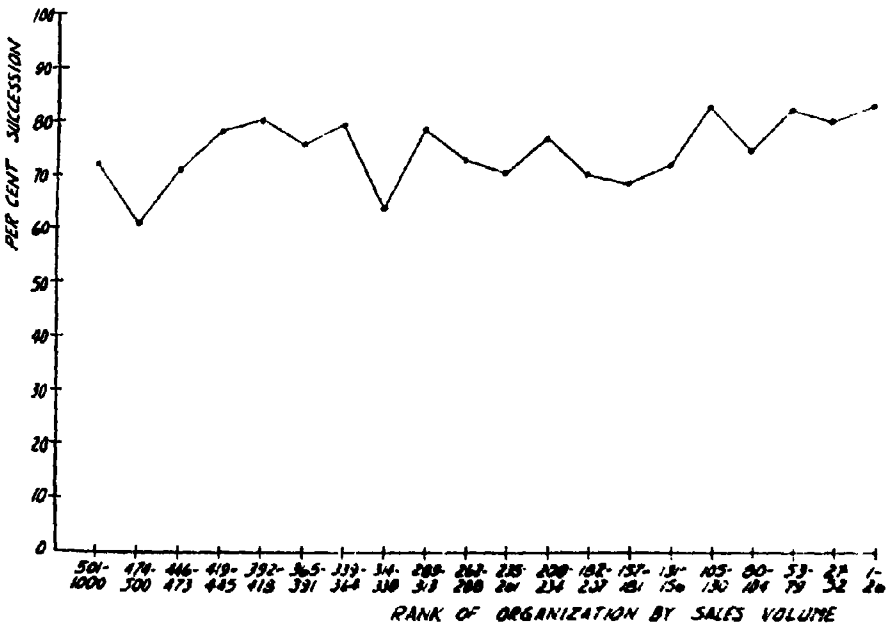


FIG. 1.—Over-all rates of succession for 476 of the 500 largest companies and for 50 randomly selected companies ranked from 501 to 1,000.

selling diamonds or paintings can have a huge dollar volume with little plant and few employees. Moreover, many important organizations do not have any sales, like the United States Army, various other parts of the government bureaucracy, school systems, unions, and political parties.

Number of employees is inadequate as a measure of size of organization since, in addition to its not being perfectly related to sales volume or plant size, automation and computers cause fluctuations in the number of employees necessary to an organization, so that while sales and plant may be increasing, number of employees may be decreasing. Also, it is not known whether all employees should be included in the measure or just the administrative hierarchy, or some ratio of the two. Perhaps eventually an index of size will be derived that includes both human assets and total assets.

Little can be said in conclusion beyond the now-obvious fact that the relationship between size of an organization and the rate at which its executives are replaced is not a simple, direct one. Providing new knowledge is more satisfying to the community of scholars than is the negation of previously accepted propositions. In this instance, however, we hope that some contribution has been made by the negative evidence in helping to redirect the efforts of future researchers.

GERALD GORDON
SELWYN BECKER

Graduate School of Business
University of Chicago

Reply

This is an excellent study. One of the strategic advantages of utilizing public data for research is that they permit precise replication studies such as the present one. (In this connection, sociologists might consider setting up procedures for routinely submitting copies of their codebooks to the American Documentation Institute or a similar agency so that investigators can undertake replications more readily.)

This is not meant to be critical of what is a fine contribution. However, Professors Becker and Gordon indicated that succession in five organizational positions was studied. In fact, findings were reported on a sixth, vice-president (see Table 4). Table 4 also specified the definition of succession used in the project. The failure to consider vice-presidents in the replication may have been due to the fact that their number was related to size of firm.

A major methodological weakness of my study was the focus on only a small number of executive positions. Large firms not only have greater number of vice-presidents, but also appear more likely than small firms to have other key executive positions such as assistant to the president and assistant vice-president. A more adequate test of the hypothesis that large firms replace their executives more rapidly than small ones would compare the proportion of successions in a firm during a given time period considering *all* top executive positions. Still another weakness was the short time period. I think it would be useful to examine a related and perhaps more important hypothesis proposing that a *change* in organizational size is directly related to a *change* in rate of executive succession. Such a test would be of relevance to bureaucratic theory, which was used to interpret the findings of the earlier study.

OSCAR GRUSKY

University of California
Los Angeles

Reply

I will confine my rejoinder to the comments Becker and Gordon make concerning my paper. First, a minor point: I did control for age of agency or program head; this was mentioned in footnotes 3 and 6. One might argue, however, that age should not be controlled. If the career line is such that a person does not become the head or a leading official in the largest organizations in his field until he is relatively old, this in itself has significance for the problem of succession.

Becker and Gordon "reanalyze" my data treating state and local agencies as differing only in size and find no direct relationship between size and length of tenure. This finding, and the failure of their detailed analysis to substantiate Grusky's findings, lead them to conclude that the findings reported in my paper are unsupported.

My major point, however, was that organizational size is related to succession only if certain conditions are met, and the relationship is likely to vary under different conditions. The conditions at the state and local level are different, as noted in the paper.[1] The condition emphasized in my paper is the career line of organization heads. I suggested that an itinerant career pattern independently affects the rate of succession and also interacts with size of organization to affect the rate; and I also suggested some conditions that affect the likelihood that an itinerant pattern would develop. Since Becker and Gordon already have data on officials in so many companies, it would be interesting to see whether or not differences in rate of succession, perhaps even related to size of organization, would be revealed if they did an industry-by-industry analysis. This would constitute an indirect test of some of the speculations ventured in my paper.

Becker and Gordon are correct in stressing the need for the accumulation of a considerable number of "non-theoretical" observations as well as for guiding theory. In the light of these considerations, their sweeping generalization on the basis of a negative finding is surprising. Rather, the integration of apparently contradictory findings by specification of the conditions under which each occurs would seem appropriate. I would hope that some of the admittedly speculative ideas in my paper may assist in this effort. Of course, a replication of the analysis presented in my paper, using more and better data, would undoubtedly modify and perhaps contradict the findings I reported; but Becker and Gordon have not done that.

LOUIS KRIESBERG

Syracuse University

[1] Other differences certainly are also important, although I did not discuss them in my paper. The local agencies often have different programs than the state agencies and, more significantly, they are frequently subsidiary agencies of the parent state bodies. See my full report, "Mental Health and Public Health Personnel in the Fifty States," (National Opinion Research Center, Report No. 83, 1962), esp. pp. 29–36.

A Criticism of an Empirical Assessment of the Concept of Bureaucracy on Conceptual Independence and Empirical Independence

When a considerable literature has accumulated on a given concept, it is a most reasonable procedure to analyze the works of various writers to determine what elements appear to some extent common among them. This Richard H. Hall has

done for the concept of bureaucracy.[1] He identifies six dimensions according to which an organization may be classified as more or less bureaucratic: (1) division of labor based upon functional specialization, (2) well-defined hierarchy of authority, (3) system of rules covering the rights and duties of positional incumbents, (4) system of procedures for dealing with work situations, (5) impersonality of interpersonal relations, and (6) promotion and selection for employment based upon technical competence. It seems to me that most students of organizations would probably agree that this list of dimensions is quite satisfactory and that the theoretically important criteria for assessing the degree of bureaucracy manifested by an organization have been included.

However, exception must be taken to the operational translation. Each dimension is measured by a scale of ten or twelve items that represent participants' perceptions.[2] In constructing these six scales, Hall reports that he eliminated items during the pretest phase of his study in order to achieve "maximal elimination of interdependency" among the scales, that is, items have been eliminated from each of the scales so that the correlation between any two scales will be as low as possible. In effect, the clear implication of his operations is that conceptual independence among a set of variables must result in empirical independence.

This type of unwarranted, and probably unrecognized, a priori assumption is not uncommon in studies which involve the construction of a number of scales or operational measures for a number of variables. It is easy to forget that the subjects who serve in the construction and pretesting phase of a study should be expected to show more or less the same relationships that will eventually be more confidently demonstrated in the final study. If the researcher has explicitly manipulated his measures to achieve independence in the pretest, presumably all that he can do is to establish this independence more reliably in his full-scale study. Certainly the final study can no longer be considered a *test* for independence; for a good job of constructing the measures *necessarily* means in this case that the possibility of disproving the hypothesis of independence does not exist. Furthermore, if the hypothesis of independence is in fact false, then this operation of constructing empirically independent scales must mean that one or more of the scales are not *valid* measures of their respective concepts.

Perhaps a crude analogy will make the point at issue here clear. Let us suppose an investigator wishes to test the hypothesis that the height and weight of persons vary independently. He works with his measures of each of these variables, pretesting and modifying them until he finally comes up with two measures that show minimal empirical correlation. He then applies these measures to a sample of persons and "demonstrates" that height and weight are "independent." Obviously, this exercise represents nothing less than a methodological tour de force. The "demonstration" is neither problematic nor meaningful. In addition, at least one of these measures can no longer be a valid measure of height or weight.

[1] "The Concept of Bureaucracy: An Empirical Assessment," *American Journal of Sociology*, LXIX (July, 1963), 32–40.

[2] An acceptable rationale is offered for the use of participants' perceptions of their organization as the basis for representing organizational structure. But in order to assess the reliability of such representation, some information on the within-groups variability must be furnished. Where there is a good deal of variability of perception among members of the same organization, the mean perception score can hardly be taken as a reliable measure and may obscure more than it reveals, about the nature of the organizations. As a matter of fact, it may well be that one important difference between organizations is the degree of consensus with which each organization is perceived by its members. For recent brief attention to this problem see Hanan C. Selvin and Warren O. Hagstrom, "The Empirical Classification of Formal Groups," *American Sociological Review*, XXVIII (June, 1963), 399–411, esp. n. 24.

It is hardly surprising that Hall "demonstrates" that the six dimensions of bureaucracy with which he deals vary independently.[3] What is surprising is that some of the correlation coefficients are as high as they are.[4] Indeed this would suggest that, in spite of his best efforts, the empirical world would not quite allow him complete freedom in constructing his scales. And this in turn adds to the suspicion that he could not have been faithful to the original concepts in his operational translations. For example, the demonstration of no correlation between Hall's measures of "hierarchy of authority" and "division of labor" makes us suspect that these scales do not represent the concepts theorists had in mind when analyzing hierarchy of authority and division of labor. What sorts of organizations would *not* be expected to show some relationship between a well-defined hierarchy of authority and a division of labor based upon functional specialization? Indeed, it is inconceivable that in our society, at least, a sample of organizations showing some variety could be found in which there would be no association between these two variables as they have been conceptualized by any of the writers to whom Hall refers. (That the relationship may be substantially less than perfect and that both of these variables should be attended to in making distinctions among organizations are propositions entirely different from that of independence.)

If an investigator seeks to establish the existence of given relationships empirically, it is well known that he must take pains not to do something in the preanalytic stage of research that will lead to observed relationships simply as the result of the bias in his procedures. It is less well recognized that an equally serious methodological error in the preanalytic stage of research is any procedure that predetermines to some extent that no consequent empirical relationships can be observed.[5] In particular, the design of instruments to measure variable whose empirical relationship is subsequently to be investigated must involve completely independent operations for each instrument. The contents of one instrument must in no way, either positively *or negatively*, be made dependent upon its relationship to any one of the other instruments.

David Gold

University of California
Santa Barbara

[3] Though this is Hall's major conclusion, he attributes some significance to the sign of the association between "technical qualifications" and three of the other dimensions, which, of course, negates the conclusion that the six bureaucratic dimensions vary independently.

[4] As a matter of statistical fact, Hall simply demonstrates that with a sample of only ten organizations, a correlation coefficient as high as .6 cannot be considered to differ "significantly" from zero. This is hardly convincing support of the assertion that the true correlation is zero. The *reductio ad absurdum* of simply using lack of statistical significance to "prove" independence is to do so by the mere expedient of reducing the sample *n*.

[5] This same type of error occurs in the analysis stage of some factor-analytic studies. The insistence upon orthogonal factors often results in variables the indicators (items) of which can be rationalized only by the most tortuous reasoning. In effect, the analyst has prejudged his case; he assumes that given variation is to be explained only in terms of a set of empirically independent variables (factors). No allowance is made for the possibility that variables which are conceptually independent, and perhaps most meaningful in a given study, may not be empirically independent.

Reply

Dr. Gold's criticism is based on a misunderstanding of the procedure used. I attempted to construct scales in accordance with the principle set forth in his note: "the design of instruments to measure variables whose empirical relationship is

subsequently to be investigated must involve completely independent construction operations for each instrument." The underlying purpose here is the construction of scales which would be impartial in respect to the hypothesis under investigation. Obviously, when scales are entangled, rejection of the hypothesis of independence is a foregone conclusion. On the other hand, if scales are contrived so as to yield uncorrelated measures, then the hypothesis of independence will necessarily be sustained. My intention was to construct scales which would allow a fair test of the hypothesis.

The source of the misunderstanding may well lie in the concept of interdependency. Interdependency refers to an overlap in content among scales which would necessarily lead to correlated measures and a rejection of the hypothesis of independence. I naturally intended to eliminate those items which would have resulted in correlated measures and which would have precluded a fair test of the hypothesis.

RICHARD H. HALL

Indiana University

Gumplowicz' *Outlines of Sociology*

It seems that the life and work of Ludwig Gumplowicz were pursued by misfortune. Although he was one of the creators of sociology, there has never been a large work written about his views. Professor I. L. Horowitz has written a comprehensive introduction to the sociology of Ludwig Gumplowicz in his edition of Gumplowicz' *Outlines of Sociology* (New York: Paine-Whitman, 1963).

This introduction is without doubt one of the best monographs concerning Gumplowicz' views on sociology. Unfortunately, Horowitz makes a few statements that are not true. I feel obliged to point out the misleading information supplied by Horowitz and its implications, as I am preparing a book on Gumplowicz and his relation with Lester F. Ward.

1. Horowitz writes that Gumplowicz Polish biographers were wrong in treating him "as a distinctively Polish product. (Cf. S. Posner [1911] and F. Mirek [1929].) The basis of this claim is partially supported by Gumplowicz' participation in the abortive Polish uprising of 1863 and his editing of a radical magazine, *Kraj* ('The Country')." According to Horowitz, they claimed this in spite of the fact of "his thorough immersion in the educational and cultural spheres of Austrian and German life" (p. 12).

Gumplowicz not only was educated in Poland but was also in touch with Polish intellectual life to the end of his days, and his relation with his Austrian and German environment was an unfriendly one. His identification with the Polish nation—aside from his Jewish background and radically liberal point of view—not only made difficulties in his academic career but also cut off his social contacts in university circles. Of course, his connection with German intellectual life undeniably existed, but Gumplowicz himself stressed that his theory "grew up on Polish soil" (*System Socjologii*, p. 3, edited in Warsaw, 1887).

2. Horowitz writes that Gumplowicz received his doctorate at the University of Vienna and that the topic of his dissertation was "Coordination of Historical Evolution and Its Relation to the Philosophy of the Testaments" (p. 13). Horowitz was badly informed. Gumplowicz received his doctorate at the Jagiellonian University in Cracow in 1862. He studied in Vienna for only one year. His dissertation topic was "Some Opinions from All Legal and Political Sciences" (*Zdania ze wszystkich umiejętnosci prawnych i politycznych*). Between 1864 and 1872 he published in Cracow five other historical and legal contributions, all in Polish.

3. Horowitz writes that "Gumplowicz was completely caught up in the claims and counter-claims of the Germanic Zeitgeist" (p. 13), and that Gumplowicz was born into a "Jewish family of Polish and Russian ancestry" (p. 12). The truth is

however, that Gumplowicz' family considered it an honor that one of its members had fought on the Polish side against Russia in 1831. In 1863 Gumplowicz' father was a trustee of the secret Polish "National Government," and his home was a meeting place for conspirators and a hospital for the wounded transported into Cracow from the part of Poland occupied by Russia. Two of Ludwig's brothers took part in the uprising, and he was in the weapon transportation service. Gumplowicz not only remained a Polish patriot but also raised his two sons to be Polish patriots and intellectuals.

4. Horowitz also writes that Gumplowicz' older son, who committed suicide, "had achieved a high professional status in social statistics" (p. 14). That, again, is not true. He was a brilliant young historian. (The second son later became a professor of social geography in Warsaw.)

5. Horowitz states inaccurately that "Gumplowicz was a professor of political science as well as sociology" (p. 34). Gumplowicz' professional life was occupied with law; on sociology he worked only in his spare time. When he began his work on sociology, there was no department of sociology in Central Europe.

6. Gumplowicz had cancer of the tongue, not of the eye, as Horowitz writes. Besides, it is misleading to emphasize that Gumplowicz' suicide resulted from a "nervous breakdown," since it rested on a quiet decision made in full consciousness of the situation.

Horowitz' analysis of Gumplowicz is seriously handicapped by these errors and by his neglect of the fact that Gumplowicz was, above all, a historian and theoretician of law. Gumplowicz arrived at his sociological concept of the "struggle of races" through his theory of the state and its origins. Others he took from his native literature. The so-called raid hypothesis (or "conquest hypothesis") had been present in Polish literature for some time and had been used by K. Szajnocha in 1858 and F. Piekosinski in 1881.

Gumplowicz was, as F. Oppenheimer noted, doubly without a country—as a Jew and as a Pole. Throughout his life he stressed that he was a Pole, which the Germans did not appreciate. He published the majority of his articles, reviews, and sociological serials in Polish newspapers, weeklies, and scholarly journals. In the Introduction to his major sociological work, *System Socjologii* (1887), Gumplowicz remarked how he was influenced by the political situation of his country. He stressed that his sociological concepts were rooted in his thinking as a student at the University of Cracow, and he noted that he first wrote his theory in German in order to receive an unbiased criticism from abroad.

It is possible that Horowitz' mistaken information about Gumplowicz' life history caused him also to fail to appreciate duly Gumplowicz' views as a reaction against the optimism of the nineteenth-century theory of progress.

ALEKSANDER GELLA

Polish Academy of Sciences, Warsaw

Reply

I am grateful for Professor Gella's remarks on my Introduction to Gumplowicz' *Outlines of Sociology*. It is indeed a misfortune that so significant a figure in sociological history should have been so shabbily treated by historical fortune. I certainly appreciate his kind words that my introduction "is without doubt one of the best monographs concerning Gumplowicz' views on sociology." Apparently, it is much more difficult to settle nationalistic claims than sociological issues.

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Aleksander Gella

Polish Academy of Sciences, Warsaw

Reply

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Despite the force of Gella's remarks they do not serve as a basis for any alteration of my judgment that Gumplowicz was something more and something less than a distinctively Polish product. From a *socio-logical* point of view his mentors did not include a single Pole. We have his own outline of the history of sociology as evidence for this: Comte, Quetelet, Spencer, and in particular, Bastian, form the intellectual backdrop for Gumplowicz' sociological opinions. Nor as a matter of fact is there any *functional* reason for viewing him as distinctively Polish. Once he left Poland, he never returned—not even for a sociable visit. While it may be that he kept in touch with Polish affairs, he did so with the curiosity and concern of any exile. His correspondence with Poles is at the level of his concern with Spanish affairs or American affairs—to make sure that Spanish, English, and Polish editions of his work were properly translated and published. This hardly establishes a nationalist claim.

Gella is a trifle ingenuous in squaring Gumplowicz' sense of being a Pole with his sense of being a Jew. The implication that it was Gumplowicz' Polish loyalties rather than his Jewish faith and his radical politics which caused him grave difficulties in Polish university circles is simple conjecture. The Polish aristocratic tradition, coupled as it was with strong religious tests of allegiance, was quite sufficient to cause Gumplowicz to go into *voluntary* exile. It is fantasy to claim that it was his being a Pole which caused him to leave Poland—since it was the Polish intellectual climate which drove him out!

It is the irony of Gumplowicz' personal career that he should be both attached to Polish ambitions and become disattached from them. Doubtless, it is true that "one member of his family fought on the Polish side." I venture to say that if one digs hard enough, an ancestor can be found who fought on the Czarist side as well. But, given the fluidity of the Russo-Polish border throughout the nineteenth century and given the fact that Jews in particular were subject to terrorism and slaughter from *both* Poles and Russians and hence marginal to the claims on their loyalty of each, it strikes an aberrant note to find an Ashkenazi family being so vociferously claimed for Poland at this late date. It is a fact that in the struggles for Polish national liberation Gumplowicz and his family were, in the main, on the side of the Polish claims. But it is precisely the betrayal of the Polish national revolution, its conversion into one of the worst tyrannies of Central Europe, that helped make Gumplowicz a "cosmopolitan" and not a "local," a European and not a Pole.

Unless one prefers romanticized myths, there can be no other judgment than that Gumplowicz was "alienated"—as a Jew because of his Polishness, as a Pole because of his Jewishness, as an Austrian because of his concern with German scholarship, etc. To compound the alienation, he was ostracized as a professor of law for his profound appreciation of and belief in sociology. Franz Oppenheimer's point is not quite the same as that made by Gella. Gumplowicz was a man without a country: as a Jew, as an Austrian, and as a Pole as well. It was one of Gumplowicz' great contributions to sociology that the scientist alone is in the unique position to rise above petty claims of a petty nationalism. And his occasional references to his sociological system growing up on Polish soil are obviously in the nature of prefatory rhetoric to say nothing to substantiate any claims of nationalism. Indeed, they might more readily be read as Eric Heller does, as a piece of irony.

Now as for the points of personal biography. Gella is unclear on several points. My remarks on the cancer problem were in reference to Gumplowicz' wife as well as to himself. His wife developed an eye cataract, which prevented his travel abroad. And several years later, Gumplowicz too developed an eye cancer. After all, cancer of the tongue would not have prevented him from his favorite pastime—proofreading. He may also have had an extensive

cancer. But surely this is no way to score points.

Gella and I have different views of what a nervous breakdown is. I submit that a man who speaks of "being suddenly robbed of my composure" . . . "being put in a despairing mood," and who notes that the seriousness of the situation "has given me a strong jolt" eminently qualifies, by self-definition, for the title "nervous breakdown." No doubt, his suicide pact was made with the recognition of the medical hopelessness of his and his wife's situation. But to speak of suicide as a "quiet decision" under such extreme circumstances cannot be accepted as "true" (a word Gella employs whether he means conjecture or opinion, no less than fact).

The older son of Gumplowicz, who committed suicide, perhaps with less reason than the father, was indeed a historian. But a historian in the new (for the nineteenth century at least) tradition of the widest use of social statistics, rather than in the German tradition of *Kulturgeschichte*. The Webbs, the Hammonds, Rountree, *et al.*, were also historians. But their claim to fame is social statistics—as was the younger Gumplowicz', infused as he was with positivistic currents.

The only point at which I was probably in error is that the topic of his Cracow dissertation was on legal and political science. But it is nonetheless the case that for his research paper at Vienna, Pedro Montero indicates that he chose the philosophic theme of historical necessity in the Testaments.

I find it difficult to understand why Gella would say that Gumplowicz was "above all a historian and theoretician of law." First, because Gumplowicz himself often decried the narrowness and naïveté of his colleagues in the faculty of law. Second, because he insisted on being called a sociologist, and not a "theoretician of law." Third, because each of his major works has the word "sociology" somewhere in the title: *Der Rassenkampf: Soziologische Untersuchungen* (1883); *Grundriss der Soziologie* (1885); *Die soziologische Staatsidee* (1892); *Soziologie und Politik* (1892); and *Soziologische Essays* (1899). His personal contacts abroad, his pleas for wider dissemination of sociological periodicals, his insistence on being viewed as a sociologist —all of this makes it clear that Gumplowicz was "above all" a sociologist. The fourth and clinching point is Gumplowicz' sociological "imperialism": "Being the science of human society and social laws, sociology is obviously the basis of all the special social sciences treating parts of human society, or of particular manifestations of associated life." And Gumplowicz made clear that "the State is a social phenomenon consisting of social elements behaving according to social laws."

Now this is not to deny that a deeper appreciation on my part of the nuances of Gumplowicz' personal history would have netted me a deeper insight into his work. But my introduction does not rest on personal biography, but on sociological interpretation—and on this score Gella and I are in accord that I have not failed in my chores. Undoubtedly, a full-length book would have necessarily gone into these personal issues more thoroughly, and I share my critic's hope that this will be forthcoming. I must confess that the unique blend required for such a work, an expert knowledge of Polish, an interest in Polish intellectual history—fall outside the scope of my competence. On the other hand, if I were to await the coming of Gumplowicz' private Messiah, who was perfectly equipped, we would still be without an operational guide to Gumplowicz' not to mention a sound edition of his *Grundriss*. I would also like to add that I tried to compensate for this linguistic shortcoming by constant consultation on points of detail with a foremost Polish jurist (also in exile), Ignacy Aleksandrowicz. At the same time, a perfect appreciation of Polish history and language is scarcely enough to preserve my critic from the fundamental error of the genetic fallacy—from assuming that the derivation of Gumplowicz'

thought lies thoroughly outside the intellectual tradition and academic milieux which, after all, defines him as a social scientist.

I hope it is understood that I did not intend my Introduction as a slur against Gumplowicz' Polish background. Nonetheless, irrespective of whether we consider him "primarily" a pole, a Russian, a German, an Austrian in nationality, or a Jew, agnostic, or atheist in religious matters—or some strange blend of all, which is more likely the truth—I cannot for a moment doubt that Quetelet's *Physique sociale,* and Bastian's *Die Vorgeschichte der Ethnologie* meant considerable more to his development than all the personalia put together. Surely, my eminent critic would not deny the possibility that Gumplowicz' reaction against the optimism of the nineteenth century was just as much a consequence of the conservative character of sociological literature he knew as it was of his personal disenchantment with Poland, Austria, or Germany.

Claims and counterclaims of nationalism represent a sterile and specious form of scholarship—directing us, as they do, to take pride in ethnic and national backgrounds, rather than in the international communion of scholars. I am confident that Gella and I have the same attitude toward this larger issue.

IRVING LOUIS HOROWITZ

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Errata

The *Journal* wishes to report two errors in the article by Arnold S. Tannenbaum and Jerald G. Bachman, "Structural versus Individual Effects" in its May, 1964, issue: On page 586, second column, ninth line from the bottom, Z^2 should have been Z_2; in Figure 2, page 588, a vertical line should also have appeared down the middle of the graph (from top to bottom) as shown below:

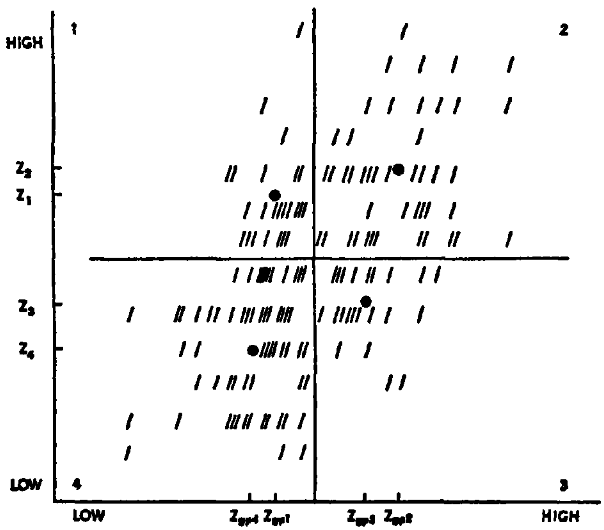


FIG. 2

BOOK REVIEWS

Social Change. By WILBERT E. MOORE. ("Foundations of Modern Sociology Series.") Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. 120. $3.00 (cloth); $1.00 (paper).

To examine any work by Wilbert E. Moore is to confront an honest intellectual product. And by honesty I mean a degree of self-awareness which at the same time is self-critical. Not many sociologists will admit that they have "been challenged particularly (and correctly)" on a fundamental point of social stratification. Fewer still recognize the handicap of studying social change after having "been long subject to the discipline of thought in sociology that discouraged the study of change." Having been reared myself in a tradition of "conflict sociology," it is equally difficult for me to appreciate the primacy of social structure and social order. This admission should make it clear that my differences with this text may well be due to contrasting intellectual upbringings than to disagreement with any of its specific facts or analytic frames.

One could treat this slim volume perfunctorily, as a useful pedagogic tool, well worth the attention of first-year students of sociology, and relax with a feeling of a job well done. However, the questions dealt with in *Social Change* are much too vital for such casual commentary. I propose therefore to deal with basic questions in search of an answer.

The first section on the "normality of change" contains an acute critique of functional equilibrium models, with their assumptions of perfect integration, absence of strains and inconsistencies through time, and their foreclosure on questions of the sources of change. But whether a view of social agencies as instruments of "tension management" does more than introduce change as a factor of structure remains unresolved. Insofar as Aristotle's naturalism is better than Spencer's functionalism, Moore's formulation has merit. But the "golden mean" implied in the settlement of questions of static-dynamic, order-and-change, chaos-predictability antinomies is itself a kind of long-range faith in the middle ground that has little evidential basis; indeed, none is suggested.

The second section raises critically "qualities of change," especially the "myth" of a singular theory of change or direction of change. Here again, Moore launches a powerful critique against monistic doctrines, such as those developed by Spengler, Sorokin, and the evolutionists. But whether Moore's "eclectic and tolerant" acceptance of *all* theories of change as potentially valuable in the study of different aspects of social systems can really escape the weaknesses of extreme historical relativism remains unclear. Moore better appreciates the weaknesses in holistic doctrines of change than equally serious shortcomings in atomistic theories. Indeed, he has a tendency to dismiss the possibility that the past *was in fact* better than the present simply because nostalgic distortion is embodied in the "noble savage" perspective. He dismisses a priori the possibility that complex multivariate analytic models may actually prove the superiority of one or another holistic model.

Various theories of change are not so much differentially warranted over space as they are over time. It might be possible to deal with change in terms of primary pivots: with the eighteenth century in terms of national struggles, nineteenth century in terms of class struggles, and twentieth century in terms of race struggles. It may well be that a singular theory of change can be so cast in dialectical terms that the reductionistic features most objectionable in any one theory of change can be removed. The contests between individualism and socialism, logos and mythos, reason and unreason, or what have you, may offer a general theory of change no less objectionable and more practicable than a general theory of action (which strangely enough rarely seems to evoke criticism for its holistic premises). But I would agree with Moore that it is only the beginning of wisdom to identify dichotomies and the pursuit of wisdom to observe that polar types do not concretely exist.

Moore's analysis of the specific factors involved in generational cycles, changes in formal organization, and intergroup conflict in

preclusive groups is a model of good sense and sensible goods. Particularly noteworthy is Moore's appreciation of the fact that the ascription-achievement axis is not one of mutually exclusive alternatives, but conflicting principles present in every social system. This is particularly important in the study of the developmental process, given the inundation of psychologistic theories of achievement which simply rule out the importance of ascription in even the most developed societies. However, I would suggest that the struggle is not simply between partisans of tradition and upholders of rational innovation. This assumes too neat a world, one in which "irrational" processes are simply ruled out on the grounds that they entail unstructured or spontaneous behavior. Yet, is it not the case that collective dynamics is the ground of change, structured or otherwise, neatly packaged or clumsily lumped together into temporary alliances? In this sense, Moore's lack of consideration of the work of Blumer, Lang and Lang, Smelser, and others, is a serious defect.

Moore's study of changes in societies raises extremely important questions of the functional autonomy of certain features within the social system. His discussion of how this partial autonomy works out in relation to art, science, and technology offers very suggestive lines for further study. Strangely enough, given Moore's rejection of Marxist formulations, his description of autonomous change squares very well with the independent role assigned to the superstructural elements by men like Lukacs, Hauser, and Ossowski.

The typology of revolutions offered could have been considerably beefed up, especially the analysis of types of revolutions. Moore is overwhelmed by an extreme rationalism in his denial of the role of ideals and utopias in the actual conduct of social change. It would seem that he could have paid a good deal more attention to explaining how revolutionary changes and demands for the amelioration of inequality are responses not simply to material exigencies but also to a vision of a more perfect future. It may sound trite, but Moore sometimes sounds as if he actually believes that men live by bread alone.

That his basic chapter on the developmental process is called "modernization" is itself highly revealing, since, as a matter of fact, the basic choice in the newly emerging nations is precisely one between modernism and structural innovation. The process of modernization is *not* synonymous with economic development. Modernization is related to a special form of economic change which emphasizes bureaucratic innovation and a host of mending processes such as education and juridical reform. In contrast to this, the structuralist school of development basically holds that the process of development requires smashing even more than mending; that is, requires an overhaul in social relations no less than in the level of industrial productivity as such. Moore's failure to grapple seriously with this problem leads him to describe as "radically inappropriate" structuralist options to modernization. On the other hand, since he recognizes that continuous growth involves the direct intervention of the state at those points where voluntary choice breaks down, he is hard put to explain how developing regions can respond to the necessity of continuous growth short of calling upon mechanisms of persuasion and coercion such as are lodged in the state. Moore tends to speak of modernization as an autonomous social process, and hence he avoids the political problems of development occasioned by the role of planned processes.

That Moore concludes his book with an examination of the doctrine of "social evolution" is something like beating a dead horse. His generous sentiments toward the Spencerian philosophy of history do not address the point. And the point is the process of political revolution coincident with social development. What is the relation between social revolution and human survival? What is the distinction between mechanisms of concensus, command, and terrorism in the promotion and planning of change? What leads a specific person to adventure and change in a community setting which discourages worldly participation? What is the connection of state planning to corporate conduct? Perhaps it is unfair to burden an admittedly basic text with such profundities. Nonetheless, if pages can be spent on how to cultivate an optimistic reading of man's past and future, it seems not unwarranted to insist upon some study of the tougher problems that make historical optimism something less than sociological realism. As I mentioned at the outset, this is a quarrel more with Moore's "style" than with his "performance." To the publisher and editor who commissioned this work, it is only fair to say that they chose well. This volume will eminently serve its pur-

pose of introducing students to the problems of social change.

IRVING LOUIS HOROWITZ

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Conflict and Conformity: A Probability Model and Its Applications. By BERNARD P. COHEN. Cambridge, Mass.: Massachusetts Institute of Technology Press, 1963. Pp. xii+186. $5.00.

In this monograph Professor Cohen presents his work on a mathematical model of the process of conformity in the Asch experiment. He presents data from several series of subjects run in a "purer" version of the Asch study and with a greater number of trials. The original experiment is simplified by having the same stimulus and comparison cards each time, by having neutral trials only on the first two trials, and by having the confederates always respond with the moderate or the extreme error in a given session.

Cohen discusses much of his preliminary work in the form of a series of models more primitive than his own. The development of this series provides the reader with a clear view of the work of building a mathematical model. Few modelers have indicated so explicitly the successive steps in their work.

Markov chain theory is the mathematical basis for Cohen's model. The mathematics of this theory is presented lucidly. The author has the rare talent of writing a mathematical argument so that the mathematical reader grasps it quickly. He has also segregated the mathematical presentation from its interpretation, so that a person with no mathematical background can readily comprehend the approach.

The use of computer and Monte Carlo methods to solve mathematical problems is instructive. These techniques alone make the monograph worthwhile because they show how to produce approximate solutions to difficult mathematical problems, problems that must be solved before the modeler can proceed.

Substantively, Cohen presents some findings that are new. For example, he finds that for both moderate error and extreme error conditions, the percentage of correct responses per trial is lowest on the first experimental trials and increases slowly and apparently asymptotically to about 74 per cent in the moderate error condition (eighty trials) and to about 85 per cent in the extreme error condition (thirty-six trials).

A number of interesting issues are raised.

1. An assumption of the relation between mathematics and social reality is stated by Cohen as follows: "It is important to stress the fact that the symbolic language is a basic tool which adds nothing to the content of the model. If translating our notions into symbols added content to these ideas, it would defeat the purpose of the translation" (p. 3). To speak this way implies that both the symbols and the ideas are clearly known in advance, so that it is meaningful to talk about the addition of content. Initially, however, neither the theorist's ideas nor the consequences of the symbolic language are all that clear. Over time one clarifies the other. Occasionally, the model and many of its consequences may be clearly known in advance. Then the clarity of the model may force the theorist to select ideas that conform to it from his pool of largely implicit ideas about the problem. While I cannot say that content is added by the symbols. I believe that the theorist's ideas are coerced, made definite, shaped by the symbolic language.

2. Both Asch and Cohen had to make the experimental situation believable to subjects. Thus, two experiments were going on simultaneously: (*a*) a credibility experiment to present the guise of a psychological experiment in perception and to avoid making the subject suspicious of the confederates; and (*b*) the conformity experiment. To help create a credible experiment Asch found it necessary to vary factors not considered relevant to the production of conformity. Cohen strips off most of Asch's irrelevant variation and still succeeds in making the experiment believable to the subject. I found this one of the most interesting results in Cohen's work.

Simplifying an experiment while retaining its essential character depends on the experimenter's intuition as to what constitutes irrelevant variation. The present case is intriguing because one of the two factors that Cohen thought irrelevant and kept in his experiment turned out to be relevant. In the eighty-trial moderate error condition the percentage of correct response per trial varied greatly, depending on whether the stimulus card was presented to the right or left of the compari-

son card. The relative position of the cards seemed to have caused the effect, for when another group of subjects was run without switching cards, the effect disappeared. Several points are suggested:

a) What is irrelevant in the original Asch experiment need not be irrelevant in a simplified version of the experiment. Findings like the present one suggest that the new experiment is qualitatively different from the original experiment, so that previously learned guides to intuition need no longer apply.

b) The method by which the theorist decides relevance and irrelevance is typically unstudied. In fact, the method for creating believable situations is generally unstudied. Presumably, the unambiguous stimulus, the unanimous majority, and the public response—the core of the phenomenon, in Cohen's terms—are always relevant variables. But in creating a workable experiment Cohen had to handle many other variables, any of which could be relevant. The theorist's intuition about relevance can easily fail because he cannot completely anticipate a new experimental situation.

3. Several points become explicit because of the unexpected finding discussed above.

a) One of the best features of a mathematical model, its lead-producing capacity, is shown here. Use of the model produced a recognized surprise that served as a lead for further theorizing.

b) Upon rereading the results of his 36-trial moderate error condition, Cohen finds the same surprise, but there the finding is not statistically significant. He rereads the previous non-significant finding as "significant," now in a substantive sense. We have here an excellent example of the fact that by itself statistical significance means nothing substantively.

c) Though Cohen suggests how the unexpected finding might be incorporated in his model, he still chooses to regard it as irrelevant for a theory of conformity in the Asch situation. Cohen regards the surprise primarily as a failure in experimental technique, rather than as a new insight for theoretical speculation.

4. A very large number of statistics can be calculated from Cohen's data. If the model is complete, there is an expected value derivable from the model for every possible statistic. If the model is a perfect description of the situation, then it makes no difference which expected values are deduced; all the corresponding statistics will fit within sampling error. But perfect description is hardly likely to be the case. The fact that the modeler has derived some expected values and not others may well influence his thinking about the nature of the substantive problem. The modeler proceeds blindly, deriving expected values, checking them for fit, perhaps changing the model to account for bad fit, deriving more expected values, etc. Because the conceptualization of the problem is not fixed at any point, this procedure may lead to different conceptualizations about the phenomenon, depending on the exact sequence of expected values derived. If so, there should be theoretical criteria that direct the selection of the path through the possible expected values.

In summary, this monograph is instructive for the reader who is interested in the practical work of developing and testing a mathematical model. Even more importantly the monograph is instructive for the reader interested in ways of theorizing in sociology because of its careful detailing of procedure.

LINDSEY CHURCHILL

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Max Weber, edited by S. M. MILLER, pp. 88; *Thorstein Veblen*, edited by BERNARD ROSENBERG, pp. 100; *William Graham Sumner*, edited by MAURICE R. DAVIE, pp. 92; *Emile Durkheim*, edited by GEORGE SIMPSON, pp. 129; *Lester F. Ward*, edited by ISRAEL GERVER, pp. 91. ("Major Contributions to Social Science Series," edited by ALFRED MCCLUNG LEE.) New York: Thomas Y. Crowell Co., 1963. $1.75 each.

The Sociology of Luigi Sturzo. By NICHOLAS S. TIMASHEFF. Baltimore: Beacon Press, 1962. Pp. 247. $5.95.

The series of little volumes on Durkheim, Sumner, Veblen, Ward, and Weber, consisting of a biography, selections, and commentaries on the work of each, forms up-to-date elementary introductions designed to be "interesting to college sophomores and especially to undergraduate majors in the various social sciences" and to serve as supplementary reading materials "intended to enrich introductory courses

in the various social sciences." They will be welcome to teachers who have been forced to choose between assigning entire works of the classical thinkers or books of selections which at best give inadequate pictures of the classical theorists. It is to be hoped that this series will eventually be expanded to include the sixty or seventy additional theorists also worth parallel treatment.

Although Nicholas Timasheff's *The Sociology of Luigi Sturzo* is also destined primarily to serve as supplementary reading for sociology courses (though in this case for more advanced courses), it belongs to quite a different category. It is not a book of selected readings but a monograph on a thinker; it is not about a classical sociologist but about a contemporary; it does not illustrate one of the established theories but a recent movement in sociological theory.

Luigi Sturzo (1871–1959), "the greatest among contemporary Catholic sociologists," was born in Caltagirone, Sicily, and trained and ordained for the priesthood. He taught philosophy and sociology in the seminary in his native town, entered its politics and founded a newspaper, participated in the political and economic life of his times, founded the Populist Party and was driven into exile in England and the United States by the Fascist government. He returned to Italy in 1946.

Sturzo belongs to a long line of thinkers who have sought to reconcile Western intellectual trends with the requirements of Roman Catholic theology. His philosophic suppositions drawn from Thomist philosophy (p. 219) led Sturzo to visualize sociology as "the study of the laws of social structure in the light of the contribution of the supernatural with its transforming influx" (p. 37). His resultant sociology is conceived by Timasheff as most directly comparable to that of the structural-functional school (p. 39). However, a number of differences from that school flow from Sturzo's conception of the social system as a supernatural as well as an empirical product.

In accord with his Thomist philosophy Sturzo believes that the supernatural is knowable only through revelation and not through empirical methods and reason (p. 38). The full explanation of the supernatural-empirical social system, hence, cannot be provided by empirical methods alone. "Sturzo . . . expands sociology into an all-embracing theory of society, covering what is empirically knowable and what can be predicated of society philosophically and theologically. In this way he disrupts a universe of discourse about society on the empirical level, until the day when (if) all men acknowledge the truth of Christian revelation" (p. 56).

Timasheff's study of Sturzo is itself a monograph in the integration of a Neo-Thomist sociological theory and will serve primarily as supplementary reading material for persons interested in the adaptation of sociology to the requirements of Roman Catholic theology.

DON MARTINDALE

University of Minnesota

Pastors and Immigrants: The Role of a Religious Elite in the Absorption of Norwegian Immigrants. By NICHOLAS TAVUCHIS. The Hague: Martinus Nijhoff, 1963. Pp. 84.

It is easy to agree with William J. Goode to the effect that this little book is a fascinating story. Mr. Tavuchis' analysis of the conflicts among Norwegian Lutheran immigrants to the Midwest comes close to making an important contribution to the sociohistorical analysis both of American religion and of the assimilation process itself.

The history of Norwegian Lutheranism in America is an excellent choice for a sociohistorical case study, since the internal controversies which the author documents were reactions to general American patterns that confronted most immigrant churches. By looking at the reactions of this particular group the author is able to study social processes which took place within a relatively small, geographically compact religious group. Furthermore, unlike the reactions of other ethnic churches, the responses of the Norwegian Lutheran groups to their new environment were straightforward and sociologically manageable. The strains of culture contact and accommodation produced internal conflict which polarized along social structural lines: the clergy, with the moral support of the Missouri Lutheran Synod, were pitted against the Norwegian American laity, who found some support among American groups.

Mr. Tavuchis has studied these conflicts within the theoretical scheme devised by Ei-

senstadt for the analysis of elite-follower reactions in the assimilation process. The controversies reported were seen as both causes and consequences of assimilation: the laity were characterized as accepting more "American" positions, which brought them into conflict with the clergy whose views were less in tune with the new milieu. The clergy supported the use of foreign-language Lutheran parochial schools in lieu of the American common schools. The second controversy emerged because the clergy felt that slavery could not be condemned as a sin *in itself* since it was known and not condemned in Biblical times.

In spite of the self-conscious preoccupation with theory, the uninspired analysis of these data partially obscures their genuine sociological significance. Though obscured, these data remain striking and fascinating. For the fact is that the conflicts that the author documents so well, and those he slights, such as the controversies over church polity and the extension of sacerdotal roles to the laity, are consequences of a general confrontation between the traditional values held by many of the immigrant churches and characteristic American value orientations.

There seem to be two major American influences on the immigrant churches and their members. The first of these is the antihierarchal nature of the American religious experience. The second is the toleration of all religions, a concomitant of the constitutional separation of church and state.

Both the lasting and short-term effects of these patterns on the immigrant churches are yet to be untangled and specified. At first both of these orientations were seen as threats by most traditional religious elites. The Norwegian Lutheran pastors were no exception. It was precisely the fear of "indifferentism," which they saw as the necessary consequence of religious toleration, that drove them to support foreign-language parochial schools. Furthermore, the anticlerical and antiritualist groups among the Norwegian immigrants were strengthened by the antihierarchal feelings on the frontier. While the author does not focus on the shifts in church polity, it is clear from his work and from his sources that the pastor-lay relationship underwent radical if temporary changes in the United States. It would have been of inestimable value for him to specify the temporary and lasting changes in the role of the clergy. He might even have speculated on the reasons for the precise accommodation observed between old church and new American patterns.

There is no doubting the impact that these American value patterns had on various immigrant churches and their members. American Catholics, for instance, first discouraged Rome from naming a bishop to the American church. Later, they successfully petitioned Rome to have the first Catholic bishop in the United States *elected* by the American clergy. The trustee battles in that church were at least in part a function of the antihierarchal nature of the American religious experience. Nor is it possible to argue that these shifts were all short-lived accommodations to American life, doomed to extinction when the immigrant churches grew strong: a symbol of the assimilation process is the fact that the American Catholic hierarchy, whose predecessors were as concerned with "indifferentism" as were the Norwegian Lutheran pastors, have now insisted in Rome that the Catholic church take a stand on the religious liberty of all men.

Admittedly, the situation which the author sets out to analyze is not a simple one. Lay-clerical conflicts were not new to the Norwegians when they arrived on American shores. These conflicts, nevertheless, took on a new cast, a typically American one, in the unique situation the Norwegians faced on the frontier. The author would have made an invaluable contribution to both the sociology of religion and the theory of assimilation if he had analyzed these conflicts within this broader frame of reference. This is so because such a case study could then be used, along with others, to analyze the process by which church polity and teaching were and are being accommodated to the broad cultural patterns that the immigrant churches have had to confront in America.

The author has sought to extend the theoretical formulations of Eisenstadt. Little new emerges from this forced effort, which remains logical rather than sociological. The application of reference-group theory, though justified by the historical facts of the situation, remains sociologically barren. The author seems totally uninterested in the obvious question: why were the Lutherans of the Missouri Synod (like the Polish and German Catholics) successful in founding foreign-language parochial schools while the Norwegian Lutherans were not? Other relevant

questions remain unraised and unanswered. One has the distinct feeling while reading this book of viewing a fascinating film from a front-row seat that is off to one side of the screen. This remains an interesting story that is somewhat obscured and distorted.

LEONARD J. PINTO

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The Sociology of a Cheviot Parish. By JAMES LITTLEJOHN. New York: Humanities Press, 1963. Pp. 164. $5.00.

In recent years British social anthropologists, unquestioned masters of the study of African culture, have turned to the rural parts of their own country as new sources of information on the change from peasant to modern society. While the results may not always be significant contributions to the advance of social theory, the studies are without fail fascinating accounts of the ways human beings live in society.

Littlejohn's study of Westrigg, a subdivision of a Border county in Scotland, is no exception. There are two major points in the volume. First of all, the invasion of Westrigg by the outside world in the form of modern communications and transportation has led to a heavy migration from the area and thus a depopulation of the parish. Second, the organization of both the farmers and their hired hands into unions has led to an impersonalization of the social relationships on the job. While this may take some of the human warmth out of the social structure of the parish, there is no doubt that from the workers' viewpoint the relationship between worker and boss is much more satisfactory than that between servant and master.

An interesting result of the depopulation and the nature of the work of the large sheep farms (fourteen in the whole parish) is that there is little in the way of kin relationships within the parish—at least as compared to other British Isles rural communities on which reports have been published. This finding, which calls into question the traditional thinking about kin relationships in rural communes, is the most important contribution of the volume.

It is unfortunate that the author does not investigate more thoroughly the decline in influence of the Church of Scotland. He reports that at the turn of the century no one would have dared to miss Kirk on the Sabbath, but now the average church attendance is twelve (out of a possible 320). To anyone familiar with the importance of the Kirk in the history of post-Reformation Scotland, this is an astonishing event, the dynamics of which are well worth investigating. Indeed one would very much like to see a comparison of the decline of the Church of Scotland with the apparent survival of Roman Catholicism in Ireland. One wonders if the Scottish church might have been wiser to align itself with a nationalist movement.

ANDREW M. GREELEY

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Das Aufstiegsproblem in der modernen Gesellschaft. By FRIEDRICH FÜRSTENBERG. Stuttgart: Enke Verlag. 1962. Pp. 175.

This book presents a conceptual frame of reference and theory of upward social mobility, its nature, determinants, and effects. Research results are taken into account, but no quantitative data are given or analyzed. To illustrate the usefulness of the analytical scheme, one chapter is devoted to its application to mobility in large industrial firms. A historical dimension is introduced by a summary sketch of the ideal and real factors which contributed toward the development of a general mobility aspiration, and by projecting the analysis throughout onto the background of modern Western society.

The basic dimension defining upward mobility is said to be the increase or extension of functions, that is, an increase of the action sphere and the sphere of control of an individual. The mobility process is seen as the resultant of four basic factors. The first is subjective aspiration; a typology of motivational orientations is offered. The second factor is found in the objective criteria of selection. Again a typology is suggested, based on Max Weber's four types of orientation. The process depends further on the sector of mobility chosen (e.g., economics, politics, ad-

ministration, mass media, etc.), and on the mobility road (e.g., career, free change of positions, or marriage). All other factors which influence the mobility process are held to do so through one of these four basic determinants. Among these additional factors the author discusses innate capacities, social background, stability of the social structure, differential fertility, and economic growth processes. The consequences of mobility on the "microscopic" level are analyzed in terms of effects on the relative power and secondary rewards of positions, and on the upward mobile's behavior. On the "macroscopic" level, the effects are considered as functions or dysfunctions for social stability and adjustment. A final chapter treats ideological tendencies resulting from the discrepancy between generalized mobility aspirations and limited mobility chances, and between pretended and actual criteria of selection. A number of mobility substitutes are pointed out, such as creation of vacancies by establishing pseudo-hierarchies, or deflection of mobility aspirations to the area of consumption. Part of this chapter's argument rests on the acceptance of the definition of upward mobility as increase of functions and of all other rewards as secondary—a point which might remain open to debate.

RENATE MAYNTZ

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The Quest for the Dream: The Development of Civil Rights and Human Relations in Modern America. By JOHN P. ROCHE. New York: Macmillan Co., 1963. Pp. xii+308. $5.95.

Professor Roche has produced a fresh and exciting view of American liberties in the twentieth century. For Roche "the last fifty years have seen the most meaningful development, the greatest breakthroughs, in human freedom, in our history" (p. 3). With evident relish he demolishes the myth of a golden age of freedom in the American past; instead he sees a society in which the pressure to conform was tempered only by the fact that there were a variety of local conformities to choose among. Taking the early years of the century as his starting point, Roche recounts the abysmal history of repression and terror during World War I, the Red scare, the know-nothingness of the twenties, the brutal treatment of the Negro, and the virulent discrimination against minorities. He sees nothing but progress from the days when civil liberties were unredeemed claims by the dissident, the marginal, and the high-minded to the present when they are judicially protected, implemented by the federal, and some local, governments, accepted by increasingly wider sections of the population, and used with effect by more and more minority and dissenting groups.

Roche is concerned less with the development of legal doctrine than with the social and political conditions that make rights accepted and usable. He finds conducive to liberty the growth of big (especially federal) government, the bureaucratization of political and legal institutions, increasing urbanization and the impersonalization of city life and even "the collapse of that sense of community so highly esteemed by nostalgic sociologists" (p. 232). This urban bureaucratic society has developed "a tradition of due process and equality which it never had before. Not equality and due process as rhetoric, but as meaningful political, social and legal procedure" (p. 261). Indeed, "in 1963 there is more respect for freedom and dedication to equality in the United States than we have ever known before, and . . . every indicator points toward future elaboration" (p. 267). Our very awareness of shortcomings and deficiencies in our liberties does not reflect a decline but instead a heightened sensitivity and concern.

In the first half of the century "a new set of values built around freedom and equality in a multicultural, multiracial urban society . . . emerge[d] to provide a focus for American aspiration" (p. 25). However, it turns out that this "new set of values" is not new after all. It is but the fulfilment of "the American Dream." This dream is often invoked but never defined and, as befits a dream, it it both luminous and indistinct. Its substance is Christian and democratic (p. 142); it implies a self-restrained majority which respects minority rights and due process (p. 78). In any event, the dream had never been fulfilled before, but even while "degraded in practice, it retained its abstract purity as the inspiration of national purpose"

(p. 100). In the course of the last fifty years, the dream has taken on flesh: "[t]he majority . . . accepted the full implications of its own democratic principles and gave concrete meaning to the abstractions of the American Dream" (p. 262).

Lending the dream a certain nightmare quality are the Yahoos—bigots, nativists, and know-nothings—against whom the votaries of the dream must continually do battle. Yahoos, like many mythological monsters, are many-lived. The miserable showing of Lemke in 1936 "was the end of the political road for the Yahoos" (p. 172). Yet only a few years later (1939–41) "the destruction of the power of the Yahoos in the United States" was "[i]n the fundamental sense . . . a consequence of their becoming identified in the public mind with Nazism and anti-Semitism" (p. 177). In 1940 Yahoo power was "discredited and demolished . . . institutionally speaking their back was broken; civil liberties and minority rights had become part of American public policy" (p. 183). Yet, of the end of the McCarthy era Roche can only say with relief that "[t]he Yahoos had been contained" (p. 233). While admirably sensitive to the ways in which libertarian ideals have been given institutional form in our urban bureaucratic society, Roche seems to discount or ignore the fact that substantial parts of the Yahoo platform have also been institutionalized as public policy—for example, in our immigration laws, loyalty oaths, security procedures, restrictions on travel, anti-sedition laws, Attorney General's list. And his focus on the most virulent and bigoted of anti-libertarians obscures the impediments to freedom which proceed both from more sober and reasonable sources and from the very scale and structure of our complex industrial society.

This indifference to the ambiguities and ironies of the development of freedom in contemporary America seems to derive in part at least from his organizing metaphor—the dream—which supplies a picture of the realization and fulfilment of some pre-existing ideal. Curiously, the dream model reincarnates in new guise the argument from original intent—an argument that Roche has little use for—for it turns out that what we have achieved is what we have been dreaming all along. And although this achievement is pictured as the result of struggle and sacrifice as well as of changing conditions, it sometimes (despite disclaimers) has a Hegelian overtone of the unfolding of some latent and indwelling essence.

Roche rejects the fashionable apologia which "treats the growth of the modern United States as the unfolding of 'consensus'" (p. 106). But while he recounts the violence and conflict in recent American history, his dream projects the consensus and harmony from the surface into the depths and from the past into the future. This fiction of subliminal unanimity may explain the lack of any concern here with possibilities of conflict between claims for liberty. Although Roche perceives that freedom does not reside in abstract rights but in social procedures and opportunities, he seems to imply that all claims for freedom and equality are compatible and that social arrangements can be thought of as adding to or subtracting from the sum total of these values. While this may suit the sort of gross abuses he focuses on here, it is not so convincing when applied to what a recent writer calls "the problems beyond bigotry" which may involve choices among competing freedoms. Is there a single "American Dream" or a set of competing, though overlapping, dreams? Is the problem to realize some vision which we all share, however inchoately, or to find methods of working out accommodations among divergent and perhaps incompatible visions of the future?

Roche has packed a great many things into this book. In spite of asides, digressions, startling omissions, a brevity which often distorts, and a sweeping (and occasionally infuriating) rhetorical style, this is an important and interesting book. It is very much a brief and Roche may win his case, but one suspects that the final judgment will not concede every point to the victorious counsel.

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Judicial Decision Making. Edited by GLENDON SCHUBERT. New York: Free Press of Glencoe, 1963. Pp. ix+278.

Judicial Decision Making is the fourth volume of the *International Yearbook of Political Behavior Research*, a fact which gives a fair

indication of the book's contents. In the last five years the "behavioral" wing of political science has discovered the courts. Inspired by C. Herman Pritchett and led by Glendon Schubert, a number of American political scientists have tried to apply the statistical, psychometric, and sociometric techniques of contemporary behavioral science to the study of judicial behavior. Many of the prominent figures in this movement are represented among the authors of the nine articles in the book, including Schubert, Fred Kort, Sidney Ulmer, Harold Spaeth, and Joseph Tanenhaus.

The contents of the articles are varied: The authors consider leadership in the Michigan Supreme Court, the social and political attitudes of state and federal judges, the role of judicial attitudes and *stare decisis* in decisions regarding military jurisdiction over civilians, the cues used by Supreme Court justices in choosing cases of *certiorari* jurisdiction, the mathematical formulation of rules of law, and the role of the judiciary in American state politics. Norwegian social scientists contributed two articles, one by Vilhelm Aubert on the differential treatment of conscientious objectors in Norwegian military courts and one by Ulf Torgersen on the changing political role of the Norwegian Supreme Court.

A brief review does not permit a complete catalogue of the displays of statistical technique or the methodological sins included in the essays. Suffice it to say that some of the work evidences monumental scholarship and brilliant application of quantitative technique. Inevitably, the pieces are subject to methodological criticism. Quantitative technique involves the systematic abstraction of a few analytical features from the rich complexity of concrete experience. It would be surprising indeed if a reviewer could not find some instances of measures or manipulations which do not seem to illuminate the legal process. Nevertheless, one must congratulate the authors for their careful and rigorous contributions to the scientific study of law.

Unfortunately, one weakness is so prevalent as to require special comment. If there is a fundamental theme that unites a large portion of the analysis, it is the idea that judicial decisions are not products of the mechanical application of legal rules. Judicial decisions are said to reflect underlying social and political attitudes. Whatever the merits of the critique of conceptual jurisprudence, one wonders about the adequacy of attitude psychology as a framework for understanding the judicial process. Apart from Aubert's stimulating analysis of the impact of the organizational context of judicial recruitment, there is little systematic study of why persons with certain types of attitudes come to be judges and no analysis of the constraints arising from the judges' participation in an ongoing social system. Symptomatically, the one article on the role of judicial action in the American political arena is programmatic and is the only article that does not report a quantitative empirical study.

Sociologists need an additional caveat. We must avoid equating judicial decision-making and the legal process. Published judicial decisions constitute a minute and extremely biased sample of the universe of legal action. In fact the analysis of judicial decisions in abstraction from their organizational contexts and the larger universe of legally oriented action is the methodological device used to force the legal process into the categories of attitude psychology.

LEON MAYHEW

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Legislative Partisanship: The Deviant Case of California. By WILLIAM BUCHANAN. Berkeley: University of California Press, 1963. Pp. 167. $3.50.

The author has chosen to analyze the California legislature because he feels that it presents an unusual opportunity to the political scientist in that "the variability of partisanship makes California useful for understanding other legislative systems" (p. 5). In particular, he examines the effects that this variability has had upon the structuring of leadership roles within the legislature, the creation of functional substitutes for parties in organizing the legislature during periods of non-partisanship, and the kinds of laws produced by different forms of internal organization.

Moreover, Buchanan says that he intends "to trace the origins of these fluctuations [in partisanship] back into the constitutional, economic, and societal characteristics of the state" (p. 3). His study does suffer somewhat in that he treats fully only the first but not the latter two avenues of causal explanation. And yet there is a certain virtue in this, for his primary focus is always political—the constitu-

tion and electoral laws of the state, the rules governing the operation of the legislature, and the impact of different styles and goals of political leadership are the parameters which he sets for his analysis. Too often sociologists have failed to seek distinctly political causes for political events. On the other hand, if Buchanan's analysis is incomplete, it is because he has not gone beyond the political system systematically and in enough detail. Yet to the extent that his study of California politics is adequate, he will have reminded those of us who call ourselves political sociologists that we are above all students of political things.

The author traces California's "peculiar system of politics" to the reforming administration of Governor Hiram Johnson, which began in 1911. Various laws explicitly aimed at weakening political parties were enacted and a nonpartisan tradition was widely diffused among the people of the state, by the governor and his regime. The impact of national politics during the 1930's brought a partial return to partisanship in the California legislature. But this was snuffed out substantially by the nonpartisan Warren-Knight regime. There was a period of transition during which changes were made in California electoral laws and partisans not accustomed to California's "peculiar system" migrated to the state. Finally, the election of Governor Pat Brown in 1958 brought a return to partisan government.

Within this fluctuating context Buchanan goes about his analysis. The role of the Assembly speaker, the creation and maintenance of legislative factions, the operation and strength of lobbies, and the fate of the legislative programs of each governor are examined in considerable detail. He explores the potential for lobbies and factions to act as functional substitutes for parties in the political system.

Two chapters are devoted to the analysis of roll-call votes in the delineation of factional coalitions within the Assembly. In chapter v his adaptation of sociometry to roll-call analysis is novel and useful. Fourteen votes are selected for a nine-year period to demonstrate the development of two major factions. Roll calls relating to the election of the speaker and the organization of the Assembly are included. But the author also includes a number of votes on other matters without explaining why he has chosen them. (He does note that they are "major roll calls.") The criteria of inclusion are not clear.

In chapter vii the 1957 session of the Assembly is analyzed by the use of the cluster-bloc techniques developed by Stuart Rice and Herman Beyle and used on the United States Congress by David Truman. Partisan, factional, and regional alignments are shown to account for about 25 per cent of the roll calls taken in the session. The author is unable to explain most of the remaining vote distributions. For that matter, he points out that the members of the Assembly cannot explain them either. He therefore concludes that these Assembly decisions "are unrelated to one another; coherent principles and programs are hard to delineate" (p. 121). "We must conclude that the California legislative process is genuinely mystifying" (p. 120).

That may well be, but I believe that before coming to this conclusion the author should have explored at least two additional avenues of research. In the first place, he has not analyzed adequately the diverse social, economic, and political subsystems of the individual constituencies represented in the Assembly. Diversities in these structural bases of representation may be at least partially responsible for the legislative chaos in Sacramento.

Second, he needs to evaluate and classify those issues which cannot be accounted for in terms of partisanship, factionalism, or regionalism (in which he includes urbanism). The author does point out that there are many "small concerns" which all state legislatures must handle. "Left by the federal and state constitutions with police and residual powers, legislatures are the agencies charged with making whatever decisions are left over—decisions no other branch or level cares to undertake" (p. 145). The Assembly's seeming "chaotic" handling of these issues may be the result of weak party organization, or an unusual structuring of constituency characteristics, or both. But until the nature of the constituencies and the content of these secondary issues are examined and compared in detail to those of other more partisan states, we will have to call this feature of California political life "genuinely mystifying" indeed.

These remarks must not detract from the value of the core of the book, which deals with the legislative system as such and the more important issues and problems which are explicable in political terms. Moreover, it offers the proper political framework within

which supplementary sociological research should and can be profitably carried out.

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The Negro Leadership Class. By DANIEL C. THOMPSON. Englewood Cliffs, N.J.: Prentice-Hall, 1963. Pp. x+174. $1.95.

"If New Orleans, like Atlanta, has no Negroes in the power structure, then how do they get things done?" This is the question posed by Daniel Thompson on the first page of his study of Negro leadership. The answer, supported by data carefully gathered in New Orleans over a four-year period, is that—until quite recently—Negroes have gotten things done by playing the political game according to the white man's rules.

Thompson indicates that between 1940 and 1960 the leadership patterns (and the leaders too) changed considerably. The traditional host-parasitic relationship between white segregationists and Negro "Uncle Toms" has been steadily disintegrating since World War II. There has been a corresponding breakdown in the functional ties between moderate whites and *class*-oriented "racial diplomats," the gentle people of prejudice and their Negro counterparts who favor good community relations and compromise (and who frequently compromise themselves into retaining traditional status arrangements, accepting token measures to assuage their *own* feelings of having done something to further interracial harmony). Already under strain, the tenuous relations between these parties were loosened following the Brown decision of the Supreme Court. They ruptured with the confrontation over segregation in local schools in 1960. Negroes had become impatient with the maneuvers of the diplomats; moderate whites had become embittered by what they saw as a sellout of the Negroes to radical elements.

Finally, with heightened tensions, even the working relationship between the few brave liberals and the growing number of *mass*-oriented Negro leaders was threatened. Many white liberals retreated from the public arena in the face of increasing intimidation from segregationists. By 1960 the Negroes of New Orleans faced a turning point in their long history. Favors and tokens were no longer satisfactory; to win the fight for desegregation they had to mobilize—and they did. The school crisis brought about the first effective coalition of Negro leadership and, simultaneously, gave impetus to a ground swell of activity within the Negro community.

The book ends at this point of a new beginning. In many ways, *The Negro Leadership Class* sets the stage for a drama that was to unfold not only in New Orleans but throughout the nation.

Thompson's story of changing patterns of Negro leadership is told "sociologically," beginning with a description of the situation, the methods employed in gathering the facts, the social and occupational characteristics of New Orleans intra- and interracial leaders, and—in the words of the author and his respondents—a recounting of the political, economic, and educational injustices suffered by Negroes and their attempts to come to terms with them.

The story is well told, and the book is an important addition to the growing list of race relations monographs. It offers new insights into the old problem of minority leadership, an inside view of one city's most pressing social issue, and—when discussing the varied results of interviewing the *same* subsample of "top" white and Negro leaders by a biracial staff—an important sidelight on research in race relations.

It could have been even more. Early in the book the author offers some excellent observations regarding the phenomena of leadership and social change. Would that he had returned to these later on in light of his data. Then, perhaps, *The Negro Leadership Class* might have been another *Community Power Structure,* a book after which Thompson's study was modeled.

PETER I. ROSE

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The Decision-Makers: The Power Structure of Dallas. By CAROL ESTES THOMETZ. Dallas: Southern Methodist University Press, 1963. Pp. xiii+141. $4.00.

This is a study of the power structure of Dallas, Texas, based squarely on the so-called reputational method of investigation. Armed with a list of sixty-seven persons named as community leaders by a panel of eight knowledgeable citizens, Mrs. Thometz set forth to

dentify the decision-makers through interviews with the reputed leaders themselves. Seventeen respondents were asked to describe the decisions in which they had been involved personally during the previous two years (the research was undertaken in 1961) and to estimate whether their involvement was typical of the way most important decisions were made in Dallas. The respondents were also asked to rate each of the sixty-seven reputed leaders on a "Power-Rating Scale" ranging from 1 (least influential) to 4 (most influential). Persons judged by the respondents to have no community-wide influence could be indicated by the letter "N." A partial check on these ratings was provided by a request that each respondent check the names of the ten persons he would most want to work with on a major project for which he was responsible.

Information thus generated leads Mrs. Thometz to conclude that major decisions in Dallas are made by a small and highly organized group of conservative businessmen, according to well-understood procedures. Tabulation of her power-rating scale reveals a "power structure" composed of seven "key" leaders and fifty-five "top-level" leaders, whose decisions are normally implemented by a larger "second echelon" group. All the "key" and most of the "top-level" leaders are members of the Civic Committee (i.e., the Dallas Citizens Council), an organization of business executives devoted to civic affairs. Both the city council and the school board are controlled by the committee through "front" organizations which nominate candidates for these positions and generally elect the candidates nominated. Major community policies—such as desegregation, public housing, education, or united fund activities—are initiated and supported by members of the committee, using other local organizations or the city government for implementation. Through self-selection and a conscious policy of training younger business executives for future leadership positions a unified and ideologically homogeneous elite is perpetuated. Though somewhat flat-topped, this is a recognizable pyramid of power, performing the essential political functions of interest aggregation, rule-making, and leadership recruitment through a tightly organized structure of economic roles.

Mrs. Thometz is sensible enough to recognize both the virtues and the defects of her method, but not sufficiently patient to allow her good sense enough time to deal with the problem at hand. Thus the great care with which she develops her power structure contrasts sharply with her almost cavalier discussion of the decision-making process. Surely we are entitled to more than two paragraphs on issues as important as public housing or education, just as we are entitled to some rationalization for her choice of issue, apart from an assurance that the issues discussed are "typical of the process by which many other community decisions are reached" (p. 69). Without further specification of the nature of these "many other" decisions and the processes by which they are made, it is quite difficult to know what the author means by the word "typical." Mrs. Thometz, in short, has completed only half of the task. It now remains for someone—perhaps Mrs. Thometz herself—to provide a closer examination of the decision process to determine whether or not the interesting conclusions set forth here are worthy of confidence.

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Política y sociedad en una época de transición: De la sociedad tradicional a la sociedad de masas. By GINO GERMANI. Buenos Aires: Editorial Paidos, 1963. Pp. 266.

Since *Estructura social en la Argentina* in 1955, Germani has published mainly in the form of articles in scattered journals, making it difficult to grasp his work as an organized whole. In 1960, some of these essays were published in a slim volume in Portuguese entitled *Política e massa* (Belo Horizonte, Brazil: Revista Brasileira de Estudos Políticos); most of the same pieces, plus others, were then collected and re-edited so as to produce an integrated volume in Spanish, *Política y sociedad en una época de transición* ("Politics and Society in an Epoch of Transition"), which is the text reviewed here. Fortunately for those who do not read Spanish, this book is being translated by Irving Louis Horowitz for early publication in English.

Germani's volume begins with two chapters presenting his theoretical perspective, which can be labeled as a combination of Mannheim, Parsons, and Lipset. It is a modified version

of the currently standard "theory of action" and appears to be written mainly as a text for students. Although it provides a framework that relates the main body of the volume to general theory, the articulation is not very tight, and many of the theoretical concepts are not used in the empirical analyses—which puts Germani in the company of a number of distinguished colleagues.

There follow two chapters on Germani's main theoretical tool: the contrast between the ideal-types of "traditional society" and "modern society." Here he shows great versatility in constructing an abstract model that integrates ideas from the early European dichotomy of *Gemeinschaft-Gesellschaft*, as elaborated by later theorists such as Redfield and Parsons with themes from the economics of development, and then placing these abstractions into an appropriate historical context. Germani is well aware of the differences between those countries that were in the vanguard of modernization in the nineteenth century and those now undertaking the process when conditions are very different.

Germani, better than most writers, is able to demonstrate the complexity of the modernization process. He details the changes toward national identification and effective political integration, toward an open stratification system, and toward a modern family system that are essential parts of the transformation. And he uses effectively the notion of non-synchronous change, an elaboration of Ogburn's "cultural lag." He shows how formal political structures, such as constitutions, have often been borrowed by Latin American countries from more advanced nations and imposed upon a traditional society that lacks the educated middle class so essential to the operation of representative democracy. He discusses the strains between the demands of new industrial bureaucracies and the older forms of human relations based on prescriptive rather than rational choice. He details the distress of those caught between traditional family values and the facts of modern urban life and shows how that distress is related to the rate of demographic transition. He shows that certain strange bedfellows can appear during modernization, such as a traditional authoritarianism stemming from rural landowning and a left-wing authoritarianism among newly urbanized and half-integrated urban masses.

The ideal-type contrast between traditional and modern society is elaborated into a series of stages of transition; comparative census data from various Latin American countries are used to place them appropriately. Here the material is suggestive but not quite satisfying. Various measures are used (such as urbanization, distribution of labor force as an index of social class structure, and education), but these are not related to one another as systematically as possible. For instance, rank orders based on the various indexes do not correlate perfectly, which the notion of non-synchronous development would predict, but the patterns of deviation from perfect correlation are not adequately measured or interpreted.

Finally, there appear four chapters on Argentina. Here Germani is at his best: historically informed (not only about Argentina, but about other countries that have gone through the modernizing process); theoretically sophisticated (always seeking analytic perspectives that illuminate the Argentine experience while making comparative thought possible); and empirically imaginative (using census data that were available but not probed, and combining them with material from field investigations which stemmed mostly from his own efforts as research entrepreneur).

Germani studies the importance of immigration in Argentina, which had, relative to its population base, one of the most massive influxes of new citizens known to the world; early in this century, three-quarters of the adults in Buenos Aires were immigrants, and very few had bothered to become citizens despite the ease of that legal process. There was differential distribution of migrants in the labor force, with disproportionately high representation in commerce and industry. The immigration was a result of a deliberate policy of modernization by a liberal elite, yet had unforseen consequences on the power of that elite. Germani shows how foreign migrants have, in more recent years, been replaced by internal migrants, which keeps Buenos Aires growing at an even faster rate.

The central political problem of contemporary Argentina concerns the integration of the new urban masses. They live in an environment that is technologically modern, for they work in industrial and commercial bureaucracies of an advanced type; they are subjected to the stimuli of the mass media; they live in great urban concentrations (a third of the na-

tional population lives in greater Buenos Aires). But they have not yet been absorbed completely into modern social structures. Many are recent migrants from traditional rural zones; few have had a secondary education; and the leaders of business and politics have traditionally looked down upon them as inferior—or at least unimportant—people. Institutions of popular protest, such as labor unions, were suppressed by dictatorial action during the 1930's, at the very moment of the most rapid urban expansion when the pressures of the new mass society were steadily growing. The new urban workers felt dehumanized and rejected. Then came Peron, who supplied them with a sense of participation, of dignity, of worth—and also threw them a few crumbs of bread in the form of increased wages. Germani insists that the genuine support received by Peron cannot be explained away by the bread or by demagogy, and he warns his compatriots that if the nation is to modernize completely and thus reduce the tensions of non-synchronous development, it must find ways to institutionalize the legitimate protest of the masses and incorporate them into active participation in the polity.

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The Civic Culture, Political Attitudes and Democracy in Five Nations. By GABRIEL A. ALMOND and SIDNEY VERBA. Princeton, N.J.: Princeton University Press, 1963. Pp. xi+562. $8.50.

How to account for the functioning of a political system? For a time it appeared to political scientists that the basis of a proper explanation lay in the arrangement and powers of the various institutions of government, together, perhaps, with reference to the political history and experience of a country. In particular, Americans, with their attention on the law of the Constitution, did it this way, as may be seen in A. L. Lowell's account of *The Government of England* and the work of F. A. Ogg and Harold Zink. But there was another theme: a search for a cultural ethos, or an understanding of "the people and culture" of a country, a theme which, curiously now in the light of later events, was developed by Europeans looking at the first new nation, the underdeveloped people of the United States. Tocqueville is still the most brilliant of these interpreters; but Bryce, whose sections on "public opinion" and "social institutions" in *The American Commonwealth* have no parallels in Lowell's work on England, was a distinguished expositor of this school. Inevitably, these interpretations of opinion and culture were impressionistic, based on selective observation and the reports of "native informants."

Now fresh from their interests in the new nations, emerging as leaders of the behavioral school of political science, and armed with techniques and funds never available to another generation of scholars, Almond and Verba have developed a set of explanatory hypotheses and comparative survey data to show in interesting detail the cultural bases of political functioning in five nations—Great Britain, Germany and Italy in Europe, and the United States and Mexico in North America. In doing this they draw on and test many hypotheses developed by others with less adequate data and explore some new ideas of their own.

The book has five parts. First, a statement of their approach and basic theory, with their three types of citizen orientation to the political system being most important: (1) "parochial," that is, distant from and unaware of political phenomena, (2) "subject," an orientation toward governmental output, the rules and services which affect people's lives, and (3) "participant," an active orientation toward politics and the possibilities of influencing government. The second part offers a discussion of the findings of the five surveys with respect to people's awareness and feelings toward their governments and the politics of their countries, patterns of partisanship, the sense of civic competence (a major dependent variable), and certain attitudes toward the entire system, including some very interesting material on the components of national pride. The third part, perhaps the most interesting, shows the underlying importance of a general feeling of faith and trust in other people and the propensity to work with others in effecting political ends. The fourth part develops some interesting ideas on the role of educational and sex variables in affecting political culture and gives a profile of the main characteristics of the five nations. The fifth part, perhaps the weak-

est in the book, suggests how the political culture affects democratic stability in a political system.

The message of the book may be stated bluntly as follows: The United States and Great Britain, in contrast to the other three countries, have stable, effective, and responsive government because their people trust each other, work well together, keep partisan political attitudes within bounds which are transcended by overarching feelings of mutual respect in primary group structures, and are willing to assume responsibility for their own political fates. The secret of their political success lies not in their institutions, but in the interpersonal relations of their people. The material to support this argument is rich, illuminating, and vital to the study of the political process.

The book is sometimes wordy, sometimes elaborates the obvious at painful length, suffers from typologies that are not adequately explained, uses scoring procedures on which the Appendix offers no information, offers a defective index (for example, there is no reference to "parochial"; "alienation" is first explained on p. *22* but the first index reference is to p. *82*), tends far too much merely to describe in words the distributions of attitudes presented in the tables with far too little effort at some second-order analysis. But with all these minor defects, it gives us some good ideas to work with and invaluable data on a crucial aspect of the political system.

ROBERT E. LANE

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The Soviet Political Mind. By ROBERT C. TUCKER. New York: Frederick A. Praeger, Inc., 1963. Pp. xiii+238. $2.25.

Professor Tucker has assembled ten of his essays in this thought-provoking volume. Two major problems attract his attention—problems that in a general form are of special interest to several of the behavioral sciences. One is the analysis of change in the Soviet system; the other is the relationship between ideology and policy.

In any analysis of social and political change in a specific historical setting, a crucial problem is the identification of what changes are most worthy of explanation. Tucker takes an interesting approach. As a basis for treating change in the post-Stalin era, he traces Stalin's rise to power and outlines the essential characteristics of Stalinism. His framework for this analysis is the comparative study of totalitarian systems. From this viewpoint, it is not the October Revolution that is comparable to Hitler's accession to power in 1933 or Mussolini's march on Rome; it is, rather, the policies that Stalin introduced in 1928. Nazism emerged from the ruins of Weimar, Fascism displaced the liberal Italian Republic, and totalitarian Communism destroyed not the Tsarist state, but Lenin's Bolshevism and the New Economic Policy (NEP).

Tucker is impressed by the way in which Stalin consciously adopted an ideology from Lenin but copied his style of governing from Ivan the Terrible. He argues, for example, that Stalin modeled his NKVD on Ivan's *oprichnina* and patterned the purge of the old Bolsheviks after Ivan's liquidation of the Boyars. In this way, the influence of old Russia upon Soviet policy is brought into the book, and is used as part of the groundwork for treating the second major theme—the relationship between ideology and policy.

The ideology that is treated, then, is not simply Communist but also Russian, and the author shows how these two elements are intertwined in a complex way to influence Soviet policy.

The two themes of the book are, from a broader perspective, the same. Tucker views change in the Soviet system largely as the result of changes in policy. It is at this point, however, that the book becomes somewhat disappointing. The author is no crude *ideological determinist*. He recognizes that there are other factors besides Russian and Communist ideology that determine policy. The personality of the leaders is one of these factors; what might be called *reality* is another. But these other factors are never clearly treated, nor is their interaction with ideology as joint determinants of policy rigorously analyzed. The model of social change that he employs in his treatment of change in the Soviet system remains at best implicit.

This book clearly reveals that the author has intuition, insight, and an impressive knowledge of Russian and Soviet affairs. The treatment of political change and of the relationship between ideology and policy reflects

these qualities. While this work represents a laudable attempt to place a totalitarian system in broader perspective, it also reveals some of the problems that arise when one seeks to bring the insights of the area specialist to bear upon major theoretical problems in the social sciences.

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University of Minnesota

The Press and Foreign Policy. By BERNARD C. COHEN. Princeton, N.J.: Princeton University Press, 1963. Pp. ix+288. $6.00.

This is a study of the nature and effects of foreign affairs reporting in the United States. Written by a political behaviorist, it describes the production, distribution, and consumption of foreign affairs news as a process of interaction between various participants. The perceptions and values of these participants—news sources, reporters, editors, foreign-policy specialists, and the attentive public—are believed to mold newspaper reports on foreign affairs in such a way that the product is inadequate in both quantity and quality.

Professor Cohen's functional analysis of the role of the press in the making of American foreign policy is based in part on a series of unstructured interviews with Washington foreign-policy specialists who write for the prestige papers, and on some 150 interviews with persons in staff or policy positions in the executive branch and in Congress. These interviews, conducted between 1953 and 1960, are buttressed by prodigious reference to readership studies, opinion polls, biographies, and the many previous publications dealing with the press, public opinion, and foreign policy.

The author's central thesis is that the press —by which he means the handful of eastern "prestige papers" and some widely syndicated and respected columnists—influences the making of American foreign policy because it sets the stage on which the policy-maker must perform. "It may not be successful much of the time in telling people what to think, but it is stunningly successful in telling its readers what to think *about*." By no clear criteria, a small group of journalists who specialize in foreign affairs determines what becomes news and what goes unreported, thus deciding for policy-maker and public alike the major foreign-policy issues of the day.

This thesis, which at times gets lost in a welter of side issues, leads Cohen to examine the background and philosophy of the reporter who specializes in foreign-policy news, the way in which he goes about his work, his relationships with news sources, and the consequences of his activity. He finds that the conventional emphasis on timely, controversial, and dramatic news leads to foreign-policy coverage that is "spasmodic, piecemeal, impressionistic, and oversimplified, sometimes inaccurate or garbled, and generally failing to deal with policy issues until they have become matters of public record." Yet this unsystematic treatment of a series of dramatic events is read by and influences both policy-makers and other reporters.

Readership studies have shown that only a small percentage of the general public reads more than headlines about foreign affairs. To develop a taste for international news, some editors try to present it in a more understandable, readable fashion, and most give it front-page treatment. But Cohen feels that the hope of expanding the audience for foreign affairs news by a greatly simplified discussion is largely illusory. Even if it could be done, he argues, the material to which this new audience is attracted would be so far from reflecting complex and difficult international political realities that the reader could not make a sound judgment on policy alternatives.

These conclusions lead the author to suggest a radical innovation: Since foreign affairs news obviously serves a specialized audience, why not handle it like other materials meant for relatively few readers? Retain the front-page stories on the big topical issues, but add an inside page addressed specifically to the foreign-policy specialist and to the small group of attentive citizens. This approach would maximize intelligent participation by opinion elites, while making more and better foreign affairs coverage available to any citizen inclined to read it.

Unfortunately, Cohen's book is not likely to receive the attention it deserves. Despite evidence that public participation in the making of foreign policy is very limited, democratic theorists will not wish to dignify this observaton with *de facto* recognition. Jour-

nalists will take offense at the loose lumping of reporters, columnists, editors, and foreign correspondents into one mélange, and at the author's effrontery to doubt the feasibility of "objective" interpretation. The reader will be discouraged by the author's pedantic, sometimes turgid prose and needless redundancies. Nonetheless, those seeking a better understanding of the role of the press in the process of foreign-policy-making will find this book worth wading through.

KENNETH P. ADLER

United States Information Agency

Simulation in International Relations: Developments for Research and Teaching. By HAROLD GUETZKOW, CHADWICK F. ALGER, RICHARD A. BRODY, ROBERT C. NOEL, and RICHARD C. SNYDER. Englewood Cliffs, N.J.: Prentice-Hall, 1963. Pp. vi+248. $5.50.

We now know what happens to avid Monopoly players when they grow up: they do game-simulation experiments in research or teaching. This is no gibe (or if it is, I include myself in the target), but I am struck by a covert reason for simulation which none of the authors is willing to make explicit—simulation is fun. Students and subjects, teachers and experimenters, all seem to become involved and enjoy it.

The authors of this volume devote themselves to an evaluation of game simulation as a *means* rather than as an end in itself. The primary focus is the particular internation simulation developed at Northwestern University. A conscientious effort is made to provide sufficient detail (including about twenty pages of appendixes with sample forms, examples, etc.) so that one could, using the book, use the internation simulation in his own classroom or laboratory. There is an important difference between this simulation and earlier role-playing games (e.g., mock UN sessions or political conventions). In the internation simulation, the player is not asked to act the way he thinks the head or foreign secretary of some particular country *would* act; rather, he is put in a situation with constraints and obligations, and asked to act *as himself*. In the Northwestern simulation, five hypothetical nations, differing in initial strength, are represented by teams of students who proceed to negotiate, threaten, make war, form international organizations, and so forth.

No one in this volume claims that game simulation is anything more than a useful complement to more traditional methods of research and teaching. The authors seem sensitive to anticipated criticism of the gimmicky and/or faddish character of such experimentation and are anxious to avoid overclaims. On the other hand, there is little serious or systematic criticism of the general usefulness—only cautious optimism.

One point not much discussed in this volume is worth mentioning. The dual functions of teaching and research cannot always be served simultaneously. Research purposes require some restrictions on interaction and communication so that the experimenter may have accurate records of what has transpired; the parameters of the game must remain relatively frozen. On the other hand, for teaching purposes we may need to maximize richness of interaction, spontaneity, and the possibility of innovation. In pursuing teaching goals, we may change features of the game while it is in progress whenever it seems desirable to do so.

The authors have an extensive discussion of the use of the internation simulation in teaching and they make a more convincing case for its value in this realm. Its teaching potentiality rests on its impact on students' motivation and the effects of their more active involvement in the educational process—the appeal is that of learning by vicariously doing. The authors would readily grant, I am sure, that the suggestive evidence they present on the pedagogical usefulness of simulation is no final or conclusive answer. But for me, it is through its vision of the exciting possibilities of game simulaton as a teaching device that this book ultimately succeeds.

WILLIAM A. GAMSON

University of Michigan

Liberalism and the Retreat from Politics. By WILLIAM J. NEWMAN. New York: George Braziller, Inc., 1964. Pp. xv+190. $5.00.

May I use the pages of your estimable journal to pose a pedagogic problem to your academic readers? I was induced to offer a

freshman course on "Critiques of American Society," and it was put to me that if it was to serve its function of recruiting majors to the department, its level should not be too high. In line with this criterion, I asked each student to write a ten-page review of one of the following books: Riesman's *Lonely Crowd*, Mills's *Power Elite*, Galbraith's *Affluent Society*, Whyte's *Organization Man*, Goodman's *Growing Up Absurd*, Arendt's *Between Past and Future*, and Heilbroner's *Future as History*. I told the students that I had read all these books and did not want summaries, and I suggested strongly to them that some negative criticisms would be in order.

Now, one student has written a 190-page term paper (complete with index, no less) reviewing *all* of the books. He ignored the instructions and summarized each of them, so that I am sure he read them. Indeed, he went beyond the multiple assignment and even did some research on his own. He dug up an article in *Atlantic Monthly* in which Galbraith had pointed out that "for those who seek change, criticism is an essential element of political action." And from one of Riesman's innumerable prefaces to other men's books, he quoted: "While we can prefigure the end of tradition, it is hard to envisage the beginning of enlightenment." You might suspect, seeing such gems in cold print, that the student was pulling my leg, or that he was trying to show how an adeptness with words can sometimes hinder communication. But I assure you this is an earnest plodder, with not a drop of irony in his veins.

Riesman and Mills, he wrote, have acted as "significant and responsible leaders." (This was near the beginning of the term paper, where I was still marking up the margin, and I wrote: "This reminds me of the comment that the Holy Roman Empire was not Holy, not Roman, and not Empire.") But they have also "led liberal thought into some wrong and even dangerous paths." One of their faults, which they share with the others on the list, is that they have lost a sense of history. Was the student getting at something here? The reconstruction of the past from its remnants in the present was expunged from anthropology a generation or more ago; would he tell Riesman that twentieth-century Vermont is not the same as eighteenth-century Philadelphia? Twelve of the original thirteen states had property qualifications for voting; would he ask Mills how such facts fit in with his thesis that a "power elite" has developed since then? Not at all. "History, after all," he wrote, "must be going someplace or there is no story to it and it signifies nothing. . . . The liberal social critic today has told us that our society is No Good. He tells us that it should be changed. The rest is silence."

The rest, alas, is not silence. The student's program is to offer a new image of government to the American public, "an image of government as a manifestation of a whole society that is engaged in pursuits important to society, freely chosen by a process of political interaction among those who compose society. It is an image that can be brought down to earth by facing the concrete issues of this particular moment of history and using them to engage both the public and the government in their solution."

You see what I mean, and you see my dilemma. Anyone who does that much work ordinarily would get an A, but someone who offers a warmed-over campaign speech, complete with mixed metaphors, as a radical new program—should he be encouraged, or even permitted, to go on?

I neglected to mention that the student is now a professor of government at Boston University. Maybe that is pertinent.

William Petersen

University of California
Berkeley

Change, Hope, and the Bomb. By David E. Lilienthal. Princeton, N.J.: Princeton University Press, 1963. Pp. 168. $3.50.

In an earlier critique of *Change, Hope, and the Bomb*, the reviewer quoted the jacket blurb to the effect that Mr. Lilienthal "has kept silent on the Atom" for thirteen years and then suggested that such silence might profitably have been maintained. If we look to this former AEC and TVA Chairman for a thoughtfully reasoned or carefully researched analysis of international politics or military strategy, such a caustic comment seems fully justified, since he offers little but polemic and platitude on these issues. Nor does he contribute usefully in his discussion of the reciprocal influence between scientists and government. But on two other subjects

(closer to the core of his experience) he makes some telling, if unoriginal, points: peaceful uses of atomic energy, and relations between the AEC and Congress.

The major value of this very marginal book (based *comme il faut,* on a set of campus lectures, with Princeton the scene of the present crime) is that it makes *public* some of the inputs which affect decisions often made in *private:* the values and operational code of the individual officeholder. Here we view a forthright, intelligent, and competent public servant who played a leading role in two great transition periods, as he bares much of his social soul.

In the limited space allotted, let me comment on Lilienthal's views on the four problems he selects, preceding this, however, with a word or two about his cognitive style. Like most public officials (elected or appointed) whose education was provided either by law school or payroll meeting, his distrust of scholars and intellectualizing is profound. We do not understand the "technique of the manageable job" and are prone to look for utopian Single Solutions. Thus he eschews "theories and abstractions that are found in [others'?] books." Likewise, he tends to think of "science" as the world of laboratories and test tubes; and when he does include the "so-called social sciences," he confuses the work of the military think-factories with "empirical studies."

Following a breathless exposé of our erroneous premises and policies in dealing with The Atom (his caps), he proceeds to knock down these straw men with the sure instinct of the practical—but profoundly ignorant—man of affairs. This onslaught leads to a sadly uninformed attack on "overnight disarmament" and world government, often equating unilateral reductions with reciprocal ones, and ignoring the well-recognized link between arms reduction and the strengthening of UN peace-keeping machinery. This obsessive American preoccupation with disarmament he attributes to our fear and anxiety over the Bomb, giving the reader two wildly inaccurate generalizations in short order. On scientists and government, Lilienthal is alternately banal and misinformed, but on the Peaceful Atom he does a compelling job of debunking the atomic empire-builders, and in dealing with AEC–Joint Committee relations, his expertise likewise produces some valuable commentary.

As one reads this well-phrased but badly reasoned and undocumented little volume, alarm is the dominant reaction. This is not an arrogant, opportunistic, Red-baiting individual (see Lewis Strauss's *Men and Decisions* for the sake of comparison) but a serious, dedicated, and competent public official. Yet in both Lilienthal and Strauss we find the same weaknesses: undisciplined mental processes, the conceptual skills of an average undergraduate, and most serious, an ignorance of their ignorance. As social scientists we had better begin to consider alternative means of educating and recruiting the men who may well bring us to the point where homicide and suicide are society's only options. Their intellectual inadequacies have already brought us a long way down that road.

J. DAVID SINGER

University of Michigan

German Catholics and Hitler's Wars. By GORDON ZAHN. New York: Sheed & Ward, 1962. Pp. 232. $4.75.

During World War II the Catholic Church in Germany supported the war effort "as a Christian duty and as a means of protecting Germany's most cherished values." Professor Zahn documents this fact convincingly in this book. He also adequately accounts for this support not only in terms of the quite obvious external controls exercised in wartime, but in terms of powerful internal controls—feelings of patriotism, the desire for a victorious Germany—shared by Catholic clergy and laymen as well. Apparently emotional and traditional attachment to *Volk, Vaterland* and *Heimat* transcended all other values among German Catholics as it did among Germans in general. Nothing else could have been expected. The majority of people in all countries and at all times virtually always rallies behind the government in time of war.

However, Zahn raises an issue about this support of World War II by the Catholic Church. He holds that Hitler's Wars constituted "the classic example" of the unjust war condemned by the Church. Therefore, he views the official approval by the Church as "the tragic distortion of traditional Catholic teaching." But Zahn fails to provide evi-

dence that there was any awareness by Catholic leaders that the wars were unjust and that therefore it was a mortal sin to participate in them. Zahn refers to the severe censure which the Catholic bishops directed against National Socialism for its policy on euthanasia, the secularization of Catholic schools, the confiscation of religious property, and the promotion of Germanic paganism. On the basis of this condemnation by the bishops he would have expected them to oppose Hitler's war plans as well.

In my opinion, Zahn poses a spurious "value-selection dimension." In fact no conflict of values existed for most of the German people. One has to realize that one of the basic reasons for Hitler's success was that much of what he wanted was enthusiastically desired by many Germans. He got away with actions that were morally reprehensible to individuals as such by making opposition in general unpatriotic and hence unacceptable to them as members of the nation.

Zahn does not seem to have a case against the Church on sociological grounds, but he has a good case on theological grounds. We may agree with his proposal that "the failure of Christianity" to oppose war, which can "most clearly be seen in the support by German Catholics of Hitler's wars . . . presents a real challenge to the theologian to analyze that failure and to discover the weakness in the traditional formulation and application of the Catholic morality of war that made such failure possible" (p. 223).

THEODORE ABEL

Hunter College

Responsibility for the Community: A New Norm Confronts Tradition in Lutheran City Churches. By IMOGEN SEGER. Totowa, N.J.: Bedminster Press, 1963. Pp. vii+366. $5.50.

The question of the survival of the inner-city church is of obvious importance to religious leaders. It is of potential significance also for the sociologist who is interested in the role of urban religious institutions, in organizational change, or in the influence of environmental factors and membership characteristics on the structure or goals of an organization. Seger contends that the view that the "Social Gospel"—an action orientation toward social planning and improvement of the conditions of human life—is the key to survival is generally characteristic of American Protestantism. To achieve theological goals, the church must first survive; to survive it must be effective, and effectiveness means some sort of effective social action. In contrast to most of American Protestantism, however, this emphasis on social action is in direct conflict with Lutheran religious tradition and hence poses a special problem for the Lutheran churches. Using data collected by the National Lutheran Council, consisting primarily of questionnaire responses from members of seven Lutheran churches and supplemented by information about the church, its neighborhood, and its pastor, this study attempts to determine how this new norm of effectiveness "fares in Lutheran city congregations of different kinds, in what way and how far effectiveness as a normative prescription is understood and accepted, put into action or adapted and modified." Briefly, the analysis reasons that effectiveness is accepted to the extent that concern for social problems is considered part of the job of the congregation. It is considered part of the job of the congregation to the extent that it influences general satisfaction with the congregation and pastor.

Satisfaction with the job the congregation is doing and with the job the pastor is doing is measured by direct question. Concern for social problems is measured by satisfaction with specific actions of the church in easing city and neighborhood problems. This measure is indirect in that it is really a measure of whether or not the respondent feels that the church is doing a good job in solving one set of problems (slums, juvenile delinquency, overcrowded schools, and racial conflict, among others) and whether or not he feels the church is doing a lot to meet a second set of problems (loneliness, ill feeling between different kinds of people, changing population). Except for a discussion of the extent of racial integration of the congregations, no information is given regarding the actual efforts of the churches in any of these problem areas. The difficulty with this measure is that the opposite of satisfied is not necessarily dissatisfied. If the statement that the church is doing a good job on juvenile delinquency or is doing a lot to meet the problem

of ill feeling between groups of people permits the assumption that the respondent feels the church should be working in these areas, the reverse statement by no means permits the reverse assumption. The individual who says that the church is not doing much to meet a problem may merely be more realistic or more honest; but his answer does not indicate whether or not he thinks the church should do anything about these problems.

A fourth measure used is that of personal religious satisfaction. Respondents were asked how much church membership helps them to know of God's love and care; those who checked "much" were given a high rating on personal religious satisfaction. But isn't this really the response one would generally expect to get from church members, especially from those who are sufficiently motivated to return the questionnaire? The fact that the uniformly high rate of satisfaction here neither varies by demographic characteristics nor correlates well with other measures of satisfaction seems to reinforce the suspicion that the answers merely represent the expected answer to an obvious question and are not necessarily measuring anything except conformity to that expectation. The conclusion that of the four types of satisfaction this is the most important for the survival of the church does not seem warranted.

The author concludes that the practice of effectiveness has helped only one of the seven churches survive and that the price of survival was rather high; this one church has successfully integrated but lost half of its former members in the process. It is unfortunate that there is no information on the practice of any type of effectiveness other than integration. Surely it is risky to generalize from the area of racial integration to other aspects of effectiveness.

The book has minor annoyances. One would perhaps prefer that percentages not be computed when *N* equals 1; there are several instances in which the percentages are based on ten or even fewer cases. To one accustomed to reading *N* as the total number of cases, it is momentarily confusing to have it used to indicate the actual number of cases represented by a given percentage; one is more than momentarily confused after the discovery that the symbol is not used in a consistent manner but may carry either meaning. The way it is being used in any given table is not always clear to the reader.

Within the limitations of the data available to the author the book offers considerable insight into the relationship of the inner-city church to its neighborhood. To the sociologist, however, the book is somewhat disappointing; the conclusions seem stronger than the data warrant, and it is discouraging to see one who has earned a Ph.D. at Columbia equate function with motive and purpose.

JOAN STELLING

Purdue University Calumet

Handbuch der empirischen Sozialforschung. Edited by RENÉ KÖNIG. Stuttgart: Enke Verlag, 1962. Pp. xvi+649. DM. 100.

This is a landmark in the development of social research in Germany: over the space of more than 650 pages, eighteen authors explain and comment on different aspects of social research. Lucidly written and well documented, their contributions offer a comprehensive view of historical developments, assorted techniques, and selected areas of inquiry. According to the editor, René König, a second volume is to follow which will deal with different fields in sociology.

This first volume has three major parts which differ in subject matter and are somewhat distinct in quality. The center section, the core of the *Handbuch* in both content and coverage, seems best. This section deals with basic methods and techniques of empirical social research. Its ten component texts account for most of the armory of social investigation: in topic, the articles range from observation as a comparatively simple way of reaching the facts to experimentation as a highly sophisticated technique.

Most of the contributions to this section are so thorough, solid, and rounded that they might well stand on their own. There are, for instance, an excellent article on factor analysis by Peter R. Hofstätter, a highly informative text on basic concepts and operations in statistics by Paul Neurath, and a paper on experimentation, by Robert Pagès, which wisely slights technology to the benefit of perspective and problem-orientation.

More than one-third of this center section was contributed by Erwin K. Scheuch, whose articles on interviewing, sampling, and scaling

should become standard fare for aspiring as well as advanced researchers. His presentation rates especially high on documentation and compactness. The text on sampling, for example, tends to dispose of quota-sampling as dubious and therefore an illegitimate claimant to precious space. More than 10 per cent of his exposition is reserved for "select bibliographies."

Other contributions of note in this section are one by the editor (on observation) and one by Jiri Nehnevajsa (on panel interviews and on sociometry). Of lesser importance is a brief article by Werner Mangold, on the "group discussion" technique, that is, a study of group reactions to given discussion stimuli. Though resourceful in argument and experience, the author fails to explain away one critical fact that may well prove fatal to this technique in the long run: the fact that several studies using this tool have failed to produce convincing results.

The first part of the *Handbuch* is also well done. Its focus is on the history and basic methodological issues in social research. The historical part proper, written by Heinz Maus, is brief. While some might prefer more detail, it may have been just as well not to block the over-all purpose by a lengthy "history of the elephant."

König's Introduction in Part One begins with the concept of "empirical sociology" and extends into a discussion of the ethical problems of empirical sociology. Apart from giving a preview of what is to follow, this introduction is of substantive value to the over-all examination of social research. For instance, König broaches the question of how concepts are evolved and employed in research. Unfortunately, his discussion remains sketchy because he does not consider it fully relevant at this point—which would seem logical only from his debatable view that the evolution of concepts is not part of empirical science and that analysis proper begins only when concepts are applied in specific hypotheses.

Hans Albert, in his essay on basic methodological problems, defends the positivist orientations of the research enterprise. In a sequence of rigorously critical statements, he explodes a number of fallacies common in non-positivist approaches to facts and their meaning. Such criticism is supplemented by an extended statement on the logical status of social science; it deals largely with the methodology of ideal types, models, theories, and related constructs.

Special mention is due the contribution by Hans L. Zetterberg on theory, research, and applied research. From a didactic point of view, this chapter perhaps excels the rest.

Finally, in the third major part of the *Handbuch*, attention shifts between three main subjects: special fields on the fringe of sociology, select subjects of sociological inquiry, and specialized techniques of research. Such heterogeneity lends a certain air of confusion to this part which might easily have been remedied if the papers on techniques (content analysis, "biographical method," intercultural comparisons) had been included in the center section of the book. The three papers, incidentally, are of quite varied quality and usefulness.

The chapters on peripheral fields might have fitted better into the coming second volume of the *Handbuch* which will deal with specialized sociologies; these are chapters on demography (by K. Mayer), on ecology (by Amos H. Hawley), on sociography (by H. D. de Vries Reilingh), on ethnography-ethnology by G. Heilfurth), and on social history and the sociology of history [*sic*] by G. Salomon Delatour). It is difficult to be critical of content here because the authors proceed mostly from the premises of their own fields and do not entirely talk the language of the other authors or, for that matter, of sociology.

Regrettably, this concluding part slights the one major topic that might most appropriately have served as a separate theme; that is, a confrontation of select subjects of research with the peculiar combinations of techniques called for in their analysis. This is exactly what the over-all title for this part promises: a discussion of "complex approaches." One author explicitly sets out to do it justice (Conrad Arensberg, in his article on community as object and paradigm), and several others come close (e.g., Mayer, Hawley, on population, the city). But more would have been accomplished by a more definite attack on this question.

Taken as a whole, however, this volume admirably accomplishes what it aims to do: to present—in presentation not too technical nor popular—the whys and hows of social research. Now it is up to the readers to do as good a job of applying these ideas and instruments as most of the authors did in setting them forth.

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Finally, in the third major part of the *Handbuch*, attention shifts between three main subjects: special fields on the fringe of sociology, select subjects of sociological inquiry, and specialized techniques of research. Such heterogeneity lends a certain air of confusion to this part which might easily have been remedied if the papers on techniques (content analysis, "biographical method," intercultural comparisons) had been included in the center section of the book. The three papers, incidentally, are of quite varied quality and usefulness.

The chapters on peripheral fields might have fitted better into the coming second volume of the *Handbuch* which will deal with specialized sociologies: these are chapters on demography (by K. Mayer), on ecology (by Amos H. Hawley), on sociography (by H. D. de Vries Reilingh), on ethnography-ethnology by G. Heilfurth), and on social history and the sociology of history [*sic*] by G. Salomon Delatour). It is difficult to be critical of content here because the authors proceed mostly from the premises of their own fields and do not entirely talk the language of the other authors or, for that matter, of sociology.

Regrettably, this concluding part slights the one major topic that might most appropriately have served as a separate theme; that is, a confrontation of select subjects of research with the peculiar combinations of techniques called for in their analysis. This is exactly what the over-all title for this part promises: a discussion of "complex approaches." One author explicitly sets out to do it justice (Conrad Arensberg, in his article on community as object and paradigm), and several others come close (e.g., Mayer, Hawley, on population, the city). But more would have been accomplished by a more definite attack on this question.

Taken as a whole, however, this volume admirably accomplishes what it aims to do: to present—in presentation not too technical nor popular—the whys and hows of social research. Now it is up to the readers to do as good a job of applying these ideas and instruments as most of the authors did in setting them forth.

Perhaps one may wish that the *Handbuch* become a status symbol for sociologists. True, this book is expensive. But then, most status symbols are.

HEINZ HARTMAN

University of Muenster
Dortmund, Germany

Concepts of Personality. Edited by JOSEPH M. WEPMAN and RALPH W. HEINE. Chicago: Aldine Publishing Co., 1963. Pp. 514. $8.95.

The viability of a science depends not only upon a continuous flow of new data and ideas, methodological innovations, and theoretical formulations, but also upon a constant re-examination and re-evaluation of the current accumulation of data, methodology, and concepts. *Concepts of Personality* attempts to survey the current status of a number of topics in the area of personality.

It grew out of a series of lectures delivered to students and staff of the Interdepartmental Committee on Clinical and Counseling Psychology at the University of Chicago. Like many such lecture series which turn into books, there is considerable unevenness in the quality of the chapters. Some of the writers take their responsibilities lightly. Others use the occasion as a showcase for their own research, which is all right if one is a Cattell but not all right if one's wares are skimpy. Some do not seem to possess the necessary qualifications for discussing the topics they have been assigned.

Fortunately there are many competent, and several really outstanding, chapters in this book edited and introduced by Joseph Wepman and Ralph Heine.

The first two chapters, "Learning Theory and Personality" by Janet Taylor Spence and "Perception and Personality" by Charles Eriksen, are strong chapters. It is apparent that they know what they are talking about and that they have made a serious attempt to find new meanings in the data. These two chapters fall under the rubric of "Basic Processes," which includes four chapters.

The next section, "Theories of Personality," consists of seven chapters and covers such theorists as Freud, Lewin, Allport, Murray, Kelly, Adler, and Skinner. John Shlien's chapter on phenomenology is gracefully written, lucid in a domain marked by murky writing, and erudite. He does not let his preference for this kind of psychology prevent him from discussing the problems which phenomenology faces in obtaining a hearing from a predominantly positivistic American psychology.

The three chapters on "Social Processes and Personality" are uniformly good, and one of them is excellent. Norman Bradburn's account of the cultural context of personality theory is just about as good a short introduction to this topic as one can find. Robert LeVine shows how Hull's behaviorism has been useful in anthropological investigations. But the stellar chapter of the entire book is Patricke Heine's masterful elucidation of the problem of personality in sociological theory. It reflects deep thought and broad knowledge, historical perspective as well as a firm grasp of the contemporary scene, and an acute sixth sense for smoking out the formidable issues that one faces in doing research in this area, whether he be a sociologist, anthropologist, or psychologist.

Raymond Cattell's discussion of his multivariate experiments and their implications for personality theory in the section "Methods of Personality Assessment" is what one might expect from a man who believes passionately in the significance of his work and wishes to convert others to the cause. It is a clear presentation of a complex subject.

A total of seven competent chapters, to which could be added Sechrest on George Kelly, Dreikurs on Adler, and Garfield on the clinical method, constitutes a pretty good batting average for a book of this kind.

CALVIN HALL

Institute of Dream Research
Miami, Florida

Classroom Group Behavior: Group Dynamics in Education. By MARY A. BANY and LOIS V. JOHNSON. New York: Macmillan Co., 1964. Pp. xii+412. $5.95.

Although the authors do not actually say so, it would appear that this text was developed from lecture notes for a course on classroom group behavior given at Los Angeles State College in the Division of Education. They do thank "the more than 300 teachers who were willing to give their efforts to describe group behaviors that created problems in their classrooms and to the many teachers who tried us-

ing participatory techniques with their classes." "The majority of these teachers' reports," they go on to say, "were so clearly written that the only changes made were those of eliminating identifying data." These teachers' reports make up a large part of the text. For example, in chapter x on "Frustration," a short description of the study by Polansky *et al.* on contagion is followed by eight pages of teachers' reports interspersed with comments by the authors The reports are headed "Gary knows." "the class clown," "the funny batter," "the boy who kicked the teacher," "Fredie," "rebel with paint," "the blame," "the untouchable," and "the whistle." Since these reports and others throughout the text draw heavily on grade-school situations, one would suppose that the book would have its greatest appeal for the grade-school teacher or future teacher.

As another indication of the probable audience for the book one can note the "questions for study and discussion" appended to each chapter. In the same chapter on frustration there are twelve questions including the following:

> Outline a plan showing how you as a teacher would prepare your class to meet severe stress. Indicate the age or grade level you are thinking of and how your plan is suitable for that level.
>
> Observe a class—in the classroom, play field, lunch room, or other school location—for indications of frustration or anxiety.

The teachers' reports and the questions are perhaps the major innovations in the text since the organization of the text follows the now "standard" form of including chapters on group cohesiveness, structure, norms, goals, composition, and leadership. In style and content the text closely follows previous reviews of the group dynamics, small groups, and education literature. The authors may have leaned a little too heavily on secondary sources at times since in one chapter, at least, the same article is referred to three times, each with a different form of citation.

Interspersed with the reports of research are comments by the authors which have a definite "applied" flavor. As they indicate in the introduction, "A focus of the book is upon classroom group behavior problems and upon ways of preventing problems from occurring or guiding these problems when they do occur."

In sum, the text will probably be used by those who teach future grade-school teachers or those who may be interested in the group behavior problems faced in the grade-school classroom.

A. PAUL HARE

Haverford College

Role Development and Interpersonal Competence. By DAVID MOMENT and ABRAHAM ZALEZNIK. Boston: Division of Research, Harvard Business School, 1963. Pp. xvi+346. $6.00.

This monograph, consisting largely of Mr. Moment's doctoral dissertation, reports on an ambitious but poorly executed study of the correlates of group performance measures. The study is based on fifty-two executives and engineers who each participated in one of four group problem-solving sessions. The major purpose of the book is to explore motivational and developmental influences upon role specialization in group problem-solving.

The role-specialization typology upon which the entire book is based was developed by classifying each of the subjects in terms of group evaluations of the extent to which they contributed ideas to the discussion and the extent to which they were congenial. Ratings on these measures were made after each group was shown an eight-minute film and had discussed it for forty minutes. By classifying each subject as high or low on the "ideas" and "congeniality" ratings (scores were divided at the median), four types were derived. The four types of role specialization were then viewed in relation to ratings of group process and data obtained from questionnaires and tests each subject completed. Categories of variables were developed, using some thirty-seven different measures. Among the major categories were predispositions for role-taking (including need achievement and need affiliation), social and developmental influences (including age, salary, and education), satisfaction and developmental trends (including social mobility, closeness to family, ordinal position in family, affect measures, and independence training).

Of the four groups of subjects, two were natural groups while the other two consisted of persons brought together for "experimental" purposes. Although these groups responded differently in their behavior and ratings, these data were curiously pooled for most of the

analysis. The groups used also differed substantially in terms of the characteristics of the subjects. All of the analysis is on a zero-order level, and findings are interpreted (sometimes in a laborious way) regardless of their statistical significance or the number of cases involved. Some of the discussion in the book is interesting, but the proportion of unbounded speculation is relatively high.

It is a pity that this research was not done more competently because some of the ideas are worthy of careful investigation.

DAVID MECHANIC

University of Wisconsin

The Management of Ineffective Performance. By JOHN S. MINER. New York: McGraw-Hill Book Co., 1963. Pp. ix+369. $7.95.

Miner offers this book as a "guide for managers" in the diagnosis and treatment of inadequate work performance. His aim is to promote an understanding of the various factors that contribute to the ineffective performance of employees and to suggest a systematic way of applying such understanding in remedial action. He discusses, by categories, the "strategic factors" which, singly and in combination, engender the substandard deportment that impedes achievement of organizational goals. The latter he defines as "maximization of net profits within the limits of ethical and legal standards." Four of nine categories have to do with characteristics of the individual employee; they include intelligence, emotions, motivation, and physical characteristics. Four refer to environmental influences on the worker: family ties, work groups, the company, and cultural values. The ninth category, "situational forces," includes a heterogeneous group of influences emanating from both social and physical situations. Standards of performance are to be set by management and may be largely subjective. Four basic criteria for evaluation of work would be quality, quantity, time spent at work, and co-operation with others toward organizational goals.

Presentation of twelve case histories of performance failure gives Miner a chance to demonstrate application of his diagnostic technique. Each case is examined for "factor fit." Each factoral category is held up for acceptance or rejection until a relevant combination is selected. Suggestions fo remedial action follow.

Miner labels his operations "clinical psychology" and suggests that management employ "performance analysis groups" to do this work. Such groups would best be staffed by persons with graduate degrees in psychology or business administration who have studied appropriate courses and who have staff experience in business. Miner cautions against staffing these groups with people who hold graduate degrees from university social science departments because they tend to hold strong liberal values; "there is a real chance that individuals who hold strong liberal beliefs will become ineffective themselves."

As the source of his analytic and judgmental materials, Miner points to social science, including psychology, and the wisdom of experience. These materials admittedly do not carry the stamp "proved," but then business managers do not have the time to wait for proof. They customarily must make decisions "from risk." The author's uncertainties are at least given orderly presentation, with a handy check list for ticking off reasons for underperformance.

The twelve failure cases provide depressing reading. If the reader can free his mind from the constantly recurring thought, "There but for the Grace of God go I," long enough to pay close attention to Miner's "solutions," he will find, in the main, recommendations of firing, demotion, transfer, and psychotherapy. When the performance analysis groups move in, the underperformer may find himself subjected to a lot of moving out, down, and around.

DONALD F. ROY

Duke University

Sociology and the Field of Public Health. By EDWARD A. SUCHMAN. New York: Russell Sage Foundation, 1963. Pp. 182. $2.00.

This is one of several publications sponsored by the Russell Sage Foundation exploring the application of sociology to service professions such as law, medicine, social work, education, and in the present instance, public health. The relatively short and succinct review is directed both to sociologists and to the public health profession. A professional sociologist himself, the author shows in this review that his experi-

ence as a researcher in public health enables him to straddle and appreciate two worlds. He supports the view that sociology in public health is basically an applied social science and that in order to co-operate effectively with public health professionals it must be problem-oriented rather than discipline-oriented. Still, his discipline orientation sustains him so that he sees that "basic sociological research in the area of public health can serve as a valuable test of existing sociological hypotheses and theories. The subjecting of social theory to the crucial test of prediction and control in real-life situations can provide a most helpful evaluation of the validity of such theory." The author has demonstrated that sociology and public health have a natural alliance. This review was written while he was a staff member of the New York City Health Department. He is now professor of sociology at the University of Pittsburgh.

ODIN W. ANDERSON

University of Chicago

Health Progress in the United States: 1900–1960. By MONROE LERNER and ODIN W. ANDERSON. Chicago: University of Chicago Press, 1963. Pp. 352. $6.50.

Health status of populations and health services received by people have long been of interest to sociologists because they are important reflections of the larger social structure and dynamics. These conditions have also been of enormous interest, of course, to the health professions faced with the specific tasks of treating or preventing disease.

More than in many other fields of social concern, data on health status and services are relatively abundant. Their collection has been built into our national framework of governmental welfare services. But these data are often in reports not easily accessible to the social scientist and in multiple sources that libraries often seem to have difficulty in organizing.

Monroe Lerner and Odin Anderson, therefore, have done a great service in bringing together in one volume this collection of basic data on trends in mortality, morbidity, and health services in the United States over the last sixty years. It is based largely on work done previously for the production of the monthly bulletin *Progress in Health Services*, issued since 1956 by the Health Information Foundation. There are nineteen chapters on health status (mortality and morbidity), nine on health services (health manpower, facilities, and use of their services), and three on the influence of health on other aspects of life. Most of this material has been up-dated from its earlier periodical version, and two good summarizing chapters have been provided to draw the broad picture.

The only quarrel one might have with this excellent compilation is over its title, which could have more objectively referred to "health *trends.*" Not all of the data document "health *progress*"—for example, the course of heart diseases (even when age-adjusted) or the relative supply of physicians. Other analysts might have put more emphasis on the deficiencies in American health achievements, in relation to economic resources and potentialities. But Lerner and Anderson have a remarkable skill in simplifying and extracting the essential features of complex social data. The relationships between variables explored are offered with imagination yet caution. This book will be a valuable resource for anyone concerned about the health of people.

MILTON I. ROEMER

University of California
Los Angeles

Soziologie des Krankenhauses, zur Einführung in die Soziologie der Medizin ("Sociology of the Hospital, Introduction to the Sociology of Medicine"). By JOHANN JUERGEN ROHDE. Stuttgart: Ferdinand Enke Verlag, 1962. Pp. viii+506.

The subtitle of this volume might justifiably have been "Introduction to Sociology." The breadth of the author's coverage of general sociology and literature related to health and hospital organization appears to derive from one of his purposes in writing this book: to familiarize those interested in problems of medicine and health behavior with sociological thinking. His other goals are equally modest: to provide a framework which will facilitate empirical study of hospitals, and to make a contribution to sociological teaching of organizations (*Institutionen*). It appears to this reviewer that the author succeeds admirably.

The book is limited in offering no new empirical data, an approach which is still all too

typical of German sociology in spite of many signs of careful empirical study which developed after the war with the encouragement of R. Koenig, G. Baumert, L. Rosenmayr, and others. Also, the book offers no new theoretical formulations. But, as already indicated, the goals were more limited and in accomplishing these the author does an excellent job of pulling together enough of the relevant literature to make his points. Interestingly enough, because of the dearth of empirical study in Germany, the author draws heavily on American work.

The following one-sentence descriptions of the chapters may provide some idea of the content. Chapter i discusses sociological terms and thinking, changing aspects of illness and health, changing structure of health organization, disciplinary boundary problems of medical sociology. Chapter ii gives a history of hospitals. Chapter iii presents a theory of organizations. Chapter iv deals with internal aspects of a hospital—Purposes, expansion of purposes in the future, motives, personnel, specialty groups, authority structure. Chapter v describes inner structure of hospital, horizontal and vertical structure, role of patient, role of doctor, role of nurse. Chapter vi summarizes inner conflicts and tensions in a hospital. Chapter vii evaluates the external tensions and the position of the hospital within society. A very complete listing of references and name and subject indexes are included.

Even if not particularly original, this is a fine contribution to German sociology. For the American reader the book pulls together much of the work available at the time of writing and provides thereby the most comprehensive sociological treatment of the hospital available.

RAY H. ELLING

University of Pittsburgh

Probation and Parole. Compiled and edited by BARBARA A. KAY and CLYDE B. VEDDER. Springfield: Charles C Thomas, 1963. Pp. xi+208. $7.50.

The fields of probation and parole badly need a systematic and comprehensive treatise which would undertake to trace with sophisticated care their development, their philosophy, and their practices and problems. As courses in these subjects begin to find their way into the curriculums of undergraduate correctional sequences and graduate programs in social work, the shortage of adequate teaching materials has become increasingly acute. Until now, the choice has been between David Dressler's *Probation and Parole,* a readable, accurate, but somewhat thin coverage of the subject, and a collection of readings put together by Charles Newman and now in its second edition. G. I. Giardini's *The Parole Process* is too vocational and heavy-handed for intellectual purposes, and, unfortunately, Helen Pigeon's *Probation and Parole in Theory and Practice,* an excellent training manual, is far too out of date to be serviceable.

The present volume does little to meet the real need for an integrated and comprehensive overview of probation and parole. Professors Kay and Vedder have gathered together thirteen previously published articles, attached five pages of comment to each of the two sections, and then added a nineteen-page bibliography which, despite its carefree approach to matters of style and consistency, is thorough and valuable.

Five of the thirteen articles are from United Nations publications, while four others appeared in a single issue of *Crime and Delinquency* (July, 1962) devoted to a survey of parole prediction techniques. One of the articles in this group, in fact, presenting the opinions of a number of authorities on the value of such techniques, covers forty pages, or more than 20 per cent of the volume's text.

The co-authors' slim contributions often prove more intriguing than the material in the articles, and it might be wished that they had probed more deeply. "It is surprising that we have as many successes on probation as we do," they note at one point, although they indicate no particular reason for this conclusion; nor do they draw out the possible nuances of the observation that "probation is social casework with the power of law behind it. It is the only really promising rehabilitative technique for violators of the law." Equally unexplored is their dictum that "authorities should eliminate every unnecessary parole condition that forces men on parole to refrain from what law-abiding citizens do everyday, such as changing jobs, going out of town, buying on credit, or having a glass of beer in their own homes." (Why, one may ask, may they not also have a beer outside of their homes?)

The articles by Ralph England and J. Doug-

las Grant seem particularly noteworthy. England's reservations about possible contradictions between the ethos of parole and the intensity of its use in the face of the adoption of parole prediction devices are well stated, as is Grant's observation that some treatment, while producing desirable results with some subjects, may also produce negative results with others, and that "agencies must systematically determine which clients should receive which programs." In addition, the United Nations material is eminently solid and sensible. In short, it is not what is wrong with the book as it is, but only that there is not nearly enough of it, which detracts seriously from its value.

GILBERT GEIS

California State College
Los Angeles

Attitudes and Social Relations of Foreign Students in the United States. By CLAIRE SELLTIZ, JUNE R. CHRIST, JOAN HAVEL, and STUART W. COOK. Minneapolis: University of Minnesota Press, 1963. Pp. xi+434. $9.00.

This is the seventh research report issued under the sponsorship of the Committee on Cross-cultural Education of the Social Science Research Council. Unlike the previous studies, it deals with a cross-section of foreign students in American colleges and universities. The research has two parts. Study I was designed to sample systematically graduates and undergraduates from three types of educational institutions: small colleges in communities of under 100,000, metropolitan universities in communities of under 100,000 and metropolitan universities in communities of over 1,000,000. Three hundred forty-eight male students were interviewed, representing fifty-five non-English-speaking countries. Study II consisted of interviews with ninety-seven pairs of male students from twenty-four different countries and provided a matched-sample evaluation of official summer orientation programs. In both studies, respondents were interviewed in the fall and the spring, using fixed-alternative questions. The intention was to focus upon specific hypotheses derived from the earlier SSRC studies, giving particular attention to interaction, adjustment, and attitude.

Results are disappointing. The data are full of contradictions and inconsistencies, requiring qualification even of positive findings. We learn that Europeans differ from non-Europeans in extent of interaction with Americans, and that patterns of interaction and friendship evidenced by the visitors depend in part on how much opportunity they have to meet Americans. There are many factors for which predicted effects fail to materialize, including a number of personal characteristics (maturity, facility with the English language, motivation), several aspects of experience in the United States (orientation programs, community size, type of educational institution, change over time), and several discrepancy measures which contrast home-country practices and values with those in the United States.

One must ask why a study so well executed should have produced such unsatisfactory results. Perhaps the interview schedule was too inflexible, or perhaps the very heterogeneity of the sample makes generalization impossible. One guess would be that the conceptual framework of the study was inappropriate. The major concepts are taken from a tradition of research on intergroup relations and imply a sequence of group membership, contact with members of another group, face-to-face interaction, changes in attitude and adjustment. But apparently this sequence—although it occurs—is not of central importance in the process of cross-culture learning. As indicated in the earlier studies, foreign experience is defined in terms of norms and expectations taken from the home country not in its own terms. To carry these findings further would be to explore and conceptualize contrasting conditions of approach to the host culture, rather than contrasting types of experience during the visit, to focus upon the interplay of cultures rather than the interaction of persons.

JEANNE WATSON

Oak Park, Michigan

The Population Dilemma. Edited by PHILIP M. HAUSER. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. iv+188. $1 95.

The papers which make up *The Population Dilemma* were prepared as background material for the May, 1963, meeting of the American Assembly, a forum sponsored by Columbia

University which brings together leaders of industry, finance and the opinion-making professions for discussions of significant public issues.

Whether the volume is considered from the perspective of the audience to which it was first addressed or whether it is evaluated in terms of its potential use by undergraduates in courses touching on population problems, there is no doubt that *The Population Dilemma* is a worthwhile book. The late Harold Dorn contributes a comprehensive summary of the history and prospects of world population growth. Irene Taueber examines the population explosion in Asia, Africa and Latin America. Joseph Fisher and Neal Potter are encouragingly optimistic as they review the resource potential which will be required to meet the needs of an expanding world. Frank Notestein, Dudley Kirk, and Sheldon Segal discuss some of the crucial concerns of biology and social science as these disciplines struggle to develop a more adequate understanding of the reproductive process.

Two papers in the volume are particularly noteworthy. Ansley Coale offers a fascinating analysis of prospective population growth and future economic development in the underdeveloped countries, according to various assumptions about the future level of reproduction. In this paper, Coale once more demonstrates his skill in using formal demographic analysis to clarify otherwise confused issues of economic policy and to do so in a style that is at once interesting to the scholar and illuminating to the layman. Donald Bogue's paper deals with the United States' own population explosion. His review of recent demographic developments in this country and their implications for the future structure of the American population is permeated by a deep-felt sensitivity to problems of poverty, social discrimination, and urban growth. It is heartening to read a demographer who is aware that many population problems can be found right here in this country and who also recognizes that demography as a science has a responsibility to work with other fields of sociology in tackling the social problems of American society today.

The principal flaw in this book is its title, which is inconsistent with the arguments set forth by the authors. None of their papers can be used to support the proposition that the alternatives facing nations today for dealing with actual or prospective overpopulation are equally undesirable. On the contrary, the authors, and editor Philip Hauser, emphasize that the only viable population policy is a radical, sustained program, financed by national governments, international organizations, and private foundations, directed to the control and further limitation of human reproduction. Why, then, speak of "the population *dilemma*"?

ROBERT GUTMAN

Stanford University

Action Research and Its Importance in an Under-developed Economy. By RAM DAS. ("Planning Research and Action Institute Publication" No. 282.) Lucknow, India: Planning Research and Action Institute, 1963. Pp. ii+74.

This book describes the objectives and activities of the Planning Research and Action Institute of the state of Uttar Pradesh in North India. This Institute is unique in India in that it bridges the gap between fundamental and applied research and works as a transmitter of extension methodology and pilot projects to the village people. The author, who is the director of the Institute, is full of optimism and hope for its future and prescribes similar institutes for other states of India.

If Action Institutes provide the proper mechanism for establishing a successful link between the plan and the people in an underdeveloped country, their functions must be clearly identified. Some of the general functions suggested in passing by the author are: (*a*) providing workable solutions for governmental and other agencies, (*b*) co-ordinating research of various kinds in a multidisciplinary framework, (*c*) seeking solutions for people's felt and latent needs, (*d*) communicating strategic solutions arrived at by the Institute to agencies and people, (*e*) creating confidence in people for these solutions, and so on. Given this comprehensive outlook, the Action Institute may become the laboratory for any and all problems faced in development work and nation-building. While some may agree with this generalist approach, others may have honest doubts about its long-range success or scientific objectivity. In fact, the Institute did take on additional tasks of training personnel

and producing audio-visual and other communication materials, which, if I read the author correctly should lie in the field of applied research. The boundaries of action research and applied research remain very inchoate throughout the discussion in this book.

Some of the author's recommendations are value-impregnated and rather controversial. Examples: "By stages individual interests have to be relegated to the background and cooperative action has to be stimulated" (p. 9). "In some cases whenever the full implications of a new project are not clearly known it is advisable to run it through the Government agency exclusively and when successful results are available people should then be involved" (p. 13). Why should the prestige of the Government agency be given lower valuation than that of the research agency? The suggestion concerning the setting up of pilot projects on national integration (p. 54) smacks of too much pilotism without knowing facts and conditions of take-off. The line chart for a good action research agency again suggests a battle array, what with its pilot project wing, evaluation wing, etc., which, in the judgment of some, need more sober reflection and temperance than speed and haste. Whatever reservations one may have on such matters, it is good to read about the experience of eight years of solid work of the Action Institute in a pioneering field. One wishes only that the author had given some examples of failures along with the successes and had provided some case materials on how crucial solutions were arrived at in specific instances.

Baidya Nath Varma

City College
City University of New York

Vivre dans les grands ensembles. By René Kaës. Paris: Les Éditions Ouvrières, 1963. Pp. 341. New Fr. 11.40.

Whereas research into the phenomena of suburbia has flourished in North America during the fifties, European contemporaries have dealt with problems of industrial relations, co-determination, or the relations between man and machine.

In North America, problems of suburbia have already reached the level of paperback fiction dealing with either the agonies of life in split-level traps or the love life of lonesome young matrons. To sociologists, many of the questions raised by the exodus of the white, urban population into the suburbs are already passé. In the light of President Johnson's war on poverty and the available research funds, they have rededicated themselves to the exploration of the inner core of our metropolitan areas while living comfortably in their own middle-class communities.

In France, as in other parts of the European Community, sociologists have begun to ask questions about man and his life in the new suburbs. Two factors have contributed to the development of these suburbs, industrial decentralization and the development of new industries, especially in the petro-chemical field. The suburban expansion or the development of satellite cities has become a threatening reality. At the same time, the sociological characteristics of the suburban population have changed profoundly. In former times, *faubourgs* were synonymous with proletarian ways of life. Workers, unable to find housing in the city, lived in *bidonvilles*—tin-can cities. Others were of the prewar commuter type living in a semirural environment, combining part-time farming with industrial employment.

The new suburbs and satellite towns have entirely different population characteristics. The inhabitants are not proletarians but white-collar workers and highly paid skilled workers, the new *petite bourgeoisie*. Although their style of life is on a more modest scale than in North America, it can be compared with the style of life in our suburbs. But in the field of housing, the comparison ceases to be of any value. European housing is scarce and dreams are on a much more moderate scale than in the United States. Typical suburbs consist of apartment-type housing units rather than single-family dwellings. A young couple are lucky if they can afford a two- or three-unit apartment. Within a short time, young families are living under crowded conditions again when children enter the scene.

Apart from housing, however, life is similar to America's suburbia. There are the usual worries about standards of workmanship, the problems of sending children to schools, the development of community services and churches, the planning and organization of

recreation. The new suburbs are ethnically more heterogeneous than the older cities, with people from different regions and nonmetropolitan areas of France mingling for the first time on a neighborhood basis. There are people of all faiths in the suburbs, trying to make religion a meaningful component of their lives. The concept of the neighborhood church seems to emerge in these suburbs, a concept unknown in France up to this time. What has been gained in greater heterogeneity and has facilitated a breakdown in parochialism has been lost in terms of age and class segregation. The older areas at least represent the full life-cycle from childhood to old age; the new suburbs select only the young and gainfully employed families. A city without mothers-in-law might provide an attraction to a young bride, but it becomes cumbersome when babysitters have to commute into the suburbs. Young teenagers are equally unavailable for these chores, as they live in the older communities. Segregation along class lines in Europe has not necessarily followed ecological theories developed in North America. In the new suburbs, however, spatial rather than social segregation is practiced. There are no problems of extreme poverty or invidious comparisons with great wealth. Competition with the Joneses has become possible.

Kaës mentions the prolonged absence of the main breadwinner between 7 A.M. and 7 P.M., an absence which is different from the traditional French pattern. In the reviewer's opinion, this fact will have a profound influence upon the lives of the young children growing up in the *grands ensembles*. As in suburbia in America, where the center of family authority is shifting to the *mater familias*, a shift in authority from the father to the mother will become evident. It will become increasingly difficult to maintain traditional family roles among the inhabitants of the *grands ensembles*.

Kaës gives us an exhaustive survey of the legal and social ramifications of the new suburbs. As frequently happens in a survey of this kind, specific hypotheses to serve as guidelines of investigation are lacking; value judgments serve as the basis for evaluation. Another shortcoming is the lack of cross-cultural comparison. There is extensive material about the new towns of England, German *Siedlungen*, and suburbia in the United States. The experiences in these countries as well as others could have validated or refuted some of the value judgments. The otherwise exhaustive bibliography is somewhat parochial. In a total of 186 references, three deal with descriptions of urban development in other countries, and three foreign-language sources are mentioned.

However, Kaës's publication will be of great interest to those observing the rapid patterns of suburban development in Europe.

RUDOLPH A. HELLING
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Praca i Kolezenstwo ("Work and Comradeship"). By ALEXANDER MATEJKO. Warsaw: Wiedza Powszechna, 1963. Pp. 443. Zloty 30.

During the twenty years since World War II, Poland has been transformed into a modern industrial nation. This process of transformation has brought about a complex new social structure, particularly in industry. This new social structure constantly requires conscious intervention on the part of management to adjust optimally labor with the growing technology, thereby reducing conflict between various elements of the industrial structure.

In the face of these new industrial conditions, the need for industrial sociology in Poland has become very great. Unfortunately in Poland the subject of industrial sociology has not received adequate attention; consequently, problems of human relations in the large-scale industrial plants have not been adequately studied.

In this book, which is intended for practitioners, Matejko surveys some of the most pertinent problems of industrial sociology. He gives a very comprehensive and interesting presentation of what has been done in research and method in this field in the United States and the Soviet Union and outlines his own research.

One of the most interesting problems exposed in this book is that of alienation in industralized society. The author's approach to the problem is not aprioristic. By use of Marxist method, he tests the principles with facts. He points out that the socialization of means of production in fact eliminates the sources of alienation of the workers, but this

does not mean the total removal of alienation. Moreover, what is needed is the changing of the roles of the workers in such a way as to implant in the workers the consciousness of ownership; in other words, the workers must be conditioned to feel that they own the means of production. For this reason, the author emphatically stresses the need for overhauling both the formal and informal social structures to achieve this end. The author sees the solution of social alienation in this approach.

An extensive bibliography and an English summary are appended to the book.

JAN JAROSLAWSKI

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Socjologia przemystu ("Industrial Sociology in the United States"). By ALEXANDER MATEJKO. Warsaw: Panstwowe Wydawnictwo Naukowe, 1962. Pp. 283. Zloty 40.

Polish sociology, following its tradition, seeks to avoid isolation from developments in sociology in other countries and strives to keep abreast of recent trends in the field. It is particularly interested in the fields of sociology in which it is weak, especially industrial sociology.

Matejko studied industrial sociology in the United States for two years, and the fruit of his study is this book which describes the scope and methods being used in American industrial sociology. The author's intention is to introduce to Polish sociologists what has been done already in industrial sociology in industrialized countries. This introducton has not only theoretical but also practical significance. The author presents the important works of American authors in this field and notes the present trends and controversial problems among the various schools. He presents his own analysis of the social and economic factors related to the rise of this branch of sociology in the United States. The book contains a comprehensive survey of works in industrial sociology.

However, the greatest problem that the author presents is the question of to what extent these research findings and works of American industrial sociology can be used in Polish sociology and, particularly, can be applied in practical situations in Poland, in view of the fact that American industry has developed in an entirely different social context from that of Poland for very obvious reasons. Matejko writes:

> The sociological problems of socialistic economy are so different from those of capitalistic economy, that the achievements and methods of American industrial sociology must be considered with criticism if we wish to profit from it in developing our own industrial sociology. That criticism, however, should not be synonymous with rejection of that which is progressive and creative in American social science. This science, for example, affords considerable data on the theory of worker alienation in modern capitalist industry, the creation of a quasi-cartes distance among different groups of workers, resistance to technical progress, social conditions damaging worker motivation, consequences of bureaucracy in the formal organization of enterprises and other topics.

The author raises here a most fundamental question, but he does not try to present an answer.

The book contains a bibliography of relevant materials and a concise summary in English.

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The New Metropolis in the Arab World. Edited by MORROE BERGER. New Delhi: Allied Publishers, 1963. Pp. 254. Rs. 12.50.

The new Arab metropolises are actually some of the oldest cities of the world. With the independence of the Arab countries, they have grown in recent decades into great population centers and taken on, or expanded, metropolitan functions of trade, industry, finance, and transportation. Cairo, with more than three million inhabitants, and Baghdad, with about one million, indicate the size these cities are reaching and their political, economic, and cultural importance in the modern Arab nation-state.

In this volume, a collection of papers read at an urban seminar held in Cairo, Arab planners, architects, and social scientists consider the problems now assailing these cities. They deal mainly with social and physical problems of the cities: extensive slums, substandard housing, overpopulation, industrial growth, traffic congestion, inadequate public services and utilities, and suburbs. As planners they are concerned with possible solu-

tions for them through master planning, subdivision control, zoning, traffic control, improved housing, and control of industrial location and population densities.

As this enumeration of topics reveals, Arab planners have assimilated to some extent modern planning principles from the Western countries. They aspire against formidable odds to apply them to their cities. On the one hand, they contend with cities which contain confusing mixtures of new and old buildings, systems of streets, and residential, commercial, and industrial areas, and whose economies are unable to employ the hordes of impoverished, illiterate rural migrants who flock to them. On the other hand, they lack authority, funds, and personnel to plan effectively or to relate urban with regional and national planning.

While the planners hope to guide the future growth of the metropolises, they want also to preserve the character of the old Arab city in them. They are well advised, of course, to adapt planning principles to the nature of their own communities. But a knowledge of what the old Arab city was does not emerge clearly from their discussion.

Several authors assert that the old Arab city resembled the medieval European city physically and designate as comparable features the mosque; minaret; church square; market; baths; residential, commercial, and artisan quarters; and walls. According to them, Jerusalem retains this appearance today.

But as urban communities Arab and medieval European cities were dissimilar and their social resemblances relatively few. Unlike European cities, the Arab cities were never complete communities with their own officials, courts, economic systems, institutions, and social life. Instead their inhabitants were segregated into ethnic and cultural groups which were nearly self-governing but subject to higher political and religious rulers, and which had guilds and religious orders as their principal organizations.

These papers represent a modest but promising effort to understand these complex, fascinating cities of the Near East.

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Some Aspects of Women's Ambition[1]

Ralph H. Turner

ABSTRACT

Correlational analysis shows that several discrete measures of ambition express two latent types of ambition for both men and women, but are more highly differentiated in the case of women than men. Material ambition approximates one type, and women's career and men's eminence ambition the other. Degree of difference between level of men's and women's ambitions arrays on the same dimension. Since career- and non-career-choosers, with minimum acceptable husband's occupation controlled, do not differ in level of material ambition, the conclusion is suggested that women expect to leave the extrinsic rewards to the husband, while seeking intrinsic satisfactions from career and education. Comparisons between career-choosers and non-career-choosers with respect to value indorsements and sociometric characteristics round out a consistent picture.

Sociological thinking about the nature of ambition has generally been geared to the situation of men rather than women. A man works toward some goal that the sociologist can locate on the stratification grid; it can be related to his starting point in order to measure mobility; and its components such as education, money, and occupation can be tested for consistency. The term "ambition" is appropriately used because many features of his life are focused about an active pursuit of a particular station in society. Because of this focus, specific measures of ambition can be used interchangeably for many purposes.

When women's ambition is discussed or measured, it is usually by direct but questionable analogy. Most frequently, women are asked about their educational and occupational goals. But the relative concentration of women college students in majors which are not markedly vocation-related makes the interpretation of gradations in educational ambition in the same terms as for men a doubtful procedure.[2] And the applicability of serious career patterns to only a fraction of all women makes occupational ambition plainly non-comparable between men and women.[3]

Our objective in this paper is to shed some light on the character of women's ambitions, by making use of data regarding 1,441 high-school senior women in Los

[1] These data were collected under a grant from the Social Science Research Council and analyzed with assistance from the U.C.L.A. Research Committee and National Science Foundation, using facilities of the Western Data Processing Center. David Chandler assisted in the statistical computations and made valuable substantive suggestions. Melvin Seeman painstakingly read and criticized a draft.

[2] Subject-matter fields in which degrees were awarded in 1961–62 are reported, by sex, in U.S. Office of Education, *Digest of Educational Statistics* (Washington, D.C.: Government Printing Office, 1963), p. 70.

[3] See, e.g., Rose K. Goldsen, Morris Rosenberg, Robin M. Williams, Jr., and Edward A. Suchman, *What College Students Think* (Princeton, N.J.: D. Van Nostrand Co., 1960), pp. 46–59.

Angeles. Measures of several types of ambition were secured for both women and men students, and gross frequencies and relationships with socioeconomic background have been reported elsewhere.[4] The data were secured in questionnaires administered by the investigator and his assistants under carefully controlled classroom conditions. Ten high schools were selected so as to provide a sample which would be socioeconomically representative of the central metropolis.

THE PROBLEM OF WOMEN'S AMBITION

There are two broad ways in which women's ambition can pose a different problem in measurement from men's ambition. First, the nature of ambition may be different in women, perhaps in degree, in goal, and in means to the goal. Second, women's ambition may be more differentiated or segmented, so that a typical individual simultaneously pursues different ambitions which have little relationship to one another. An exploration of these two kinds of distinctiveness will set the stage for the consideration of data.

The nature of ambition.—There is recurring evidence that sex of respondent is a pervasive variable affecting behavior and attitudes of many sorts. Carlson and Carlson observed that twenty-two out of thirty-two studies in the *Journal of Abnormal and Social Psychology* from 1958 to 1960 showed significant differences in response for males and females.[5] They warn investigators against the danger of using only male subjects in experiments, and of failing to keep separate the responses of males and females.

W. E. Vinacke and his associates have called attention to the different patterns of performance by men and women subjects in competitive game experiments.[6] From a series of experiments has come the formulation of an ideal-typical *accommodative* strategy for women, contrasted to *exploitative* strategy for men. Applied to coalition formation in a three-person group, accommodative strategy involves offers to form triple coalitions when two-person coalitions are needed to win, division of prizes equally rather than according to bargaining strength, altruistic offers in which one player suggests that the other two form a coalition to her disadvantage, and similar steps. Procedures of this sort serve to modify the character of the game away from the masculine strategy of exploiting others in order to win.

There is consistent evidence that the ambition of women in the socioeconomic realm is lower than that of men. Although women make better records in high school, fewer aim to graduate from college and still fewer to earn graduate degrees.[7] The ambitions of women are constricted to a few

[4] Ralph H. Turner, *The Social Context of Ambition: A Study of High School Seniors in Los Angeles* (San Francisco: Chandler Publishing Co., 1964).

[5] Earl R. Carlson and Rae Carlson, "Male and Female Subjects in Personality Research," *Journal of Abnormal and Social Psychology*, LXI (February, 1960), 482–83.

[6] Thomas K. Uesugi and W. Edgar Vinacke, "Strategy in a Feminine Game," *Sociometry*, XXVI (March, 1963), 75–88.

[7] On superior secondary-school performance of girls, see G. R. Johnson, "Girls Lead in Progress through School," *American School Board Journal*, CXV (1937), 25–26; and Robert S. Carter, "How Invalid Are Marks Assigned by Teachers," *Journal of Educational Psychology*, XLIII (April, 1952), 218–28. Only 38 per cent of degree-credit enrolments in U.S. institutions of higher learning in the fall of 1962 were women, as compared with 51 per cent of high-school graduates for 1961–62 (see U.S. Office of Education, *op. cit.*, pp. 41, 58). Sixty-two per cent of girls who graduated from Los Angeles high schools in 1963, as compared with 70 per cent of boys, were attending college or junior college in the fall of the year (see *After High School* [Los Angeles: City School Districts, Evaluation and Research Section, 1964], p. 4). The 51 per cent of high-school graduates who are women constitute further evidence of the superiority of women in secondary schools, when compared with the 49.6 per cent of the population 15–24 years of age who are females (see U.S. Bureau of the Census, *Statistical Abstract of the United States: 1963* [Washington, D.C.: Government Printing Office, 1963], p. 24).

occupational categories, with few aspiring to the highest professions and business positions.[8]

Evidence from the Los Angeles high-school study, reported elsewhere, suggests a distinctive pattern of values associated with high ambition in women.[9] Indorsement of deferred gratification is associated with high ambition in men but not in women; indorsement of individuality is associated with high ambition in women but not in men. Deferred gratification suggests an active stance, with the individual shepherding his resources for the sake of achievement. Individuality suggests the concern with being conspicuous by being different, which in turn is necessary if the individual wishes to be noticed and selected by others. If the woman's ambitions depend upon her being selected by the right man, individuality plays a key part in the woman's ambition configuration.

The same study also showed that most of the values which were found to distinguish ambitious from unambitious boys also distinguished men from women. Association of "feminine" values with low ambition and "masculine" values with high ambition suggests the extension of Vinacke's *accommodative* and *exploitative* strategies from the game situation into the world of socioeconomic stratification.

It is not easy to say, however, whether women aspire less highly in the socioeconomic realm than men, or whether their lower scores on conventional indicators merely reflect somewhat different emphases in their ambition or the use of different means in the pursuit of similar goals. The evidence which makes individuality the feminine value counterpart to deferred gratification suggests a difference in means, though nothing is said about goals. It is also disquieting to find, in Vinacke's work, that the women's *accommodative* strategy actually led to victory over the male players in certain game situations.

Diversification of ambition.—In a recent paper, French and Lesser reviewed differences in findings regarding achievement motivation in men and women.[10] When male subjects are placed in situations which ostensibly test their intelligence and leadership, they consistently show heightened achievement-motivation scores. Results for women, on the other hand, are inconsistent, a finding which French and Lesser ascribed to the alternative goals available to women. A man's "one primary goal is almost universally success in his job area. . . . What spells achievement for a woman is somewhat less universal." The authors then present experimental evidence in which they initially distinguished women according to their *intellectual* and *women's role* value orientations, and employed corresponding arousal cues in the experimental treatment. Their findings are that the heightened achievement-motive scores occur for women when arousal cues correspond to their own value orientations. Women high in intellectual role orientation respond with heightened indications of achievement motivation when stimulated by intellectual cues; women high in the traditional homemaker role orientation exhibit heightened achievement motive when the cue is relevant to that role.

The problem of women's ambition is inherently more complex than that of men. First, there are some areas in which the girl normally sets and pursues an ambition in much the same fashion as a boy. Such is the case with education. Second, there are those areas in which a woman may or may

[8] Robert W. Smuts, *Women and Work in America* (New York: Columbia University Press, 1959), pp. 35–36 and *passim* concerning the occupational distribution of women.

[9] Turner, *op. cit.*, pp. 98–99. Suggestively similar are the implications of a finding that whereas Machiavellianism contributes to good grades in college for certain men, it is physical attractiveness which does so for certain women (Jerome E. Singer, "The Use of Manipulative Strategies: Machiavellianism and Attractiveness," *Sociometry*, XXVII [June, 1964], 128–50.

[10] Elizabeth G. French and Gerald S. Lesser, "Some Characteristics of the Achievement Motive in Women," *Journal of Abnormal and Social Psychology*, LXVIII (February, 1964), 119–28.

not choose to set and pursue an ambition of her own. When girls are asked for statements of occupational ambition there will always be some who have career goals in the same manner as boys, some who have occupational goals which are quite secondary in importance to their other goals, and some who genuinely have no such goal. And third, there are areas in which a woman may set goals which can be realized only through the efforts and achievement of a husband. Each of these areas poses difficult problems in the conceptualization and measurement of ambition.

In the case of education the women's ambitions can be measured in exactly the same fashion as men's, but the result is not necessarily interpretable in the same manner. A man's educational ambition should be viewed in two ways, as a goal desired for itself and for the achievement of a "cultured" style of life, and as a means toward high occupational and material status. The former aspect of education can be the same for the two sexes. But the latter aspect for some women will be the means to their own occupations and for others the means to securing husbands through whom to realize their other ambitions.

In dealing with women's own career ambitions we not only confront the problem that many have no such ambitions, but we must deal with the existence of pseudo-career or secondary career objectives. Talcott Parsons has remarked on the frequency with which women have careers that would place them much lower than their husbands if their own careers did in fact determine their social stations.[11] The wife of a professional man who herself works as a secretary, a laboratory technician, or in some other modest white-collar occupation thinks of her "career" in a rather different sense from her husband's career. The career in this sense supplies a supplementary rather than essential income, and is a serious diversion or a way of remaining alert and useful rather than the major determinant of either the woman's socioeconomic station or her style of life. While the decision to have a career is a component of ambition, and while the choice of career is likewise one facet of ambition, it appears unreasonable to treat either as the central goal about which other goals are arrayed. In E. T. Hiller's terms, occupation is the American male's key status,[12] but her own occupation is not usually the key status for a woman.

If the key status for most women lies in the husband's occupation, we might seek the core of women's ambition by examining the qualifications they seek in their husbands. However, the nature of marriage in American society and the cultural sentiment of love support a social norm opposing mate selection which is governed primarily by considerations of ambition. Mate selection primarily on the instrumental grounds of socioeconomic ambition which is realizable through the husband's successes is improper when marriage is viewed as an intimate relationship in which social-emotional[13] interaction supplies the crucial criterion of adequacy. The additional pattern of romantic love in courtship leads many girls to deny that marriage is in any sense a vehicle for their ambitions and to insist that they will marry entirely for love.[14] Hence, while the husband's occupation may be the principal medium through which a woman achieves whatever ambitions she has, the ambitions are not likely to be either so clearly recognized, so specifically set, or so actively pursued as those of the men. As ambitions they are

[11] "The Social Structure of the Family," in Ruth N. Anshen (ed.), *The Family: Its Future and Destiny* (rev. ed.; New York: Harper & Bros., 1959), p. 265.

[12] *Social Relations and Structures* (New York: Harper & Bros., 1947), pp. 339–43.

[13] Robert F. Bales, *Interaction Process Analysis* (Cambridge, Mass.: Addison-Wesley, 1950).

[14] In an earlier pilot interview study with college girls, a large number objected to the direct question, "What occupation do you want your husband to have?" indignantly insisting that they would marry for love, not for their husband's occupation.

stifled to varying degrees by the attitude that a woman should passively accept the station in life her husband brings her rather than actively striving for a given station as a man is urged to do.

If the husband's occupation is woman's key status, then it should be the primary vehicle through which a material level of living is sought. But it is appropriate to ask whether her other ambitions are for more of the same, or for different goals. Is her educational ambition both a direct and indirect means toward high socioeconomic status, or is it aimed at satisfactions of a different kind? In seeking a career does the woman hope to add to the scale of living which her husband's occupation can bring her, or does she seek something else?

Questions such as these will be difficult to answer. But hints can be sought through two sorts of operation. First, a search for pattern in the intercorrelations of women's various forms of ambition should help to show whether their various ambitions apply to the same goals as their husbands' occupations, or whether they point toward alternative satisfactions. Second, examination of the patterns of ambitions which characterize girls who choose to have careers, as compared with girls who do not, will shed some light on what goals are to be realized through a career.

DISTRIBUTIONS OF AMBITION

The measure of ambition employed, the levels observed, and comparative data for men will be briefly reviewed as a prelude to the main analysis. In most instances the levels are not directly comparable between men and women. Previous experience had led us to doubt the usefulness of questions directed toward high-school and college women which asked for positive statements of intention or expectation regarding husband's occupation, education, and material standard of living. Hence the expedient of asking for acceptable minimums was employed—with good success as judged by obtained correlates of ambition.

Two measures of educational ambition were secured. Each woman answered the same question as the men: "Eventually, how much more schooling do you expect to get after you finish high school?" In addition, she answered the question, "Would you be a little disappointed if your future husband didn't have a certain amount of education? Check either 'yes' or 'no' for

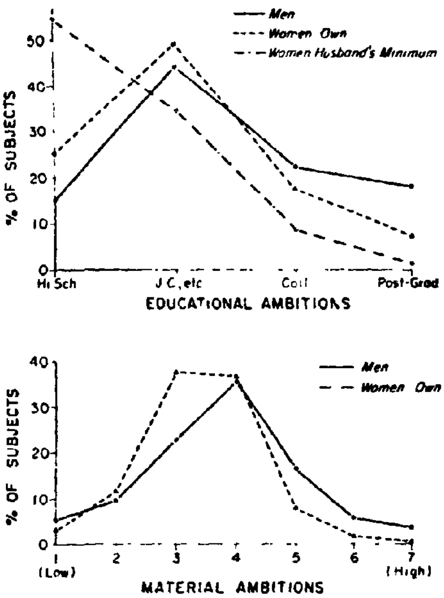


FIG. 1.—Educational and material ambitions

every item." There followed seven items, from no schooling to college graduate. These measures, with educational expectations for the males, are summarized in Figure 1. The percentage of respondents endorsing a given level of ambition is indicated on the vertical axis, with levels of ambition along the horizontal axis.

Level of material aspiration was stated in the same form for men and women, to permit comparison (Fig. 1). "Will you feel a little disappointed if—in your whole life—the best you can afford is—?" Eight alternatives followed, ranging from "a one-room house and a fifteen-year-old car," to

"several large houses and several new top-priced cars."

Occupation of the husband was asked both as a minimum and as an ideal. "Would you feel a little disappointed to have your husband spend his life as a ——?" was followed by a representative set of ten occupations. There followed an open-ended question, "What kind of occupation would you *like* your future *husband* to have?"

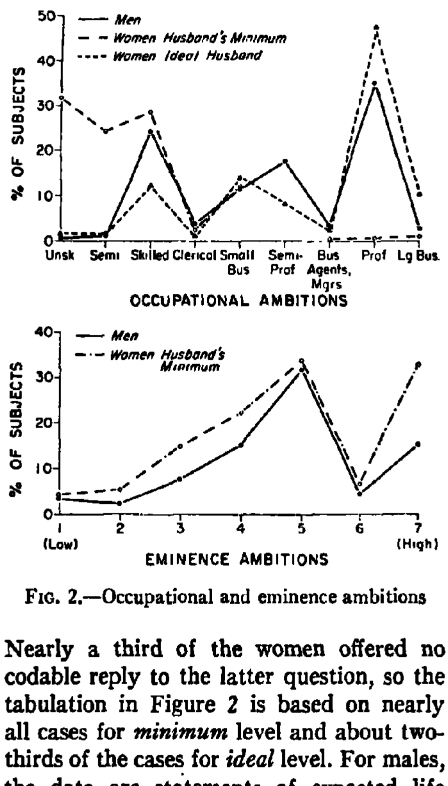


FIG. 2.—Occupational and eminence ambitions

Nearly a third of the women offered no codable reply to the latter question, so the tabulation in Figure 2 is based on nearly all cases for *minimum* level and about two-thirds of the cases for *ideal* level. For males, the data are statements of expected life work.

A question regarding *eminence* ambition had been used for the men, with some interesting results. The aim was to measure concern for standing and accomplishment within the occupational category, as an alternative to striving for identification with a high-ranking occupation or class. Men were asked, "After you are in the occupation which will be your life work, when will you consider yourself successful enough that you can relax and stop trying so hard to get ahead?" The multiple-choice replies ranged from "when I am doing well enough to stay in the occupation" to "when doing better than everyone else in my occupation" and "never." The same question was reworded for women, to refer to their husbands: "After your husband is in the occupation that will be his life work, when will you consider him successful enough that he should relax and stop trying so hard to get ahead?"

Two tentative observations can be made regarding the distributions of ambition. First, women's ambition is lower than men's in the two respects which are directly comparable (education and material level). A rough measure of difference in level of ambition can be secured by dividing the graph in two at the point where two distributions cross, and summing the differences in percentages on either the right or the left of the dividing point. If there were no overlap in the distributions, the measure would reach the maximum of 1.00. By this measure, female educational ambition is less than male educational ambition to almost the same degree as female and male material ambitions differ (0.15, 0.16, respectively). Since, as we shall see later, the relationship of own educational to material ambition is different for men and women, the finding has some special interest.

Second, in three instances the women's statements of an acceptable minimum for their husbands can be compared with the actual ambitions of the male students in the same classrooms. Here we find the greatest difference in the case of occupational ambition (0.58), the least difference for eminence (0.20), and an intermediate difference for education (0.40). If these fairly substantial differences can be taken seriously, they support the view that women are less concerned with the specific occupation or even with the placement in a particular occupation-based stratum of so-

ciety than men. Their greatest relative emphasis is on standing and accomplishment within strata. In the technical sense suggested by Kingsley Davis, it may be that the balance of concern between prestige and esteem[15] is different for men and women, the latter placing relatively less emphasis on prestige. The intermediate position of education is to be expected, since it is an instrumentality in the attainment of both prestige and esteem.

The latter relationship can be stated in another fashion. Although the class system consists of more or less separable ranked strata, there is a great deal of overlapping with respect to style of life, scale of living, reputation, etc. Thus the lowest members of a given stratum will normally fall below the highest members of the next lower stratum in all important respects except for the stratum designation. These limited findings are consistent with a view that, relatively speaking, women are more concerned with standing on the attributes of station in life which constitute a continuum than they are with the specific stratum.

THE INTERRELATIONSHIP OF AMBITIONS

The most general procedure for examining the extent of differentiation among the forms of ambition and for locating clues to the meanings of specific types of ambition is to look for patterns in a matrix of intercorrelations. The pattern of interrelationships could suggest a relatively unitary ambition, measured with varying accuracy by each specific index, or a few major types of ambition, or that each index constituted a form of ambition that is relatively unrelated to any other.

Seven ambition measures have been intercorrelated. Because of the lack of cardinal numbers, the ρ or Spearman rank-order coefficient was used throughout.[16] Because the *eminence* measure does not correlate to an appreciable degree with any of the other measures, it has been omitted from Table 1. Separate matrices are presented for the women who choose to have careers and for those who do not.

The problem at hand might best be resolved by factor analysis, except that the number of variables and the ordinal correlations would magnify errors in the use of such procedures to an excessive degree. Hence we have followed the simpler procedure of rearranging columns and rows in a search for pattern. With the exception of *ideal* husband occupation, the matrix for career-seekers fits perfectly a pattern suggesting that two distinct underlying types of ambition are being measured. The variables have been arranged in Table 1 according to this pattern.

The principle underlying this pattern is simply that coefficients should decrease in size as their distance from the diagonal increases. The attainment of such a pattern indicates that adjacent variables are relatively similar to one another and distant variables are dissimilar. Variables falling toward the ends describe best the underlying types of ambition. The pattern can be achieved in a two-dimensional matrix only if placement along a single axis will suffice to describe the major relationships among the variables.[17]

In this case, we find that material ambition is near one pole and the specific choice of career near the other. Thus the level of material ambition and the level of career ambition have nothing in common. The girl's own educational ambition has much in common with her choice of career, but is not quite so far removed from her material ambition. The minimum husband occupation has most in common with material ambition, but is not quite so far removed

[15] Kingsley Davis, *Human Society* (New York: Macmillan Co., 1949), pp. 93–94.

[16] Sidney Siegel, *Nonparametric Statistics for the Behavioral Sciences* (New York: McGraw-Hill Book Co., 1956), pp. 202 ff. This analysis applies to the native white "anglo" subjects only, though spot checks indicate similar patterns for the whole sample.

[17] The procedure is an adaptation from a method developed for use in archeological research (see William S. Robinson, "A Method for Chronologically Ordering Archaeological Deposits," *American Antiquity*, XVI [April, 1951], 293–301).

from the girl's choice of career. The girl's designation of a minimum acceptable education for her husband is something of a bridge between the extreme foci of female ambition. This pattern demonstrates in comprehensive fashion the bifurcation of women's ambition. And it shows clearly that the differentiation between material and non-material goals corresponds with a difference between values sought through the husband and by the girl's own effort.

educational ambition is "pulled away" from the more strictly socioeconomic goals only because it must serve as a means toward attainment of a career goal, in which case it should not be so sharply differentiated from the minimum husband occupation and material level among the non-career girls. On the other hand, if the pattern persists, the idea of an underlying polar continuum of ambition will be more impressive.

TABLE 1

RANK CORRELATIONS AMONG SIX TYPES OF AMBITION, FOR WOMEN

Type of Ambition	Material	Minimum Husband Occupation	Minimum Husband Education	Own Education	Own Career	Preferred Husband Occupation
	Women Planning To Have Careers					
Material ($N=546$)	......	0.41	0.39	0.17	0.08	0.08
Minimum husband occupation ($N=546$)	0.41	......	.51	.29	.27	.21
Minimum husband education ($N=546$)	.39	.51	......	.43	.33	.22
Own education ($N=546$)	.17	.29	.43	......	.65	.22
Own career ($N=484$)	.08	.27	.33	.65	......	0.20
Preferred husband occupation ($N=339$)	0.08	0.21	0.22	0.22	0.20	.....
	Women Not Planning To Have Careers					
Material ($N=563$)	......	0.42	0.46	0.22	......	0.27
Minimum husband occupation ($N=563$)	0.42	......	.49	.33	......	.33
Minimum husband education ($N=563$)	.46	.49	......	.47	......	.42
Own education ($N=563$)	.22	.33	.47	......	......	0.30
Preferred husband occupation ($N=374$)	0.27	0.33	0.42	0.30	......	......

The girl's designation of an ideal husband occupation bears a uniform modest relationship with all but material ambition. Perhaps the introduction of an ideal or fantasy element removes it from the polar continuum which orders the remaining forms of ambition.

The girls who choose only to be homemakers lack one of the polar measures of ambition, and it will be crucial for interpretation to see whether the remaining variables assume the same pattern as before.[18] It might be supposed that women's

While the fit is not quite so perfect as for the career-choosers, the best-fitting pattern maintains exactly the same ordering of the four measures of ambition, with material aspiration and the girl's own education at the poles. The *ideal* husband occupation remains outside of the main pattern, but is substantially more highly correlated with the other measures than it was among the career-choosers. Ideal husband occupa-

[18] The choice between career and homemaker roles is explained in the following section of this paper.

tion cannot be fitted into the correlation matrix in the place of the girl's own career. But the suggestion that more of what a girl values is vested in her *ideal* husband occupation when she herself does not elect to have a career may warrant further notice.

Although the patterning of women's ambitions is intrinsically interesting, it takes on full significance for our purposes when compared with the patterning for men (Table 2). Comparison will supply evidence bearing on the hypothesis that men's ambition is less differentiated than women's. In addition, similarities and differences of other sorts may afford clues to the interpretation of female patterns.

The various forms of ambition have much more in common for men, as demonstrated by the generally higher correlations in Table 2. The most crucial difference lies in the correlations between education and material ambition. For women, education is not an important instrumentality in securing an expected material level of living ($\rho = 0.17$); for men there is a substantial relationship ($\rho = 0.48$).

This difference cannot be discounted as an artifact of poorer measurement in the case of women, because the magnitudes of certain crucial relationships are quite comparable. Thus the relationship between a man's educational and occupational ambitions ($\rho = 0.72$) is similar to the corresponding relationship between the career-choosing woman's educational and career ambitions ($\rho = 0.65$). The same is true of the man's occupational and material ambitions ($\rho = 0.42$) as compared with the woman's minimum husband occupation and material ambitions ($\rho = 0.41$).

Although the range in magnitude of correlations is not so great for men, an effort was made to discover a pattern by the same procedures applied to the matrix for women. The best arrangement of the four variables produces a pattern marred only by a single trivial reversal. This order has been followed in Table 2.

Two features of the arrangement of variables are striking. First, the order corresponds with the order for women—material level, the man's occupation, and educational ambition. Second, the position in the arrangement which is occupied by the woman's career ambition is filled by eminence ambition for men. In spite of the closer clustering for men, apparently the same latent dimension organizes the forms of ambition for men and women. If career and eminence ambition are female and male counterparts in the pattern, consideration of the nature of eminence ambition may shed light on the nature of the women's career.

TABLE 2

RANK CORRELATIONS AMONG FOUR TYPES OF AMBITION, FOR MEN*

Type of Ambition	Material	Occupation	Education	Eminence
Material	. . .	0 42	0 48	0 35
Occupation.	0 42	.	.72	.55
Education.	48	.72	.....	0.66
Eminence .	0 35	0 55	0 66	. .....

* All coefficients are based on 1,000 cases Twenty cases were removed from the full sample of 1,057 native white "Anglo" men because 1 or more of the forms of ambition were coded "not answered," and the remaining 37 were eliminated with the use of a table of random numbers so as to stay within the capacity of the available computer program.

Eminence may be coupled with an ambition to rise in the socioeconomic scale, or it may be an alternative to mobility. It is not primarily a preoccupation with material reward, as indicated by the polarization. It is more closely related with education than with other types of ambition. The man's ambition normally combines a desire to achieve high status with a desire to achieve some recognition for the quality of role performance within his status. For many men the mere achievement of a high station in life and its translation into a high material level of living are empty without the complementary satisfaction of a job well done. In the same way, the woman's satisfaction with her station in life and corresponding level of living, which are anchored to her husband's occupation,

are often empty by themselves. But she is not usually involved in the performance aspects of her husband's occupation except in peripheral ways; and the separation is even more marked for the girl who is not yet married. For a considerable proportion of these girls, the career may convey a promise of the same complementary type of achievement which the man attains by striving incessantly to perform his occupational role with excellence.

TABLE 3

CAREER-HOMEMAKER CHOICE FOR WOMEN

Choice	Per Cent
Lifetime career	3.6
Homemaker	48.4
Both	47.9
Total	99.9
Total no.	(1,437)
No. not answering	(4)

THE CAREER GOAL

More light can be shed on the differentiation in women's ambition by a specific examination of the career goal, and some comparisons of women who wish to have careers with women who do not.

Career or homemaker?—If we are to determine whether the goals these high-school girls seek through careers are different from those they seek through husband status, it is important not to confuse career with the casual and temporary jobs which the majority of women hold at some time in their adult lives. The term, "lifetime career," was selected as a simple way of designating the serious occupational goal. The term "homemaker" was used in case any of the women had an aversion to the word "housewife." Expectation rather than wish or desire was requested because such wording had generally been found more satisfactory in aspiration research. The actual wording of the question was as follows:

Do you expect to
_____ (1) Have a life-time career?
_____ (2) Be a homemaker?
_____ (3) Both have a life-time career and be a homemaker?

The distribution of responses to the question appears in Table 3.

From the very small number naming a "career only" it is clear that very few of these girls think in terms of a choice *between* career and homemaker roles. The choice, on which they are evenly divided, is between having or not having a serious career *in addition to* their home role. Forty-eight per cent of the girls are undeterred by frequent discussions of the supposed difficulties in combining the two major roles.

Career in relation to educational and material ambition.—Does the girl's decision to seek a career reflect aspiration to higher social station or socioeconomic status than the choice of homemaking alone? Is the motivation described by Smuts applicable to high-school career aspirants, when he says: "Most of today's working wives show no strong internal commitment to work . . . they work mainly in order to earn money they don't absolutely have to have"?[19] The most direct way to answer this question is to see whether there are relationships between electing a career and measures of educational and material ambition. Chi-squares based upon cross-tabulation with the three career-homemaker categories show a highly significant association ($p < .001$) of career only with high educational ambition (career-plus-homemaker in intermediate position), but no relationship with material ambition. A result of this sort undermines the view that women choose the career role in order to achieve socioeconomic status.

A difference in emphasis can be measured by use of the *material-education polarization index* (MEPI). Material ambition and educational ambition were each converted to standard scores, the score for education was subtracted from the score for material ambition, and a constant was added to make all scores positive. The χ^2 indicates a highly significant ($p < .001$) relationship between MEPI and career choice, and when career-only and career-plus-homemaker categories are combined for compari-

[19] Smuts, *op. cit.*, p. 148.

son with homemaker-only, a biserial coefficient of —0.29 is secured. Women who want careers are likely to stress educational ambition more than material ambition; women who wish only to be homemakers are likely to stress material ambition more than educational ambition.

The irrelevance of material ambition for the choice between career and homemaking can be documented in an even more specific fashion. If women choose to have careers because of their ambitions for higher material standards than husbands alone can provide them, then the relationship of husband's and own occupational ambition to material ambition should be cumulative. Families in which the wife has a career are two-income families, and the total income should be higher than in a one-income family with the same husband occupation. If the prospect of this double income is important in deciding to have both career and homemaker roles, the girls who choose both should report higher material ambitions than girls who name similar minimum husband occupations but choose only the homemaker role. A glance at Table 4 reveals that this is not the case. There is a clear relation between minimum husband occupation and material ambition, but girls who choose both career and homemaker roles do not reflect the double income in setting their own material ambitions.

Values and career-homemaker choice.—Included in the questionnaire were thirty-one value items, selected to reflect several of the major sorts of values often thought to distinguish respondents from high and low socioeconomic backgrounds.[20] Comparison of those items associated with the career choice and those associated with level of ambition may shed further light on what is distinctive about the goals of girls who wish to pursue careers.

In Table 5 all items which were significantly associated with either career choice or ambition level are reported. The ambition index is a composite measure, based upon a score of 0–3 for minimum husband occupation, 0–3 for material ambition, 0–2 for own educational ambition, and 0–2 for minimum husband education. A smaller number of items are related to the career-homemaker choice as compared with general socioeconomically relevant ambition. To the extent to which the items used in this study can be taken as a sample of socioeconomically relevant values, the choice to have a career is less a reflection of class values than are ambitions which do not center on the career.[21]

TABLE 4

EFFECT OF COMBINING CAREER AND HOMEMAKER ROLES ON WOMEN'S MATERIAL AMBITION

Minimum Husband Occupation*	Mean Material Ambition: Homemaker Only	Mean Material Ambition: Career and Homemaker
Unskilled labor	2 95 (238)	3 01 (197)
Semiskilled labor	3.21 (179)	3 33 (159)
Skilled labor	3 67 (186)	3 67 (203)
Clerical and sales clerks	[4 00] (10)	[3 63] (24)
Small business owners-managers and salesmen	4 14 (76)	4 10 (86)

* Higher level occupations are not included in the table because the number of women specifying them was too small to produce stable means. The clerical and sales clerk category is included only to avoid a break in the table

The similarities and differences with respect to individual items fit a pattern of similar goal and dissimilar means. The career girls are not set apart by the use of individuality (items 14, 24, 25) as an adjunct to ambition through the traditional female role, as the ambitious girls are. But they also do not place emphasis on the more masculine virtue of self-reliance (items 22, 28). Likewise they do not share the moral opportunism (items 15, 21, 30) of the ambitious girls. However, they do

[20] Each value item was worded, "Which kind of person would you rather be . . . ," followed by two alternatives and a second question for intensity. The number of cases is reduced by about one-fifth, because some questionnaires were distributed with an alternative formulation of the value questions.

[21] It is consistent that the career-homemaker choice is also uncorrelated with measures of paternal socioeconomic level.

place the same emphasis on the pursuit of secular success (items 13, 21, 34, 37).

At the same time, the career girls do significantly more often state that they would prefer enjoyment of music, art, and books to making a very good living (item 9), suggesting a difference in the hierarchy of goals as well as means. It is notable that the career girls and ambitious girls choose business success over family (item 13) with equally great significance. It is only when the choice is for a "cultured" way of life that the career girls depreciate secular success.

This limited evidence suggests, in summary, that there are differences in both goal and means that set the career girl apart. The career girls place the "cultured" way of life higher in their value hierarchy. What means they favor for the pursuit of their goals is not clear; we see only that they do not distinctly employ the values of individuality, self-reliance, and moral opportunism, as the ambitious women seem to do.

Sociometric correlates of career-homemaker choice.—Although it is less directly relevant to assessing the character of ambition, sociometric evidence may add to the picture of the career girl. Students were asked to name three other students in their classroom whom they would like as long-term close friends, to identify two students as "big wheels," and to name two students who were best in their school work. Indexes based on proportion of possible choices *received*, adjusted and equated by schools, were computed. The indexes will be called "friend rating," "brain rating," and "wheel rating." Percentage analysis is summarized in Table 6.

TABLE 5

VALUES ASSOCIATED WITH AMBITION INDEX AND CAREER-HOMEMAKER CHOICE

Value Item*	Ambition Index	Career-Homemaker Choice
8. Can do a few things perfectly vs. can do many things fairly well	0.05†	0.01
9. Enjoys art, music, books vs. makes very good living	...	.01
12. Spends extra money on friends vs. saves extra money for future	.01	...
13. Real success in business vs. real family man	.001	.001
14. Shows people how to argue intelligently vs. smooths over disagreements	.001	...
15. Will tell small lie to help friend vs. sticks by truth	.001	...
17. Rather be own boss vs. will take orders to get ahead	.001	...
21. Smooth operator always comes out on top vs. too kind to take unfair advantage	.001	.05
22. Prides self on doing things on his own vs. likes advice and help from others	.02	...
24. Does many things better than friends vs. does most things as well as friends	.001	...
25. Tries to get group to do things his way vs. quick to go along with group	.001	...
28. Neither a borrower nor a lender be vs. often borrows-lends with friends	.01	...
29. Talks foreign policy and politics vs. talks popular music and sports	.001	.02
30. Laughs off his failures and mistakes vs. takes full blame for own failures	.05	...
31. Laughs off insult to his honor vs. never lets insult to honor go by	...	.05
34. Risks what he has to get ahead vs. prefers small but secure position	.001	.001
37. Always looking for something better vs. satisfied with what he has	0.001	0.01

* The alternative associated with higher ambition or choice of career has been listed first in each instance. There are no reversals of relationship.

† The probability that the observed associations could have occurred by chance in a random sample is less than the figure indicated in each instance. Associations were tested by chi-square, using two degrees of freedom.

Wheel rating appears to be unrelated to career-homemaker choice. If the girls who choose careers were thereby seeking social prominence, we should have expected that they would have converted such aspirations into social prominence in their own schools.

Brain rating, on the other hand, shows a small but significant relationship with career preference ($p < .02$, 2 degrees of freedom; $p < .01$ when "career only" and "both" are combined, 1 degree of freedom).

This finding fits the interpretation already placed on the relative preference for a "cultured" style of life, suggesting that the career girl has accepted academic values more fully than the girl who chooses only homemaking.

The relationship with friend rating is more complex. Girls who eschew the homemaking role altogether are the least desired as friends. Girls who are satisfied with homemaking alone are disproportionately a middle group—neither the most nor the least chosen as friends. The girls who wish

TABLE 6

SOCIOMETRIC INDEXES AND CAREER-HOMEMAKER CHOICE
(Per Cent)

Index Value	Percentages		
	Career Only	Home-maker Only	Both
Friend rating			
1, 2 (low)	36	27	33
3	42	38	32
4	12	24	20
5, 6 (high)	9	12	15
Total	99	101	100
Wheel rating			
1 (not wheels)	85	83	79
2-4 (wheels)	15	17	21
Total	100	100	100
Brain rating			
1 (not brains)	70	77	70
2-4 (brains)	30	23	30
Total	100	100	100
Total no.	(33)	(558)	(512)

to combine roles include disproportionately both extremes, the most and the least desired. The over-all relationship is of moderate significance ($p < .05$, 6 degrees of freedom), but the contrast between the homemaker-only group and the other girls is more firmly significant ($p < .02$, 3 degrees of freedom).

Type of career.—The specific careers intended by the 52 per cent of girls who expressed such ambition were identified in response to the following question: "If you plan on a lifetime career, what occupation do you think you will make your life work?" The responses are summarized in Table 7. Nearly 14 per cent of those who said they planned on a lifetime career were unable to name a specific occupation. Considering that these students are approaching graduation, and that the combined non-

TABLE 7

OCCUPATIONAL AMBITION OF CAREER WOMEN

Occupational Category	Per Cent
Unskilled labor	0 2
Semiskilled labor	1 3
Skilled labor	6.4
Clerical and sales clerks	26.4
Small business owners-managers and salesmen	0 6
Semiprofessionals	35.0
Business agents and managers	0.9
Professionals	29.1
Large business owners and officials	0.2
Total	100 1
Total no.	(640)
No. not answering	(101)

response and unclassifiable responses by men total only 1 per cent, it is clear that many of the girls who state that they plan to have a career have not taken their plans as seriously as have the men.[22]

The occupations named by women are concentrated in three categories the semiprofessions, the professions, and clerical work. Business management and ownership are almost entirely lacking, with women choosing almost exclusively the clerical phase of business. The three most popular groups of occupations are similar to those named by the men, except that the clerical category takes the place of skilled labor as the peak of ambition in the lower portions of the scale. Nearly two-thirds named one of the professions or semiprofessional occupations. The largest single

[22] For evidence suggesting that girls lack realistic knowledge of the occupations they have chosen, see Therese M. Rauner, "Occupational Information and Occupational Choice," *Personnel and Guidance Journal*, XLI (December, 1962), 311-17.

group, about 24 per cent, named occupations in the education-scientific professions. The other major groups were in the semiprofessions related to medicine and in the entertainment, art, and literature group. The choices correspond with the present concentrations of "women's occupations."

CONCLUDING REFLECTIONS

The investigation into women's ambition began with several queries. But the same answer crops up in reply to each. The fundamental differentiation of women's ambition, related to *direct* and *indirect* pursuit of goals imposed by the married woman's role, appears to be the key to the data at hand. The differentiated elements in women' ambition seem to be present in men's ambition; but because men pursue the entire range of ambitions directly, there is less structurally imposed separation among the forms of ambition.

Specific measures of ambition convey rather different meanings for men and women: educational and occupational ambitions are substantially related to material ambitions for men, but women's own educational and career aspirations bear little relationship to their material expectations. This observation should be an immediate warning to investigators never to attempt to compare the level of men's with women's socioeconomic goals or ambition by comparing levels of educational and career aspirations. Likewise, there are probably differences between career-and non-career-choosers, especially in the significance of an ideal husband occupation as a reflection of ambition. But the supervening observation is the uniform latent structure of ambition in men and in women, and in career- and non-career-choosers.

The underlying pattern of differentiation suggests—but does not exactly fit—the standard sociological distinction between prestige and esteem. Occupational level and *eminence* correspond well to prestige and esteem; but material rather than occupational level is the polar category opposite eminence in our data. *Extrinsic* and *intrinsic* rewards may supply a closer fit. Material reward reflects least the interest in effort and work for its own sake; eminence points most strongly toward striving for excellence within the occupation; educational ambition may be a better indication of dedication to achievement or accomplishment for its own sake than mere choice of occupation. The pole of intrinsic reward suggests an *achievement motive*, though no measure of this variable was included in the investigation.

A woman cannot seek both types of reward through the same activities, so long as she employs the traditional perspective in relating herself to the socioeconomic system—as these women do. With few exceptions, these women choose to *add* a special role rather than substitute for the traditional homemaker role. In adding this role they leave the extrinsic reward of material level of living entirely in its traditional place, as a function of the husband's efforts. But so long as they accept this traditional assignment, women cannot reap the intrinsic rewards of accomplishment that their husbands will in association with extrinsic returns.[23]

The general pattern which emerges is one in which educational ambition becomes more preponderantly a vehicle for the pursuit of intrinsic reward in women than in men. For girls who choose careers, the career carries this tendency even further.

A man's occupation normally offers both the intrinsic enjoyment of specific forms of activity and the extrinsic rewards, such as material comfort and social standing, which belong principally to level of occupation. But if the former is not part of the woman's perspective toward her husband's work and

[23] The evidence in this paper may offer a little support for the common assumption in stratification theory that "the family is the solidary unit of equivalent evaluation and that the husband's occupation is the principal unit of equivalent evaluation . . . ," which has been so ably challenged by Walter B. Watson and Ernest A. T. Barth (see "Questionable Assumptions in the Theory of Social Stratification," *Pacific Sociological Review*, VII [Spring, 1964], 10–16).

accomplishments, it is not surprising that she is willing to consider marriage to a man whose occupational level is low. Her *fantasy* about a husband's occupation seems to mean little when she invests intrinsic ambitions in her own career, but reflects some of her non-material ambitions when she does not have a career goal of her own.

If women plan to leave the extrinsic goal of high socioeconomic status to their husbands, it is not surprising that career-choosers are not set apart from other women with respect to some of the attitudes normally associated with high socioeconomic ambition. "Ambitious" men and women are likely to be identified as "big wheels" by their peers; but career-choosers show no such distinguishing evidence of interest in social climbing or influence. Nor do career-choosers differentially indorse values which correspond to the *means* for attaining high socioeconomic status, such as individuality and self-reliance. They do resemble the "ambitious" in valuing "success," especially when the choice is with security and adjusting to one's current situation. Thus, while the decision to have a career does not incorporate more of the same kind of ambition as the desire for high socioeconomic status, it does reflect concern with success—by different means and of a different sort.

The difference in kind of success sought through career is suggested by two kinds of evidence. Career-choosers are distinguished by their reputation for doing well in their schoolwork, though "ambitious" women are similarly distinguished. But they alone are distinguished by a preference for aesthetic and intellectual values in contrast with good living. If aesthetic and intellectual goals are more in the nature of intrinsic than extrinsic rewards from activity, then the two kinds of evidence point toward the same conclusion: (*a*) extrinsic rewards are sought through the husband's occupation and intrinsic rewards through the woman's own career; (*b*) women who value intrinsic rewards more highly are more likely to seek careers.

These conclusions can, of course, be offered only as plausible inferences. And they refer only to girls seventeen to eighteen years of age, looking in most cases toward a future which they are as yet in no position to see clearly. What transformations of motive and attitude will accompany experience in marriage and career are not indicated. But the decisions made in this Senior year of high school have far-reaching consequences. To the extent that the interpretations have been correct, they describe a structure which is likely to enter into the choice process and the initial experiences in marriage and career.

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Occupational Prestige in the United States, 1925–63

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ABSTRACT

A correlation of .99 between prestige scores derived from the 1947 North-Hatt-NORC study of occupational prestige and a 1963 replication of it indicates that very few changes in occupational prestige ratings have occurred in the sixteen-year period. Cautioned by the recognition that the North-Hatt-NORC list of occupations is not a very representative or extensive sample of occupations, one can marshal evidence from other studies dating back to 1925 which does not invalidate the view that no appreciable changes in the prestige structure of occupations have occurred in the United States in the last four decades. While over the entire period, 1925–63, no systematic trends can be detected in the prestige of particular occupations, it is possible to show that systematic, though small, changes were occurring between 1947 and 1963. Scientific occupations were increasing in prestige, culturally oriented occupations were falling, and artisans were enjoying a mild upward trend. Nevertheless, the overriding conclusion must be that the structure of occupational prestige is remarkably stable through time as well as space.

The research reported in this paper represents an attempt to add historical depth to the study of the prestige of occupations in the United States. It reports mainly on a replication conducted in 1963 of the National Opinion Research Center's well-known 1947 study of the prestige positions accorded to ninety occupations by a national sample of the American adult population.[1] We also deal with several fragmentary earlier studies, which together with the two main NORC studies, provide a rough time series going back to 1925. Since the two NORC studies were not replications of the earlier ones, we shall dwell mainly on change and stability in the prestige of occupations during the period from 1947 to 1963.

The prestige hierarchy of occupations is perhaps the best studied aspect of the stratification systems of modern societies. Extensive empirical studies have been undertaken in a variety of nations, socialist and capitalist, developed and underdeveloped. Intensive analyses have been undertaken of results of particular studies searching for the existence of disparate prestige hierarchies held by subgroups within nations.[2] Despite rather extensive searches conducted by a variety of techniques, it appears that occupational-prestige hierarchies are similar from country to country and from subgroup to subgroup within a country. This stability reflects the fundamental but gross similarities among

[1] The replication was undertaken as the first stage of a larger project supported by a National Science Foundation grant (NSF G85, "Occupations and Social Stratification") aimed at providing definitive prestige scores for a more representative sample of occupations and at uncovering some of the characteristics of occupations which generate their prestige scores. The replication was undertaken as the first step in the research program to determine whether appreciable shifts occurred in prestige scores in the time period 1947–63 so that the effects of improvements in technical procedures could be sorted out from effects of historical changes in any comparisons which would be undertaken between the 1947 study and the more definitive researches presently under way.

[2] See, e.g., Kaare Svalastoga, *Prestige, Class and Mobility* (Copenhagen: Gyldendal, 1959), pp. 43–131; C. A. Moser and J. R. Hall, "The Social Grading of Occupations," in D. V. Glass (ed.), *Social Mobility in Britain* (London: Routledge & Kegan Paul, 1954), pp. 29–50; and Albert J. Reiss, Jr., Otis Dudley Duncan, Paul K. Hatt, and Cecil C. North, *Occupations and Social Status* (New York: Free Press of Glencoe, 1961). The last-mentioned volume contains the major analyses of the 1947 North-Hatt-NORC study of occupational prestige.

the occupational systems of modern nations. Furthermore, knowledge about occupations and relatively strong consensus on the relative positions of occupations are widely diffused throughout the populations involved.

The consensus within and among populations on the prestige positions of occupations leads one to expect that there will be considerable stability over time in the positions of particular occupations. Industrialization has proceeded to different points in the several countries whose prestige hierarchies have been studied without seriously affecting the relative positions of occupations in the countries involved. Cross-sectional comparisons between different countries at different stages of industrial evolution suggest that it would be erroneous to expect any considerable change in the *prestige* structure of a single country over time, even though that country might be experiencing appreciable changes in *occupational* structure. We can only expect to observe changes on the order of those previously found between two nations at different stages of economic development.

On the other hand, there are cogent reasons for expecting that changes in occupational structure will be reflected, at least ultimately, in corresponding changes in the prestige positions of occupations. The prestige position of an occupation is apparently a characteristic generated by the way in which the occupation is articulated into the division of labor, by the amount of power and influence implied in the activities of the occupation, by the characteristics of incumbents, and by the amount of resources which society places at the disposal of incumbents. (Other factors are undoubtedly at work, but these are the most obvious.) Hence, as occupations shift in these respects over time, corresponding adjustive shifts in prestige positions can be anticipated.

Considerable changes have occurred since 1947 in the occupational structure and labor force of the United States. The long-term trend in the growth of professional and scientific occupations persisted and was even accelerated during this period. Governmental and popular concern over the numbers and quality of our professional and technical manpower was expressed in a great expansion of our universities as well as in more attention being given lower levels of schooling. The proportion of the labor force devoted to agricultural pursuits declined along with unskilled and heavy labor components. This was also the period during which automation continued to expand, raising a serious question as to whether the American labor force could absorb both workers freed from jobs eliminated by technological progress and the large cohorts of postwar births now beginning to enter the labor force. Mention must be made of the stepped-up drive for equality on the part of Negroes, although we cannot tarry here to examine it. The question at issue is whether changes in the occupational structure have been reflected in shifts in the prestige of occupations between the two points in time.

On the basis of our empirical knowledge concerning the stability under a variety of conditions of the hierarchy of occupational prestige, we can support an expectation that there will be relatively few changes in the positions of occupations as we proceed from the 1947 to the 1963 study. On the basis of what seems to be a reasonable model of how these prestige positions have been generated, we expect somewhat more in the way of changes. Neither point of view produces very precise expectations for we need to know what is an acceptable level of stability (or change) either to conform to or to negate each expectation.

One further problem plagues interpretation of any comparisons such as this study envisages: Consider a set of occupational titles for which we have an aggregate prestige rating at two points in time; the difference between these ratings can be attributed either to a general increase in the amount of prestige in the occupational system or to an increase in the prestige of

the aggregate of occupations in the set and a corresponding decrease in the prestige of some occupations not in the set. There is no conceivable way of choosing between these interpretations with the present data.

In view of the large number of professional occupations included in the NORC list, it may well be the case that in the aggregate the ninety occupations stood higher in the prestige hierarchy in 1963 than in 1947. If prestige is regarded as a "commodity" that behaves like the payoff in a "zero-sum" game, then, to be sure, what one set of occupations gains another must lose. But the NORC titles might get higher ratings in 1963 than 1947 because there is, all told, a greater amount of prestige in the system. If the latter is the case, the ninety NORC titles may get higher ratings and at the same time a smaller share of all prestige and a lower place in the total prestige hierarchy.[3]

These remarks are perhaps sufficient to alert the reader to the ambiguities which characterize the study of occupational prestige. Indeterminacies encountered in the study of a set of occupations are, of course, duplicated when the focus is upon a single occupation. It is for this reason that our focus is largely on the ordering of the ninety NORC occupational titles in two time periods and not upon changes in the prestige of particular occupations. All indications of changes in occupational prestige revealed here are of necessity relative to the set of ninety titles under consideration. These occupations exhaust our universe, and changes in their prestige are assumed to indicate restructuring of the relative prestige of the occupations under consideration.

[3] This point is perhaps more clearly illustrated with a more familiar commodity: money income in dollars. It is fairly easy to see how a group could receive a smaller proportion of all income over time, but at the same time have greater income because there is more income to spread between groups.

METHODS AND PROCEDURES

A small-scale replication of the 1947 study was undertaken in the spring of 1963. In order properly to compare the replication with the original, it was necessary to replicate the study using procedures as nearly identical as possible with those of the earlier study. The same question was used to elicit ratings, and the ninety job titles were rated in the same orders (using rotated blocks) in the same way. Most of the items (with the exception of those that were historically obsolete) were repeated. Even the sample was selected according to the outmoded quota sampling methods employed in 1947. The few new items included in the restudy were placed in the questionnaire after the occupational ratings.

Because of the stability of prestige positions of occupations from subgroup to subgroup in the 1947 study, it was felt that a relatively small national sample would be sufficient for the replication. In all a total of 651 interviews was collected according to quota sampling methods from a national sample of adults and youths.[4]

As in the 1947 study, occupational ratings were elicited by asking respondents to judge an occupation as having *excellent*, *good*, *average*, *somewhat below average*, or *poor* standing (along with a "don't know" option) in response to the item: "For each job mentioned, please pick out the statement that best gives *your own personal opinion* of the *general standing* that such a job has."

One indicator of prestige position is the

[4] Justification for our claim that 651 cases suffice to give a reliable intertemporal comparison can be derived from examination of sampling error estimates based on the assumption of a random sample. Such estimates indicate that confidence limits at the 0.90 level for $p = 0.50$ and $N = 651$ are 0.47 and 0.53. For $N = 60$ (smaller than any subgroup used in this paper) the corresponding error estimates are 0.39 and 0.61. Thus for even relatively small subgroups any dramatic changes are likely to be detected, although it must be clearly understood that error estimates for quota sampling are only approximated by assuming that formulas for random samples apply.

proportion of respondents (among those rating an occupation) giving either an "excellent" or a "good" response. Another measure which can be derived from a matrix of ratings by occupation requires weighting the various responses with arbitrary numerical values: We can assign an excellent rating a numerical value of 100, a good rating the value of 80, an average rating the value of 60, a somewhat below average rating the value of 40, and a poor rating the value of 20. Calculating the numerical average of these arbitrarily assigned values over all respondents rating the occupation yields the NORC prestige score. This latter measure has received rather widespread use despite arbitrariness in the numerical weights assigned to the five possible ratings.[5]

The ratings and derived scores for each of the ninety occupations obtained in 1947 and in 1963 are shown in Table 1. We present the findings in such detail because of their intrinsic interest. However, the bulk of the analysis contained in this paper is more concerned with characteristics of the distributions of these ratings than with the positions of particular occupations.

CONGRUITIES IN OCCUPATIONAL PRESTIGE: 1947–63

The major result of the 1963 restudy is dramatically summarized in the product-moment correlation coefficient of .99 between the scores in 1947 and the scores in 1963. The linear regression of the 1963 on the 1947 scores is given by

$$\mathbf{Y} = 0.97X + 2.98 \,,$$

a result which indicates that there are very little regression toward the mean and a slight net upward shift in scores.[6] (Here and elsewhere in the text boldface symbols are used to represent regression estimates.)

The high over-all correlation in the total set of occupations is matched by high correlations within subsets of occupations. If we group occupations into professional occupations, other non-manual occupations, and manual occupations, as in Table 2, we can see that the regression lines within the three groups are quite similar.[7]

The very slight effect of grouping occupations is shown again in Figure 1, where the three within-group regression lines are plotted over the range of the 1947 NORC scores contained within each group. The three lines nearly coincide over the observed range of the NORC scores and do not appreciably depart from the line $Y = X$ (where the 1963 and the 1947 scores are equal).

The gross similarity between the 1947 and the 1963 NORC scores tends to overshadow some interesting small changes revealed by the data. Thus, in Figure 1 the regression line for blue-collar occupations lies above (and, in fact, parallels) the line $Y = X$. Consequently, one infers that all blue-collar occupations had slightly higher scores in 1963. For professionals and other white-collar workers, however, the picture is more complex, since the within-group regression lines for these two broad groupings cross over the line $Y = X$. Consequently, in the case of professionals, those particular occupations with the highest prestige scores in 1947 (largely scientific

[5] The reader will observe that the correlation between the two ways of ordering occupations need not be unity. Of the two measures mentioned above, the proportion of excellent or good ratings enjoys some advantages over the NORC prestige scores. Its range and variance are somewhat larger than the NORC prestige scores, which tend to obscure differences between occupations in the middle of the prestige hierarchy. However, the two measures are, in fact, highly intercorrelated ($r =$ .98) and the advantages of the proportion of excellent or good ratings over the NORC prestige scores are largely statistical in nature. Throughout this paper, the bulk of our analysis employs the NORC prestige scores—a decision based largely on the wide use and popularity of the prestige scores derived from the original 1947 study.

[6] When the NORC scores are ranked, we find a Spearman rank-order correlation of .98 between the 1947 and 1963 ranks.

[7] The hypothesis that a common regression line fits all groups may be rejected at the 0.07 level of confidence, as indicated by the F-ratio resulting from an analysis of covariance.

TABLE 1

DISTRIBUTIONS OF PRESTIGE RATINGS, UNITED STATES, 1947 AND 1963

Occupation	March, 1947								June, 1963							
	Per Cent						NORC Score	Rank	Per Cent						NORC Score	Rank
	Excellent*	Good	Average	Below Average	Poor	Don't Know†			Excellent‡	Good	Average	Below Average	Poor	Don't Know§		
U.S. Supreme Court justice	83	15	2	‖	‖	3	96	1	77	18	4	1	1	1	94	1
Physician	67	30	3	‖	‖	1	93	2.5	71	25	4	‖	‖	1	93	2
Nuclear physicist	48	39	11	1	1	51	86	18	70	23	5	1	1	10	92	3.5
Scientist	53	38	8	1	‖	7	89	8	68	27	5	‖	‖	2	92	3.5
Government scientist	51	41	7	1	‖	6	88	10.5	64	30	5	‖	1	2	91	5.5
State governor	71	25	4	‖	‖	1	93	2.5	64	30	5	‖	1	1	91	5.5
Cabinet member in the federal government	66	28	5	1	‖	6	92	4.5	61	32	6	1	1	2	90	8
College professor	53	40	7	‖	‖	1	89	8	59	35	5	‖	‖	1	90	8
U.S. representative in Congress	57	35	6	1	1	4	89	8	58	33	6	2	‖	2	90	8
Chemist	42	48	9	1	‖	7	86	18	54	38	8	‖	‖	3	89	11
Lawyer	44	45	9	1	1	1	86	18	53	38	8	‖	‖	‖	89	11
Diplomat in the U.S. foreign service	70	24	4	1	1	9	92	4.5	57	34	7	1	1	3	89	11
Dentist	42	48	9	1	‖	‖	86	18	47	47	6	‖	‖	‖	88	14
Architect	42	48	9	1	‖	6	86	18	47	45	6	‖	‖	2	88	14
County judge	47	43	9	1	‖	1	87	13	50	40	8	1	‖	1	88	14
Psychologist	38	49	12	1	‖	15	85	22	49	41	8	1	‖	6	87	17.5
Minister	52	35	11	1	1	1	87	13	53	33	13	1	1	1	87	17.5
Member of the board of directors of a large corporation	42	47	10	1	‖	5	86	18	42	51	6	1	‖	1	87	17.5
Mayor of a large city	57	36	6	1	‖	1	90	6	46	44	9	1	1	‖	87	17.5
Priest	51	34	11	2	2	6	86	18	52	33	12	2	1	6	86	21.5
Head of a department in a state government	47	44	8	‖	1	3	87	13	44	48	6	1	1	1	86	21.5
Civil engineer	33	55	11	1	‖	5	84	23	40	52	8	‖	‖	2	86	21.5
Airline pilot	35	48	15	1	1	3	83	24.5	41	48	11	1	‖	1	86	21.5
Banker	49	43	8	‖	‖	1	88	10.5	39	51	10	1	‖	‖	85	24.5
Biologist	29	51	18	1	1	16	81	29	38	50	11	‖	‖	6	85	24.5
Sociologist	31	51	16	1	1	23	82	26.5	35	48	15	1	1	10	83	26
Instructor in public schools	28	45	24	2	1	1	79	34	30	53	16	1	‖	‖	82	27.5

* Bases for the 1947 occupational ratings are 2,920 less "don't know" and not answered for each occupational title.

† Base is 2,920 in all cases.

‡ Bases for the 1963 occupational ratings are 651 less "don't know" and not answered for each occupational title.

§ Base is 651 in all cases.

‖ Less than 0.5 per cent.

Source of 1947 distributions: Albert J. Reiss, Jr., and others, *Occupations and Social Status* (New York: Free Press of Glencoe, 1961) …

Occupation	March, 1947								June, 1963							
	Per Cent						NORC Score	Rank	Per Cent						NORC Score	Rank
	Excel-lent*	Good	Aver-age	Below Aver-age	Poor	Don't Know†			Excel-lent‡	Good	Aver-age	Below Aver-age	Poor	Don't Know§		
Captain in the regular army	28	49	19	2	2	2	80	31.5	28	55	16	2	‖	1	82	27.5
Accountant for a large business	25	57	17	1	‖	3	81	29	27	55	17	1	‖	‖	81	29.5
Public school teacher	26	45	24	3	2	‖	78	36	31	46	22	1	‖	‖	81	29.5
Owner of a factory that employs about 100 people	30	51	17	1	1	2	82	26.5	28	49	19	2	1	1	80	31.5
Building contractor	21	55	23	1	‖	1	79	34	22	56	20	1	‖	‖	80	31.5
Artist who paints pictures that are exhibited in galleries	40	40	15	3	2	6	83	24.5	28	45	20	5	2	4	78	34.5
Musician in a symphony orchestra	31	46	19	3	1	5	81	29	25	45	25	3	1	3	78	34.5
Author of novels	32	44	19	3	2	9	80	31.5	26	46	22	4	2	5	78	34.5
Economist	25	48	24	2	1	22	79	34	20	53	24	2	1	12	78	34.5
Official of an international labor union	26	42	20	5	7	11	75	40.5	21	53	18	5	3	5	77	37
Railroad engineer	22	45	30	3	‖	1	77	37.5	19	47	30	3	1	1	76	39
Electrician	15	38	43	4	‖	1	73	45	18	45	34	2	‖	‖	76	39
County agricultural agent	17	53	28	2	‖	5	77	37.5	13	54	30	2	1	4	76	39
Owner operator of a printing shop	13	48	36	3	‖	2	74	42.5	13	51	34	2	‖	2	75	41.5
Trained machinist	14	43	38	5	‖	2	73	45	15	50	32	4	‖	‖	75	41.5
Farm owner and operator	19	46	31	3	1	1	76	39	16	45	33	5	‖	1	74	44
Undertaker	14	43	36	5	2	2	72	47	16	46	33	3	2	3	74	44
Welfare worker for a city government	16	43	35	4	2	4	73	45	17	44	32	5	2	2	74	44
Newspaper columnist	13	51	32	3	1	5	74	42.5	10	49	38	3	1	1	73	46
Policeman	11	30	46	11	2	1	67	55	16	38	37	6	2	‖	72	47
Reporter on a daily newspaper	9	43	43	4	1	2	71	48	7	45	44	3	1	1	71	48
Radio announcer	17	45	35	3	‖	2	75	40.5	9	42	44	5	1	1	70	49.5
Bookkeeper	8	31	55	6	‖	1	68	51.5	9	40	45	5	1	‖	70	49.5
Tenant farmer—one who owns livestock and machinery and manages the farm	10	37	40	11	2	1	68	51.5	11	37	42	8	3	1	69	51.5
Insurance agent	7	34	53	4	2	2	68	51.5	6	40	47	5	2	‖	69	51.5
Carpenter	5	28	56	10	1	‖	65	58	7	36	49	8	1	‖	68	53
Manager of a small store in a city	5	40	50	4	1	1	69	49	3	40	48	7	2	‖	67	54.5
A local official of a labor union	7	29	41	14	9	11	62	62	8	36	42	9	5	4	67	54.5
Mail carrier	8	26	54	10	2	‖	66	57	7	29	53	10	1	‖	66	57
Railroad conductor	8	30	52	9	1	1	67	55	6	33	48	10	3	‖	66	57
Traveling salesman for a wholesale concern	6	35	53	5	1	2	68	51.5	4	33	54	7	3	2	66	57

TABLE 1—*Continued*

Occupation	March, 1947								June, 1963							
	Per Cent						NORC Score	Rank	Per Cent						NORC Score	Rank
	Excellent*	Good	Average	Below Average	Poor	Don't Know†			Excellent‡	Good	Average	Below Average	Poor	Don't Know§		
Plumber	5	24	55	14	2	1	63	59.5	6	29	54	9	2	‖	65	59
Automobile repairman	5	21	58	14	2	‖	63	59.5	5	25	56	12	2	‖	64	60
Playground director	7	33	48	10	2	4	67	55	6	29	46	15	4	3	63	62.5
Barber	3	17	56	20	4	1	59	66	4	25	56	13	2	1	63	62.5
Machine operator in a factory	4	20	53	20	3	2	60	64.5	6	24	51	15	4	1	63	62.5
Owner-operator of a lunch stand	4	24	55	14	3	1	62	62	4	25	57	11	3	1	63	62.5
Corporal in the regular army	5	21	48	20	6	3	60	64.5	6	25	47	15	6	2	62	65.5
Garage mechanic	4	21	57	17	1	‖	62	62	4	22	56	15	3	‖	62	65.5
Truck driver	2	11	49	29	9	‖	54	71	3	18	54	19	5	‖	59	67
Fisherman who owns his own boat	3	20	48	21	8	7	58	68	3	19	51	19	8	4	58	68
Clerk in a store	2	14	61	20	3	‖	58	68	1	14	56	22	6	‖	56	70
Milk route man	2	10	52	29	7	1	54	71	3	12	55	23	7	1	56	70
Streetcar motorman	3	16	55	21	5	2	58	68	3	16	46	27	8	2	56	70
Lumberjack	2	11	48	29	10	8	53	73	2	16	46	29	7	3	55	72.5
Restaurant cook	3	13	44	29	11	1	54	71	4	15	44	26	11	‖	55	72.5
Singer in a nightclub	3	13	43	23	18	6	52	74.5	3	16	43	24	14	3	54	74
Filling station attendant	1	9	48	34	8	1	52	74.5	2	11	41	34	11	‖	51	75
Dockworker	2	7	34	37	20	8	47	81.5	2	9	43	33	14	3	50	77.5
Railroad section hand	2	9	35	33	21	3	48	79.5	3	10	39	29	18	2	50	77.5
Night watchman	3	8	33	35	21	1	47	81.5	3	10	39	32	17	1	50	77.5
Coal miner	4	11	33	31	21	2	49	77.5	3	13	34	31	19	2	50	77.5
Restaurant waiter	2	8	37	36	17	1	48	79.5	2	8	42	32	16	‖	49	80.5
Taxi driver	2	8	38	35	17	1	49	77.5	2	8	39	31	18	1	49	80.5
Farm hand	3	12	35	31	19	1	50	76	3	12	31	32	22	‖	48	83
Janitor	1	7	30	37	25	1	44	85.5	1	9	35	35	19	1	48	83
Bartender	1	6	32	32	29	4	44	85.5	1	7	42	28	21	2	48	83
Clothes presser in a laundry	2	6	35	36	21	2	46	83	2	7	31	38	22	1	45	85
Soda fountain clerk	1	5	34	40	20	2	45	84	‖	5	30	44	20	1	44	86
Sharecropper—one who owns no livestock or equipment and does not manage farm	1	6	24	28	41	3	40	87	1	8	26	28	37	2	42	87
Garbage collector	1	4	16	26	53	2	35	88	2	5	21	32	41	1	39	88
Street sweeper	1	3	14	29	53	1	34	89	1	4	17	31	46	1	36	89
Shoe shiner	1	2	13	28	56	2	33	90	‖	3	15	30	51	2	34	90
Average	22	31	30	11	7	4	70		22	32	29	11	6	2	71	

and free professional occupations) slightly increased their scores, whereas those professional occupations with relatively low prestige in 1947 (marginal professional occupations such as "singer in a nightclub") receive somewhat lower scores. Among "other white-collar occupations" the situation is reversed. That is, from the within-group regression line we see that the other white-collar occupations with highest prestige in 1947 (largely managerial and political occupations) tended on the average to decline slightly, whereas lower white-collar occupations slightly increased in prestige.[8]

One other point is brought out sharply by Figure 1 and deserves mention. Since the within-occupational-group regression lines are plotted only for the range of 1947 scores observed within the group, one can easily see the appreciable overlap in scores between professional, other white-collar, and blue-collar occupations. Although these divisions are often employed by social scientists as though they represented fundamental class barriers,[9] Figure 1 makes clear that no such barrier can be detected on the basis of occupational prestige. The cleavage between white-collar and blue-collar—if it exists at all—is based not so much upon matters of societal evaluation as perhaps upon the character of dress and work in the three groups.

All in all the preceding results indicate

TABLE 2

REGRESSIONS WITHIN SUBSETS OF OCCUPATIONS

Occupation Group	Regression Coefficient	Regression Constant	Correlation
Total, all occupations (n=90) .. .	0 97	2 98	99
Professional, including one title duplicated for validation purposes (n=33)..	1 05	−3 61	96
One non-manual occupations (n=21)	0 92	5 85	.98
All manual occupations, including one craft occupation duplicated for validation purposes and two military titles (n=21)	1 00	2 00	99
Farm occupations (n=4); not computed . ..	. .	. .	

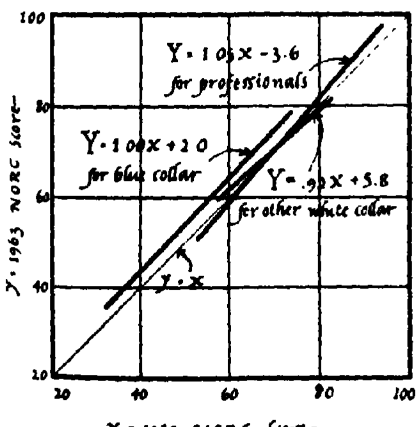


FIG. 1.—Regressions of 1963 NORC score on 1947 NORC score within occupational groups.

a striking similarity between the structure of the 1947 and the 1963 NORC scores. While we shall subsequently document a number of systematic shifts in the prestige of specific occupational groups, it is abun-

[8] There is a slight increase in the ability of the within-group regression lines to predict the direction of changes in scores between 1947 and 1963, as compared with the regression line for the total set. Correct predictions about the directions of change can be made by the over-all regression in 60.5 per cent of the cases and by the within-group regression lines in 62.8 per cent of them, an increase in efficiency of 5.8 per cent.

[9] This is, e.g., the major distinction employed in a recent comparative study of occupational mobility (Seymour Martin Lipset and Reinhard Bendix, *Social Mobility in Industrial Society* [Berkeley: University of California Press, 1959]).

dantly clear that these shifts are small and did not produce any substantial re-ordering of the relative prestige of the ninety occupations under consideration here.

There are several good reasons for this observed stability. First, relative differential educational requirements, monetary rewards, and even something as nebulous as the functional importance of occupations are not subject to rapid change in an industrial society.[10] Second, any dramatic shifts in the prestige structure of occupations would upset the dependency which is presumed to hold between the social evaluation of a job, its educational prerequisites, its rewards, and its importance to society. Finally, instabilities would further ambiguities or status inconsistencies if the prestige structure were subject to marked and rapid change. Indeed, the meaning of achievement, career, seniority, and occupational mobility would be fundamentally altered if occupational prestige were subject to large-scale changes. No small amount of intragenerational mobility between prestige classes would, for example, be induced solely by the changing structure of occupational prestige *even though individuals did not change their occupations over time.*

KNOWLEDGE ABOUT OCCUPATIONS AND OCCUPATIONAL RATINGS

Occupations vary in their visibility to wide segments of the members of a society. To capture the extent to which such knowledge is distributed throughout the American population, one of the options on the NORC rating scales was "I don't know where to place that one." "Don't know" responses of this sort may indicate ignorance about an occupation and/or ambivalence about its location in the hierarchy of prestige. Given the pattern of heavier proportions of "don't know" responses for the more esoteric occupations (see Table 1), it would appear that such responses indicate primarily lack of knowledge rather than ambivalence.

Knowledge about occupations has increased markedly over the period 1947–63. The correlation between proportions "don't know" for the two periods is .85, with the linear regression of 1963 proportions on 1947 proportions represented by

$$Y = 0.29X + 0.62 \ .$$

The "don't know" percentages were considerably less in 1963 as compared with 1947, and for only one occupation did the percentage increase during the period. Since the correlation between percentage "don't know" for the two periods was very high, occupations receiving high proportions of "don't knows" in 1947 also received high proportions in 1963, even though the average proportion "don't know" for the entire group of occupations dropped.

Inspection of the scattergram of the 1963 and 1947 percentages "don't know" for the ninety occupations reveals that the results reported above are strongly affected by the inclusion of "nuclear physicist" which elicited a very large proportion of "don't know" responses in 1947. Omitting "nuclear physicist," the regression of the 1963 percentage "don't know" on the 1947 percentage "don't know" over the remaining eighty-nine occupations becomes

$$Y = 0.44X + 0.26 \ ,$$

and r increases to .91. While these results lead to much the same interpretation as those reported above, they do call attention to the remarkable change in knowledge about the prestige rating of "nuclear physicist." In 1947, 51 per cent of all respondents did not know where to place "nuclear physicist," but by 1963 this figure had dropped to 10 per cent.

Insights into the quality of information about occupations held by respondents can

[10] For a discussion of this point see Otis Dudley Duncan, "Properties and Characteristics of the Socio-economic Index," in Reiss *et al.*, *op. cit.*, pp. 152–53. A correlation of .94 was found between an aggregate measure of the income of an occupation in 1940 and a similar indicator in 1950; a correlation of .97 was found between the proportion of high-school graduates in an occupation in 1940 and the same measure in 1950.

be gained from responses to a supplementary question included in both the 1963 and the 1947 studies. Respondents were asked: "A good many people don't know exactly what a *nuclear physicist* does, but what is your *general* idea of what he does?" In 1947 only 3 per cent of all respondents supplied a "correct" answer by indicating that a nuclear physicist "investigates the properties of the nucleus of the atom, breaks down nuclear or atomic energy, studies the innermost part of the atom, works on nuclear fission" or some similar description of the subject matter of nuclear physics. In 1963, only 2 per cent of respondents provided a correct answer of the kind indicated above. In response to this item in 1947, 55 per cent of the respondents claimed they "did not know" what a nuclear physicist was, while in 1963 this figure had dropped to 25 per cent. Thus, while there is no indication that more persons "knew" in a precise way what a nuclear physicist does, there were considerably more persons in 1963 who were willing not only to rate "nuclear physicist" but to provide indications—such as "works with atomic bomb," "runs Cape Canaveral," "is a laboratory worker," or "does atomic research"—of their impression of what a nuclear physicist does. Many of these indications are vague and erroneous, but they apparently provide a sufficient basis for respondents to draw inferences about the general standing of nuclear physicists. Thus, it appears that respondents are willing to evaluate occupations without a clear and well-defined idea of the duties involved in their performance.

The situation of the nuclear physicist suggests an hypothesis to which we will pay some attention. As it turned out the 1947 NORC score for "nuclear physicist" was 86, while the 1963 score was 92. Thus, an increase in the standing of "nuclear physicist" accompanied the remarkable increase in the proportion of respondents willing to rate the occupation. One may presume that the frequent mention of nuclear physicists in the press between 1947 and 1963 contributed to the increased willingness of respondents to give their impressions of the occupation. The particular hypothesis which concerns us is that publicity enhances prestige. The case of nuclear physicist and several other occupations suggests that there may be some truth to this hypothesis. Here we can examine whether changes in the ability to rate occupations (as indicated by the proportion of "don't know" responses), which should be a consequence of publicity, affect occupational standings.

Most of the ninety NORC occupations were already well-known in 1947, so that little if any change in the ability of respondents to rate them could be expected. We consider, therefore, only those twenty-five NORC occupations which in 1947 had 5 or more per cent of respondents refusing to rate them. The correlation between changes in percentages "don't know" between 1947 and 1963 with changes in prestige scores for this subset of occupations is .48.[11] Thus, for this subset of occupations for which there was some room for the diffusion of knowledge, the expected positive relationship between decreases in the percentage "don't know" and score increases apparently holds.[12]

[11] The regression of the change in the percentage "don't know" on the change in prestige scores is given by $Y = 0.16X - 0.11$.

[12] However, "nuclear physicist," which shows the most dramatic change in "don't knows," falls among these twenty-five occupations. Upon exclusion of this occupation, we find that for the remaining twenty-four occupations r falls to .33. On the other hand, the regression slope is two-thirds again as large since for the twenty-four occupations under consideration, $Y = 0.27X - 0.61$. Thus, whether elimination of "nuclear physicist" upsets the observed relationship between decreases in the proportion "don't know" and score changes depends on whether you take the point of view of regression or of correlation. On the one hand, the variance around the regression line is greater, but on the other hand, score changes increase more rapidly with decreases in the percentage "don't know." In any case, for the twenty-four occupations, we have found some evidence, albeit slight, in support of the predicted positive association be-

OCCUPATIONAL PRESTIGE SINCE 1925

Since the appearance of George S. Counts's pioneering 1925 study of occupational prestige, a number of readings have been taken on the distribution of occupational prestige. These studies have utilized a variety of different measurement techniques and different types of samples of raters, college students being quite popular. However, there is evidence that the over-all structure of prestige is invariant under quite drastic changes in technique.[13] Furthermore, one of the major findings of the original 1947 NORC survey was that *all* segments of the population share essentially the same view of the prestige hierarchy and rate occupations in much the same way.[14] With these findings in mind, we may utilize selected prestige studies conducted since 1925 to ascertain whether any substantial changes in the prestige structure of occupations have occurred since that date.

A pre–World War II and post-Depression bench mark is provided by the investigations of Mapheus Smith, who provides the mean ratings of one hundred occupations as rated by college and high-school students in the academic years 1938–39, 1939–40, and 1940–41. The rating technique used by Smith differs considerably from that employed in the NORC study. Respondents were originally required to *rank* occupations according to how far an average incumbent would be seated from the guest of honor at a dinner honoring a celebrity and then to *rate* the occupations on a 100-point scale of prestige (according to the rater's personal estimation).[15]

A pre-Depression bench mark of occupational prestige is provided by Counts's study, which provides rankings of forty-five occupations according to their "social standing." The data were collected from high-school students, high-school teachers, and college students.[16] Unlike the NORC and Smith studies, rankings rather than ratings were obtained by Counts. Counts provides rankings for six groups of respondents, and a continuous type variable can be derived by taking the average rank of an occupation over the six groups, weighting for the number of respondents in each group.

These four studies, then, provide an opportunity to examine occupational prestige since 1925. A fairly large number of titles are shared in common between each pair of studies, so that the number of titles utilized in any given comparison is larger than the total number of titles that have been rated in many prestige studies.[17]

Product-moment correlations between the prestige ratings of occupations common to each pair of studies are presented in Table 3, together with the number of matching titles. It is evident from the data presented in Table 3 that *there have been no substantial changes in occupational prestige in the United States since 1925.* The lowest correlation observed is .934, and this occurs between the 1963 NORC scores and the mean ranks derived from the 1925 study of Counts. In view of the high correlation between 1947 and 1963 NORC scores, it is not particularly sur-

tween score changes and shifts in public knowledge of occupational standings.

[13] One study, e.g., requested respondents to sort seventy of the occupations in the NORC list into groups of *similar* occupations. The respondent was then asked to order the groups of similar occupations he had formed into social levels. Nevertheless, a rank-order correlation of .97 was found between scores derived from this study and scores obtained from the 1947 NORC study (see John D. Campbell, "Subjective Aspects of Occupational Status" [unpublished Ph.D. thesis, Harvard University, 1952], chap. ii).

[14] Reiss *et al.*, *op. cit.*, pp. 189–90.

[15] Mapheus Smith, "An Empirical Scale of Prestige Status of Occupations," *American Sociological Review*, VIII (April, 1943), 185–92.

[16] George S. Counts, "The Social Status of Occupations: A Problem in Vocational Guidance," *School Review*, XXXIII (January, 1925), 16–27.

[17] See, e.g., the national studies cited by Alex Inkeles and Peter H. Rossi, "National Comparisons of Occupational Prestige," *American Journal of Sociology*, LXI (January, 1956), 329–39.

prising that high correlations are found between any pair of studies from adjacent points in time. That no substantial changes are observed over a span of approximately 40 years is a bit more surprising and is further evidence of constraints toward the stability of prestige hierachies.

Slight though the variation in correlations in Table 3 may be, it is noteworthy that the observed variation is apparently a function of elapsed time. The longer the elapsed time between any two studies, the smaller tends to be the correlation between them. Although this point is readily apparent on inspection of Table 3, we can provide a convenient quantitative summary by correlating the squares of the correlations between the *i*th and *j*th time periods and the elapsed times between the *i*th and *j*th time period, yielding a coefficient of —.85, a value significantly different from zero at the 0.025 level despite the fact that only six observations are involved.

TABLE 3

CORRELATIONS BETWEEN OCCUPATIONAL PRESTIGE RATINGS AT SELECTED TIME PERIODS, 1925–63*

Study and Time Period	C	S	X	Y
C (Counts' mean ranks, 1925)	. . . . .	.968	.955	934
S (Smith's mean ratings, *ca.* 1940)	23	.	982	.971
X (NORC scores, 1947)	29	38	. . . .	990
Y (NORC scores, 1963)	29	38	90	. . .

* Correlations placed above diagonal; no. of matching titles placed below diagonal.

Sources: George S Counts, "The Social Status of Occupations: A Problem in Vocational Guidance," *School Review*, XXXIII (January, 1925), 20-21, Table 1; Mapheus Smith, "An Empirical Scale of Prestige Status of Occupations," *American Sociological Review*, VIII (April, 1943), 187–88, Table I, National Opinion Research Center, "Jobs and Occupations. A Popular Evaluation," *Opinion News*, IX (September 1, 1947), 3–13. See text for details.

Small changes in occupational prestige can be obscured by the very high degree of intertemporal stability. Although the techniques used in the studies by NORC, Smith, and Counts make direct comparisons precarious, regression analysis permits us to follow changes in the prestige of nineteen occupations common to all studies over the span 1925–63. Let C = the mean rankings derived from Counts's 1925 study; S = the mean ratings taken from Smith (*ca.* 1940); X = the 1947 NORC scores; and Y = the 1963 NORC scores. Three regression equations relating these studies are as follows:

$$\mathbf{S} = 1.62(C) + 15.47\,,$$

$$\mathbf{X} = 0.60(S) + 37.04\,,$$

$$\mathbf{Y} = 0.97(X) + 2.98\,,$$

all computed over the total number of matching occupations between the studies involved. We can consider the residuals from these regressions, $S - \mathbf{S}$, $X - \mathbf{X}$, and $Y - \mathbf{Y}$, as indicators of change. Thus, for example, the values of $S - \mathbf{S}$ are indicators of change over the period 1925–40.

Are the changes in one period systematically related to changes in the next or later periods? To answer this question the correlations between the three sets of residuals were computed.

In the linear regression model which underlies the three regression equations presented in the preceding paragraph, $S - \mathbf{S}$, $X - \mathbf{X}$, and $Y - \mathbf{Y}$ are assumed to be random errors and should, therefore, be uncorrelated. If, to the contrary, they prove correlated, then we will have adduced some evidence of the presence of long-run secular *changes* in prestige.[18]

[18] Note the use of "changes" rather than "trends"; because of the fact that the prestige scales have a ceiling and a floor, one might argue that $S - \mathbf{S}$ will be negatively correlated with $X - \mathbf{X}$ which

None of the correlations involving the three sets of residuals is significantly different from zero:

$$r_{(S-S)(X-X)} = -.24$$

and

$$r_{(S-S)(Y-Y)} = +.23 \, ,$$

over the nineteen occupations matching between the studies of Counts, Smith, and NORC, while

$$r_{(X-X)(Y-Y)} = +.10 \, ,$$

over the thirty-eight occupations matching between the Smith and NORC studies. Thus there is little evidence of any particular pattern to the small changes in prestige observed between the studies and no evidence whatsoever of any substantial changes in the over-all structure of prestige.

The residuals from the three regression equations linking the four studies in a time sequence can be examined in another way. If $S - S$ is positive, then we conclude that the prestige of the occupation in question increased between 1925 and 1940, since the observed value of S exceeds the value of S expected on the basis of Counts's study. However, *all* the residuals are quite small and many would lie within a 5 per cent confidence band which might be drawn about the regression lines from which they are calculated. In addition, only five of the nineteen occupations common to the four studies either increased or decreased their prestige continuously over the entire period 1925–63.

Although the patterns of change indicated by these residuals are highly irregular and do not lend themselves to an over-all interpretation, changes observed for particular occupations invite speculation. For example, the residuals indicate that the prestige of physicians increased slightly between 1925–40 and 1940–47, but declined in the period 1947–63. Increases in the two initial periods might be attributed to concomitant progress in medical technology, while the medical profession's attitude toward Medicare and other public medical plans might account for the recent decline in the prestige of physicians. Some corroboration of this interpretation can be derived from unpublished tabulations from the 1947 study and its replication of ratings by age cohorts. While the reader familiar with previous reports of prestige ratings will recall that subgroup differences are small, shifts in the prestige rating of physicians by age cohorts—small though they are—tend to indicate that older persons, more likely to be affected by health plans, were responsible for the relative decline in the prestige of physicians. This interpretation remains, however, an *ad hoc* one and has little but plausibility to support it.

While examination of shifts in the prestige of particular occupations for only a few points in time must perforce lead to unsubstantiable speculation, focus upon changes in the prestige of a group of similar occupations or upon a single occupation over many points in time does represent a viable approach to the analysis of prestige changes. Earlier in this paper, we considered the relationship between 1947–63 prestige changes and changes in the proportion "don't know." We were thus adducing a *ubiquitous* element in the movement of prestige, or, at least, one common to all occupations considered. In contrast, the *ad hoc* explanation advanced to account for the movement in the prestige of physicians over the period 1925–63 relied on factors *unique* to physicians, but not necessarily applicable to other occupations. Both approaches to the study of changes in occupational prestige would necessarily have to be used, if a truly comparable and extensive time series pertain-

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will in turn be negatively correlated with $Y - Y$, but that $S - S$ will be positively related to $Y - Y$. Reference to secular changes rather than secular trends is used advisedly, since it seems unlikely that *most* occupations increasing or decreasing in prestige in one period would continue to do so in the next time period, owing to the presence of a floor and ceiling to the prestige scale.

ing to the prestige of selected occupations were available.

CHANGES IN OCCUPATIONAL PRESTIGE, 1947–63

Owing to the sample size of the 1963 replication of the 1947 study, small changes in the prestige of particular occupations are apt to be unreliable. However, changes in the prestige of occupational *groups* may be more reliably inferred. This is the strategy we have adopted for summarizing the small changes that were observed between 1947 and 1963.

To illustrate that, despite inferences from the preceding section, over-all changes in prestige were not random, we can look at changes in ratings by different subgroups: those with some college experience, those with only high-school education, and those with an eighth-grade education or less. Changes between 1947 and 1963 as derived from the three groups were intercorrelated in order to determine the consistency of changes. A correlation of +.76 was found between changes observed for those with some college and changes observed for those with some high school. Changes observed for those with no more than a grade-school education were correlated +.56 and +.64 with the corresponding changes observed for those with some college and some high school, respectively. These coefficients indicate an appreciable consistency in the score changes observed within the three educational groups. In addition, it is interesting to note that the lowest correlation occurs between the best-educated and most poorly educated groups, suggesting that some of the differences between the changes observed in the three groups are attributable to the dissimilarity between them.[19]

In Table 4 the NORC occupations are allocated to the major occupation groups employed by the U.S. Bureau of the Census. The results shown in Table 4 are quite clear: among all blue-collar groups there was a net upward shift in prestige which exceeded the mean shift experienced by all occupations; although professional titles in the NORC list remained roughly the same, the two remaining white-collar groups experienced a net downward drift in prestige; and finally, farming pursuits—

TABLE 4

SELECTED MEASURES OF PRESTIGE CHANGE, 1947–63, BY MAJOR OCCUPATION GROUP

Major Occupation Group	No. of NORC Titles	Average Score Difference	Average Percentage Change
Total, all occupations	90	+0 8	+1 4
White collar			
Professional . . .	33	+0 7	+0 8
Managerial.	16	−0 4	−0.3
Clerical and sales	5	−0 2	−0 4
Total white collar	54	+0 3	+0 4
Blue collar *			
Craftsmen*.	9	+1 6	+2 3
Operatives . . .	8	+0 9	+1 6
Service	9	+2.4	+4 9
Laborers.	6	+2 2	+5 3
Total blue collar.	32	+1 8	+3 4
All Farm.	4	−0 2	−0.0

* Contains two military titles in NORC list which are not classified by the U S Bureau of the Census into major occupation groups, since they are non-civilian pursuits.

which represent a declining proportion of the experienced civilian labor force—likewise lost slightly in prestige. As one could detect in the regression equation relating 1947 and 1963 scores, the pattern of change by major occupation group reveals an attenuation between 1947 and 1963 in the range of the NORC scores. In part, this pattern represents regression toward the mean, but since *all* blue-collar groups shifted upward and professionals remained much the same, it also appears likely that some further obscuring of the white-collar-blue-collar distinction was under way.

Table 4 also contains by major occupation group another measure of change, namely,

[19] Results of this kind strongly suggest that the observed changes were not random and that had the 1947 and 1963 samples been randomly split into two subgroups, nearly identical changes would have been observed in the two groups.

the percentage change in 1947 score. As the reader can easily see these changes are in every case quite small, even at the lower levels of prestige where the scores were initially low. The percentage change in score is, of course, patterned in the same way as the score differences.[20]

The major occupation groups of the Census are far from homogeneous, and the allocation of NORC titles to these groups tends to obscure a number of important differences in the patterns of change observed within various occupational situses. Indeed, we find that the correlation ratio, η, of the score differences on the major occupation groups is only .29, while a correlation ratio of .42 is found between the percentage change in score and the classification of titles into major occupation groups.[21] Thus, despite the fairly clear pattern which emerges upon consideration of the major occupation groups, a considerable portion of the variance in the score differences and percentage change is obscured by the classification.

In order to surmount this difficulty and to highlight some of the systematic, if small, prestige changes that were taking place, a classification of NORC titles was expressly designed to illuminate the changes; its chief virtue is the economy of presentation which it facilitates. The groups used in the classification are given in the stub of Table 5 and the specific NORC titles allocated to each group are shown at the bottom of the table. Some titles are dubiously classified and the classification clearly employs several dimensions of occupational structure ranging from class of worker to the kinds of interpersonal contact most frequently encountered on the job.

Several important points emerge on consideration of the classification of NORC titles into occupational situses, which had been obscured in the previous classification by major occupation group. First, it is clear that the lack of change among the professions in Table 4 stems not from an absence of change, but from an averaging out of contradictory ones. The free professions, including occupations like physicians and civil engineer, increased in prestige, while cultural or communication-oriented professions, such as "musician in a symphony orchestra" and "radio announcer," declined in standing. Perhaps the most dramatic change is among the scientific occupations, which, with the single exception of "economist," enjoyed positive score differences. A second change obscured by previous analysis was the slight decrease in prestige experienced by political and governmental occupations. One major exception to the rule for other governmental titles was "policeman," which experienced an upswing in prestige. However, such a change is difficult to interpret since there are no other governmental titles of fairly low prestige. Whether it represents regression toward the mean or a genuine increment in respect for law-enforcement officers is difficult to say. The remaining situses in Table 5 are more loosely identified, and we are loath to place any interpretation upon the directions of change observed in them (with the exception of "artisans," largely a craft occupation). They may, however, provide useful guidelines for other researchers seeking to classify the NORC titles into more meaningful categories than those currently available. Taken as a whole, the classification

[20] The correlation between the percentage change in score and the scores in the initial period, 1947, turns out to be —.37, illustrating one of the dangers in interpreting the percentage change in score. Since an increase in score of a single point represents a larger percentage change for an occupation of low prestige than for one of high prestige, the negative correlation of the percentage change in score with the initial score is to be expected, and one is faced with the problem of comparing percentage changes based on quite different initial points of departure. The correlation also reflects to a lesser extent the problem of regression toward the mean which also plagues the analysis of change.

[21] The correlation ratios reported here and those reported below have been adjusted for the number of degrees of freedom utilized in computing the coefficient. For the correction formula see Mordecai Ezekial and Karl A. Fox, ***Methods of Correlation and Regression Analysis*** (3d ed.; New York: John Wiley & Sons, 1959), p. 301.

is, however, more closely related to the score differences and the percentage change in score than are the major occupation groups. Correlation ratios of .50 and .51 were found between the classification and, respectively, the score differences and percentage changes in scores.

A few other changes not previously noted and still obscured by the classification of NORC titles into occupational situses should be mentioned. Among the more important of these are the increases in scores observed for "a local official of a labor union" and "official of an international labor union." That these titles should experience increasing prestige, despite the sensational government investigations into the conduct of labor officials during the past decade, is perhaps indicative of the extent to which unions have been assimilated by, and have themselves adopted a more accommodating attitude toward, the established order.

The two military titles in the NORC list, "captain in the regular army" and "corporal in the regular army," remained much the same in the two time periods. Similarly, the two occupations rated twice under slightly different titles for reliability purposes, "public school teacher" (instructor in the public schools) and "automobile repairman" (garage mechanic), received nearly identical ratings under both stimulus titles in the replication, as had been

TABLE 5

SELECTED MEASURES OF PRESTIGE CHANGE, 1947–63, BY OCCUPATIONAL SITUS*

Occupational Situs	No. of NORC Titles	Average Score Difference	Average Percentage Change
Total, all occupations	90	+0.8	+1.4
Free professionals	13	+1.5	+1.8
Cultural/communication-oriented professions	7	−2.0	−2.4
Scientific professions	8	+2.6	+3.1
Political/government occupations	10	−0.7	−0.6
Big businessmen	4	−0.8	−0.8
Customer-oriented occupations	11	+0.1	+0.4
Artisans	8	+1.6	+2.4
Outdoor-oriented occupations	10	+1.7	+3.2
Dead-end occupations	5	+2.8	+7.2
All farm	4	−0.2	0.0
Other	10	+1.0	+1.5

* NORC titles are classified into occupational situses as follows.

Free professionals: physician, college professor, minister, architect, dentist, lawyer, priest, civil engineer, accountant for a large business, instructor in the public schools, public school teacher, undertaker, welfare worker for a city government

Cultural/communication-oriented professions: artist who paints pictures that are exhibited in galleries, musician in a symphony orchestra, author of novels, radio announcer, newspaper columnist, reporter on a daily newspaper, singer in a nightclub

Scientific occupations. scientist, government scientist, chemist, nuclear physicist, psychologist, sociologist, biologist, economist

Political/government occupations. U S Supreme Court justice, state governor, cabinet member in the federal government, diplomat in the U.S foreign service, mayor of a large city, U S representative in congress, county judge, head of a department in a state government, county agricultural agent, policeman

Big businessmen: banker, member of the board of directors of a large corporation, owner of a factory that employs about 100 people, building contractor.

Customer-oriented occupations that require face to face contact with the public in the ordinary course of a day's work: manager of a small store in a city, railroad conductor, owner-operator of a lunch stand, barber, clerk in a store, streetcar motorman, taxi driver, restaurant waiter, soda fountain clerk, bartender, filling station attendant

Artisans: owner-operator of a printing shop, electrician, trained machinist, carpenter, automobile repairman, plumber, garage mechanic, restaurant cook

Outdoor-oriented occupations in which an ordinary day's work is typically performed either outside or in an outdoors setting. airline pilot, mail carrier, railroad engineer, fisherman who owns his own boat, milk route man, truck driver, lumberjack, coal miner, railroad section hand, dock worker

Dead-end occupations which have no possibilities of future advancement. night watchman, janitor, garbage collector, street sweeper, shoe shiner.

All farm. farm owner and operator, tenant farmer (one who owns livestock and machinery and manages the farm), farm hand, sharecropper (one who owns no livestock or equipment and does not manage farm)

Other: captain in the regular army, official of an international labor union, bookkeeper, insurance agent, traveling salesman for a wholesale concern, playground director, local official of a labor union, corporal in the regular army, machine operator in a factory, clothes presser in a laundry.

Some of the titles in the "other" category might be reclassified into the remaining categories at some expense in homogeneity; such a reclassification does not affect the results presented in this table.

the case in 1947. The reader will notice in Table 1 that one- and two-point differences in NORC scores can be produced by simply rating the same occupation under slightly different titles. It seems likely, therefore, that changes of one or two points in the NORC score of an occupation could hardly be adequate for establishing a real change in prestige or even the direction of change in prestige (if any). The results for the duplicated titles indicate, therefore, that many of the observed changes in prestige scores discussed above are quite negligible and might possibly have been reversed if a slightly different phrasing of the occupational title had been employed.

One other point is worthy of mention before turning to a summary. Duncan has recently used regression techniques to extend the 1947 NORC scores to all occupations in the detailed classification of the U.S. Bureau of the Census. In the course of presenting his results, Duncan had occasion to discuss the temporal stability of his index and the implication that changes in occupational socioeconomic status might have for the validity of his results. On the basis of comparisons between aggregate education and income of occupations as observed in the 1940 and 1950 censuses, Duncan suggested that "changes—albeit minor ones for the most part—were indeed occurring in the socio-economic status of occupations during the decade 1940–50," adding that "such evidence as we have suggests a rather high temporal stability of occupational prestige ratings, although the time periods concerned have not been lengthy ones."[22] Surely there is nothing in the present study to alter these conclusions (which, indeed, provide a fair summary of the results of this paper). As the 1960 Census data become available for detailed occupations it will, of course, be possible to revise Duncan's index on the basis of the present replication, but barring any dramatic shifts in the aggregate income and education of occupations over the period 1950–60, there is no reason to believe that such a revision would alter in any appreciable way the socioeconomic scores which Duncan assigned to occupations on the basis of the 1947 NORC study and the 1950 Census data.

[22] Duncan, *loc. cit.*

CONCLUSIONS

The theme of this paper has been accurately captured by an eminent pathologist who remarked of biochemical phenomena: "Universal instability of constituents seems to be compatible with a stability and even monotony of organized life."[23] Such is the picture one gleans of occupational structures from the present endeavor. Between 1947 and 1963 we are fully aware that many *individual* changes in occupation were under way as men advanced in their career lines, retired, or entered the labor force. Yet, despite the turnover of incumbents, occupational morphology, at least insofar as prestige is concerned, remained remarkably stable. To be sure, systematic patterns of change could be detected, but one would miss the import of this paper if one failed to recognize that these changes were minor relative to the over-all stability. The view developed here is that a stable system of occupational prestige provides a necessary foundation to which individuals may anchor their careers.

System maintenance is, however, only part of the story. Small, but nevertheless systematic, changes can be detected between 1947 and 1963. In some cases these changes appear to be attributable to increasing public knowledge of occupations, but it was suggested that any complete understanding of prestige shifts and their causes would require a time series pertaining to the standing of particular occupations. The present study is a step in that direction. Our purposes will be adequately accomplished if others are stimulated to make periodic readings of, as it were, the occupational weather.

[23] René Dubos, *The Dreams of Reason: Science and Utopias* (New York: Columbia University Press, 1961), p. 124.

Institutional and Life-Cycle Socialization of College Freshmen[1]

Walter L. Wallace

ABSTRACT

The level of importance that students attached to getting good grades is higher and the level of their graduate aspirations lower at the time of their entry into college than at any other time in their Freshman year. Attitudinal change among college Freshmen during their first seven weeks of college experience seems sufficiently rapid and pervasive as seriously to question the validity of any single observation as being representative of Freshmen, or at least to require fairly exact dating of such an observation. The Freshman changes in grades evaluation and graduate aspiration seem to result from two kinds of peer-group influence—different in direction and in relevance. A special sociometric technique enables empirical differentiation between the primary agents of peer influence on college Freshmen's attitudes toward their present lives as students, on the one hand, and on Freshmen's attitudes toward their future, adult lives, on the other.

Two different sociological problems are involved in the study of college effects on students: life-cycle processes of individual socialization and the impact of a formal organization upon its participants. The first problem calls attention to the fact that college students are most often young people passing through the final stages of socialization into adult statuses and attitudes.[2] The second problem emphasizes that, particularly in the case of a college housing most of its students on campus,[3] this passage is accomplished largely within the boundaries of what approaches a "total institution."[4]

Thus, on the one hand, the resident college may be differentiated from other total institutions such as hospitals, reformatories, and prisons by noting that the college's aim is neither to mend nor to correct its inmates. Rather, its aim is to shape them toward statuses and roles for which they have never before been eligible. On the other hand, resident college students are differentiated from most other young people their age by the institution's relatively controlled and measurable social context within which late adolescent socialization occurs for them. A study of the effects of college experience on students, therefore, presents a dual opportunity: to examine various aspects of late adolescent socialization in a relatively controlled situation and to examine the influence of a particular

[1] The research on which this paper is based was carried out under grants made to Peter H. Rossi, Director of National Opinion Research Center and Professor of Sociology, University of Chicago, by the National Institutes of Health (Grant M-4836) and the College Entrance Examination Board. The writer wishes to express his thanks to Professor Rossi for his indispensable help in carrying out this study. The present paper is a revision of part of my study report, *Peer Groups and Student Achievement* (Chicago: National Opinion Research Center, 1963).

[2] Two excellent compendia of articles on adolescent socialization are Jerome M. Seidman (ed.), *The Adolescent—a Book of Readings* (rev. ed.; New York: Holt, Rinehart & Winston, 1960) and *Daedalus* (Winter, 1962).

[3] Eighty-six per cent of all students, including 92 per cent of Freshmen, at the college we studied lived on campus.

[4] See Erving Goffman, *Asylums* (Garden City, N.Y.: Doubleday & Co., 1961), esp. pp. 1–124. For discussions of literature on formal organizations see Peter M. Blau and W. Richard Scott, *Formal Organizations* (San Francisco: Howard Chandler, 1962), and James G. March and Herbert A. Simon, *Organizations* (New York: John Wiley & Sons, 1958).

kind of formal organization upon its inmates.

But a third opportunity—perhaps deserving even greater emphasis—also presents itself, in light of the facts: that some four million persons were enrolled in American institutions of higher learning (many of them resident institutions) in 1960, and that this number is expected to triple by 1980[5] though the total population is expected to have increased by not more than 40 per cent in the same year.[6] Thus, the American population seems likely to become more and more of a college-going and college-educated one. The third research opportunity is, therefore, to gain leverage in forecasting future social structural, attitudinal, and behavioral change in the United States.

Three broad questions, related to college effects on students, have received sociological attention in recent years. They are: What changes in the attitudes and behaviors of late adolescents occur in college? May any of these changes be attributed to social mechanisms specific to college? How permanent are such changes?[7] Since this study is confined to examining certain changes among college Freshmen, the present discussion will bear upon the first two questions only.

This paper explores two hypotheses: (1) entering Freshmen are quickly socialized into the prevailing college-student culture by Sophomores, Juniors, and Seniors, and (2) such socialization has significant effects upon Freshmen's academic attitudes, aspirations, and achievement.

Perhaps the widest sociological relevance of these hypotheses lies in the fact that college students' academic achievement and aspirations can set limits on their later occupational statuses and achievements. Thus, these student attitudes ultimately affect the occupational and intellectual achievement structures of American society at large. In addition, it is significant that the hypotheses rest upon a gross distinction between Freshmen, on the one hand, and non-Freshmen (Sophomores, Juniors, and Seniors), on the other. By this, an important differentiation among students is emphasized. This distinction is, indeed, important among participants in all organizations which admit new members and release old ones only at specified, regular intervals and in groups. Such organizations create a generational differentiation among their participants that is apt to be highly specific to the organization in question, but whose sociological relevance there may be no less than that of age-graded generational stratification in the larger society.

Clearly, the perpetuation of any culture, however societally general or organizationally specific, must depend upon its transmission to each generation of newcomers. This transmission must be accomplished rapidly—before the newcomers succeed in creating their own culture, which might be disruptive of the extant one. In this connection, it is worth noting that student generations (as distinct from societal "generations" enter college as a "class," rather than as a succession of individuals. College Freshmen, thus, comprise a newcomer *collectivity*, with the attendant high potentials for group formation and the establishment of group norms and sanctions. For this reason, the Freshman "class" may offer a more serious threat to the extant college norms than would a succession of individual newcomers. "New Student Weeks" and "Freshman Orientation Weeks"—with their concentrated initiatory rituals, symbol communication, and information-giving—may be understood as the formal side of organizational efforts to neutralize this threat with all practical speed. The more informal side of these efforts, involving the day-to-day interaction of oldtimers with

[5] Donald J. Bogue, "Population Growth in the United States," in Philip M. Hauser (ed.), *The Population Dilemma* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963), p. 79.

[6] *Ibid.*, p. 72.

[7] For a review of literature pertinent to these three questions, see Nevitt Sanford (ed.), *The American College* (New York: John Wiley & Sons, 1962).

newcomers, is the subject of the present study.[8]

METHOD AND DATA

A study specifically aimed at advancing empirical proof of peer-group socialization among newcomers to a formal organization confronts special methodological problems. It should be noted that, even when newcomers are found to change their attitudes in the direction of attitudinal states prevailing among their peer hosts, at least two different explanations may still be entertained: (1) the newcomers are adapting to the same external situation (e.g., externally imposed work content, work rules, time scheduling, spatial ordering, non-peer personal influence, etc.) that support their hosts' attitudes, and (2) the newcomers are being socialized through peer-group interpersonal communication. Essential to evaluating the latter explanation is a test for relationship between changes over time in the newcomers and communications from hosts, holding constant the external situation.

Though rigorous or detailed control of such external factors was not possible in the present field study, (1) the courses Freshmen took were prescribed at least with regard to areas (e.g., foreign language, science and mathematics, social studies, etc.) and were overwhelmingly Freshman in student composition, and (2) the 92 per cent of Freshmen who lived on campus were housed in four dormitories reserved almost exclusively for them. Therefore, to the extent that a general constancy of non-socialization situation over all male and all female Freshmen may be granted, the statistical associations which will be shown between changes in Freshman attitudes and peer-group (particularly non-Freshman) contacts may tentatively be taken as indicative of genuine socialization.

Survey and sociometric methods applied both cross-sectionally and longitudinally are combined for this analysis. In September, 1959, during their second day on campus, a paper-and-pencil questionnaire was administered to all 327 Freshmen then entering a small, coeducational, midwestern, liberal arts college of high academic reputation. This *questionnaire only elicited information* about the respondents themselves. This was based on the assumption that at so early a date Freshmen were unlikely to have formed extensive or stable acquaintances with one another and, even less likely, with non-Freshmen and faculty members (the vast majority of whom had not yet arrived on campus).

During the third week of the following November, a second questionnaire was administered to the same Freshmen plus the rest of the student body. In addition to survey-type questions about the respondents themselves, this questionnaire carried a complete listing of all 1,051 students enrolled at the college.[9] Each respondent scanned this list, checked each name that he recognized, and indicated how well he liked or disliked, as well as how many hours during an average school week he spent with each student whose name he recognized. The aggregate of students recognized by a given respondent will be referred to as his Interpersonal Environment (IE).[10] Various parts of this aggregate will be

[8] The organization's generational stratification may have still another sociological relevance to the extent that it modifies stratificatory systems operating in the organization's environment, such as the socioeconomic one, on interaction within the organization. Thus, *social* class distinctions may yield some of their influence over student interaction patterns to *college* class distinctions. For example, see James S. Coleman, *The Adolescent Society* (Glencoe, Ill.: Free Press, 1961), pp. 173–219. Although Coleman does not make this point explicitly, it seems reasonable to infer it from his data and discussion. The present data (not otherwise discussed here) show that more of the Freshmen's student acquaintances were confined to his own college class than to his own socioeconomic rank, as measured by father's occupation.

[9] This number includes forty-six graduate and special students who were not included in the analysis.

[10] The general procedure used here follows that set forth by Peter H. Rossi in "Research Strategies in the Measurement of Peer Group Influence" (unpublished manuscript).

termed IE segments. Each name recognized was then physically associated (on computer magnetic tape) with the survey data gathered from the recognized student. In this way, it became possible to measure a Freshman's IE on dimensions parallel to those measured by the Freshman's own survey data.

During the last week of April, 1960, a third and final questionnaire (identical, in most respects, with the November questionnaire) was administered to the Freshmen. At the same time, a questionnaire was administered to faculty members at the college. There will be only limited occasion to refer to these latter data in this paper.

In addition, official college records yielded data on the Freshmen's percentile rank in their high-school graduating classes, scores on the College Entrance Examination Board Scholastic Aptitude Test (SAT), and first-semester as well as first-year cumulative college grade-point averages.

FINDINGS

Grades evaluation, survey data.— In September, 78 per cent of Freshman boys and 74 per cent of Freshman girls reported that "getting the highest grades that I possibly can" was "highly important" (as opposed to "moderately" and "not" important) to them. In November, 50 and 35 per cent of Freshman boys and girls, respectively, rated "making at least an A-minus grade average" first or second when asked to compare its importance with that of "participating in extra-curricular clubs, teams, fraternities (or sororities), etc., as much as I would like to," "having as many dates as I want, with whomever I want," and "having close friends on campus and spending time with them." In April, 37 and 26 per cent of Freshman boys and girls, respectively, rated "making at least an A-minus grade average" first or second when asked the identical comparison questions from the November questionnaire.[11]

This decline in Freshman grades evaluation over the first seven months of college experience seems to have entailed considerable attitudinal turnover, but at a decreasing rate. Thus, the association (Yule's Q)[12] between Freshman grades evaluation in September and in November was 0.24 for men and 0.32 for women, but the association between grades evaluation in November and in April was 0.83 for men and 0.84 for women. Such findings immediately suggest change toward an asymptote, possibly toward conformity with a social norm.[13] In this regard, the levels of grades evaluation that prevailed among non-Freshmen are suggestive: In November, 42 and 27 per cent of non-Freshman men and women, respectively, rated "making at least an A-minus grade average" first or second when asked to make the same comparisons that Freshmen made then and in April. It, therefore, seems clear that Freshmen moved from an initially strong emphasis on grades toward increasing congruence with the relatively low levels of such emphasis that prevailed among their peer hosts.[14]

[11] The question was posed to respondents as a series of paired comparisons from which a rank-ordering was subsequently derived for each respondent. Respondents who made no choices between pairs or insufficiently transitive choices were excluded from all phases of the analysis that involved this variable. Approximately 86 per cent of all respondents made choices sufficiently transitive to locate the rank of getting high grades.

[12] Unless otherwise noted, Yule's Q is the measure of association used throughout this paper. For a discussion of Yule's Q, see Leo A. Goodman and William H. Kruskal, "Measures of Association for Cross-Classifications," *Journal of the American Statistical Association*, XLIX (December, 1954), 732–64.

[13] Although some of the decreasing rate of Freshman change may be due to the fact that the form in which the grades-evaluation question was put to respondents changed between September and November but did not change between November and April, it does not seem likely that this can account for all of the decrease in the rate of Freshman change.

[14] Sixty-two per cent of the fifty-eight faculty respondents gave high rank to grades evaluation when asked their preferences in male students; 43 per cent gave high rank to grades evaluation

As a first step toward explaining this development, some of its antecedent (i.e., measured in September) correlates were examined among the survey data. These results are shown in Table 1. The most striking thing here is that what may be called sociability predisposition (indicated by whether high, versus moderate or low, importance was assigned in September to "being accepted and liked by other students") was more likely than any other factor measured to precede turnover in grades evaluation. Moreover, this predisposition was strongly and positively associated with decrease in grades evaluation and negatively associated with increase in grades evaluation.[15]

Two alternative, but not contradictory, interpretations of this finding are possible: (1) a strong sociability predisposition implies a tendency to allocate more value, attention, and energy to social activities and less to academic ones, regardless of the attitudes of those to whom one is striving to be acceptable; and (2) a strong sociability predisposition implies greater sensitivity to the criteria of acceptability and greater willingness to conform to them. The latter alone may properly be termed a socialization hypotheses, and it is clear that only by bringing sociometric data to bear on this question can it be decisively tested.

Before that, however, the second main hypothesis can be examined by asking: What were the grade-point achievement consequences of grades-evaluation change?

TABLE 1

SOME ANTECEDENT VARIABLES ASSOCIATED WITH FRESHMAN GRADES-EVALUATION CHANGE (YULE'S Q)

ANTECEDENT VARIABLES	MALES			FEMALES		
	Turnover	Decrease*	Increase*	Turnover	Decrease	Increase
Importance of being accepted and liked by other students. .	+0.45 (125)†	+0.60 (95)	−0.11 (30)	+0.42 (147)	+0 62 (108)	−0 33 (39)
Importance of falling in love and getting married.	− .07 (125)	− .05 (95)	− .38 (30)	+ 21 (147)	+ 36 (108)	− 55 (39)
Importance of pleasing parents.	+ .38 (125)	+ .02 (95)	+ .60 (30)	+ 09 (147)	+ .10 (108)	+ 07 (39)
Rank in high-school graduating class. . . .	−0.23 (112)	−0.44 (86)	+0.43 (26)	+0 07 (131)	−0 22 (99)	+0 39 (32)

* "Decrease" = high September, low November grades evaluation; "Increase" = moderate or low September, high November grades evaluation.

† Here and throughout each number in parentheses is the total number of cases in the fourfold table from which its associated Q was calculated.

for female students. When these figures are compared with the corresponding student figures noted above, it becomes apparent that the faculty placed greater emphasis on students getting good grades than did either student generation in November. Therefore, any interpersonal influence that faculty members had on Freshmen seems likely to have been toward sustaining or increasing the latter's grades evaluations. But the fact that the actual Freshman trend was downward, toward non-freshman levels, suggests that interstudent influence (if it operated) on this variable was not only opposed to, but more powerful than, faculty-student influence.

[15] Comparison of increase and decrease association coefficients enables examination of discontinuity in "effect" of the independent variable. Specifically, the discrepancy shown in Table 1 between the strengths of associations between sociability predisposition and decrease in grades evaluation versus increase in grades evaluation indicates that sociability predisposition had greater "effect" in inducing a decline in grades evaluation than in maintaining an initially low grades evaluation. This finding complements the discussion of Table 6 below.

In approaching this question, account was taken of the fact that percentile rank in high-school graduating class together with SAT score generally predict from one-third to one-half of the variance in grade-point averages[16] by employing the standardized residuals from a multiple regression of the latter two variables on the former as our measure. Data on the regression and correlation analyses are shown in Table 2, and

TABLE 2

DATA ON FRESHMAN GRADE-POINT REGRESSION AND CORRELATION ANALYSES*

	Males	Females
No. of observations..	129	151
Regression coefficients		
b_0........	−0.8523	0 9869
b_1......	+0.9409	1.1926
b_2.....	+0.0014	0.0014
Correlation coefficients		
$R_{Y \cdot X_1}$........ ...	+0.47	0 51
$R_{Y \cdot X_1 X_2}$.	+0.57	0.64
Standard deviation of the residuals.... ..	0.44	0.44
Mean of Y...... ..	1.27	1 41
Mean of X_1..... .	0.70	0.79
Mean of X_2....... ..	1057.65	1004.19

* Variables: Y = first-year cumulative grade-point average (the college uses a three-point grading system); X_1 = percentile rank in high-school graduating class, X_2 = scholastic aptitude test total score.

it should be noted that what we shall call "overachievement" (i.e., a standardized residual of +0.50 or higher) means that a Freshman achieved a first-year grade-point average of at least 0.22 points[17] above that which his high-school rank and SAT score predicted for him.

It is evident from the findings presented in Table 3 that grades evaluation was positively related to overachievement, as defined above. Moreover, and in strong testimony to the utility of longitudinal techniques, this table shows that the prediction of overachievement by direction of grades-evaluation *change* was better than prediction by *either* grades-evaluation observation taken singly.

The main general conclusions from the findings shown so far are as follows: (1) change in the importance that Freshmen gave to getting good grades was related to sociability predisposition and, therefore, perhaps to peer-group interpersonal association as well; (2) grades-evaluation change was related to grade-point achievement, independently of the latter's relationship to high-school rank and SAT score; and (3) change in grades evaluation was more closely associated with actual Freshman-year grade-point achievement than was state of grades evaluation.

TABLE 3

ASSOCIATION (YULE'S Q) OF FRESHMAN "OVERACHIEVEMENT" WITH SEPTEMBER GRADES EVALUATION, NOVEMBER GRADES EVALUATION AND GRADES-EVALUATION CHANGE

	Males	Females
September grades evaluation.	−0.13 (128)	−0.16 (151)
November grades evaluation. . ..	+ 28 (109)	+ .37 (130)
Grades evaluation		
Turnover	− .04 (109)	+ .05 (130)
Decrease	− 22 (83)	− 21 (98)
Increase...... .	+0 50 (26)	+0 84 (32)

Grades-evaluation, interpersonal environments.—Up to this point, the survey data have been relied upon to build up suggestive evidence for the hypothesis that certain attitudinal changes among Freshmen were the result chiefly of the socializing influences of non-Freshmen. The data on interpersonal environments provide the most direct examination of this idea. The first type of question asked of these data was: Did the sheer number of students whom a respondent said he knew have a relationship to his grades-evaluation change? Did the sex and college-class distribution of students known by a respondent have such a relationship? The relevant data are shown in Table 4.

[16] See Joshua A. Fishman and Ann K. Pasanella, "College Admission-Selection Studies," *Review of Educational Research*, XXX (1960), 298-310.

[17] The college used a three-point grading system.

The analysis of effects of having different proportions of Freshmen and non-Freshmen in one's IE depends, of course, upon the fact that non-Freshmen tended to have lower grades evaluations than did Freshmen and on the inference that, on the average, this difference will be reflected in interpersonal environments. Given this assumption, Table 4 supports the hypothesis of socialization by showing that (1) turnover in grades evaluation was more sensitive to the proportion of non-Freshmen of his own sex in his IE than it was to the proportion of Freshmen; and (2) the higher the proportion of own-sex non-Freshmen, the more likely a Freshman was to lower his grades evaluation toward the non-Freshmen level, while the reverse was the case for the proportion of Freshmen. As further elaboration of the hypotheses, it was found that the more students a Freshman knew, the more his grades evaluation declined. But of greater interest is the observation that the directions of Q's—relating grades-evaluation change to the total IE proportions toward which the respondent felt neutral affect on the one hand, and *either* like *or* dislike on the other—indicate that emotional involvement with one's peers, whether this involvement was one of attraction or repulsion, was critical. It would seem that aloofness and emotional distance from one's peers provided strong insulation against their social influence—an insulation which neither "like" nor "dislike" for them provided.

The foregoing examination of effects of relative IE quantities of non-Freshman and Freshman males and females treats all non-Freshman boys, for example, as though they shared the same low grades evaluation. Since it is known that this was not, in fact, the case, it is conceivable that the findings in this connection might reflect not the hypothesized impacts of non-Freshman and Freshman grades evaluations but some third variable that induced Freshmen differentially to change their grades evaluations *and also* to have different IE proportions of Freshmen and non-Freshmen. The credibility of this alternative hypothesis can be reduced if it can be shown that not only the ***amounts*** but the ***kinds*** of Freshmen and non-Freshmen (in terms of their attitudes toward grades) whom a Freshman included in his IE was related to his grades-evaluation change.

In examining the effects of different grades evaluations among the same non-Freshman boys, for example, the familiar statistical computation of difference between observed and expected values was employed, asking whether the observed proportion of non-Freshman boys in a given IE who had a high grades evaluation was larger than the corresponding proportion among all non-Freshman boys (i.e., the "expected" proportion). If so, that IE segment was labeled as a positively biased sample of all non-Freshman boys, and the correlation between IE grades-evaluation bias and respondent grades-evaluation change was calculated. By following this

TABLE 4

ASSOCIATIONS (YULE'S Q) BETWEEN FRESHMAN GRADES-EVALUATION CHANGE AND TOTAL NUMBER OF RECOGNITIONS AND SELECTED PROPORTION IN FRESHMEN'S IE'S* (ALL FRESHMEN)

	GRADES-EVALUATION CHANGE		
	Turnover	Decrease	Increase
Total no. of recognitions	+0 24	+0 22	−0 45
Proportions of total IE which was			
Liked...	+ 23	+ 36	− 36
Neutral	− 28	− 37	+ 37
Disliked	+ 15	+ 24	− 14
Interacted with more than six hours per week	+ 14	+ 19	− 22
Own-sex Freshmen	− 01	− 29	+ 69
Own-sex non-Freshmen	+0 26	+0 27	−0 09
N	(269)	(200)	(69)

* Number of recognitions and IE proportions dichotomized at their medians within sex of respondent. E.g., the median number of recognitions by Freshman male respondents was 137 (range = 10–357); the median number of recognitions by Freshman female respondents was 184 (range = 27–377).

procedure, all IE segments were treated as though they were the same size over all respondents in order to isolate the effect of grades-evaluation quality in these segments. The results of this analysis upheld the hypothesis by showing that IE grades-evaluation bias was strongly and positively associated with respondent grades-evaluation change and that the bias of own-sex non-Freshmen had greater effect on respondent change than did that of own-sex Freshmen.[18]

TABLE 5

FRESHMAN GRADES-EVALUATION CHANGE BY CHANGES IN GRADES-EVALUATION BIAS OF FRESHMAN IE SEGMENTS (PER CENT)

	CHANGES IN GRADES-EVALUATION BIAS OF OWN-SEX FRESHMAN IE SEGMENTS			
	High Stable	Increase	Decrease	Low Stable
Per cent all Freshmen having high grades evaluations in				
Sept.	78	71	69	78
Nov.	55	43	37	35
N	(60)	(35)	(78)	(98)
Freshman grades-evaluation change*	−30	−39	−46	−55

* Change measured by the "Effectiveness Index" described in Carl I. Hovland, Arthur A. Lumsdaine, and Fred D. Sheffield, *Experiments on Mass Communication* (Princeton, N.J.: Princeton University Press, 1949), pp. 284–92. This measure is the increase or decrease in per cent between two time-periods, taken as a per cent of the total possible increase or decrease. Thus, for the data in the first column (78 — 55)/78 = —30.

In discussing the tests for non-Freshman influence on Freshmen, it has been assumed that non-Freshmen maintained stable grades evaluations between the beginning of the academic year and the end of the third week of November. Such an assumption seems plausible if the idea is accepted that most Sophomores', Juniors', and Seniors' adaptations to college life had already occurred during their Freshman college years and that by now their attitudes toward college had reached a stability reinforced by peer-group sanctions. But, obviously, no such assumption is warranted in dealing with the influence of Freshmen since our evidence shows that Freshmen were rapidly changing their attitudes during the first several weeks of college. Therefore, in order to estimate the influence of Freshman acquaintances on Freshman respondents' grades-evaluation changes, concurrent changes in the attitudes of these acquaintances must be taken into account.

In Table 5, IE's are classified into four categories according to the way in which the Freshmen in them had changed their grades evaluations between September and November. It should be emphasized that any given Freshman can figure in the analysis represented by Table 5 once as a respondent whose grades-evaluation change is the dependent variable and one or more times as an IE member whose grades-evaluation change is the independent variable.

It can readily be seen from Table 5 that Freshman respondents whose Freshman acquaintances were relatively low on grades evaluation at both times ("low stable") decreased their grades evaluations much more than did respondents whose Freshman

[18] For all Freshman respondents, the associations between grades-evaluation bias of own-sex Freshman IE segments and respondent grades-evaluation decrease and increase were —0.27 and 0.42, respectively. The corresponding associations between bias of own-sex non-Freshman IE segments and respondent decrease and increase were —0.46 and 0.68, respectively.

acquaintances were high at both times ("high stable"). Further, the changing IE types fall between these extremes, and their differences are in the expected order. Thus, it may be concluded from Table 5 that Freshmen tended to change their own grades evaluations according to the way in which their Freshman acquaintances changed theirs.

These findings elaborate the evidences of Freshman socialization by non-Freshmen by demonstrating the supplementary role played by Freshmen in their own socialization. In this connection, our data show that Freshmen varied widely in the extent of their direct contact with non-Freshmen. For example, the per cent own-sex non-Freshman of Freshman respondents' total IE's ranged from 2 to 56 per cent. This means that some Freshmen (because up to 56 per cent of all students they knew were non-Freshmen) received a great deal of non-Freshman influence directly, while other Freshmen seem to have had very little direct contact with, and influence from, non-Freshmen. The latter Freshmen, though sociometrically more insular, do not seem to have been attitudinally insulated from non-Freshman influence that must have filtered through to them indirectly, via their more "cosmopolitan" Freshman acquaintances. Thus, the growth of Freshman-to-Freshman influence seems gradually to have taken the same general direction as the originating non-Freshman-to-Freshman influence. The emergence of such group self-socialization would seem to be of very great importance to the perpetuation of student culture, given the organizational permanence of generational distinctions among the college classes (a permanence that bars complete assimilation) and given a situation where most Freshmen have fewer direct contacts with non-Freshmen than with Freshmen. The "moment" when attitudinal currents flowing (both as a result of internal predispositions and induction by the external host culture) within the newcomer collectivity begin their accelerating swing toward a single general direction—coalescing into a genuine culture of interrelated norms and generating group sanctions and group self-socialization—would seem to be a recurrent and researchable crisis in the life of a college.

At this point, a basic objection can be raised to this interpretation of associations between social environment measures and respondent changes in attitudes. That is, such associations may reflect *selection of acquaintances* by respondents rather than socialization of respondents by their acquaintances. Considering that other data not presented here indicate some opposition between a task orientation (as reflected in the grades and extra-curricular values) and a socioemotional orientation (as reflected in the dating and friendship values) toward college, the selection hypothesis may be stated in two parts, as follows: (1) Freshmen with a strong sociability predisposition in September will have proportionately fewer high grades-evaluation non-Freshmen in their November IE's than will Freshmen of weak sociability orientation; and (2) if sociability predisposition is controlled, the IE grades-evaluation bias effect will disappear, leaving only the negative sociability effect.

TABLE 6

ASSOCIATIONS (YULE'S Q) BETWEEN FRESHMAN GRADES-EVALUATION CHANGE AND GRADES-EVALUATION BIAS IN OWN-SEX NON-FRESHMEN IE SEGMENT, BY SOCIABILITY PREDISPOSITION (ALL FRESHMEN)

Grades-Evaluation Change	Sociability Predisposition	
	High	Moderate-Low
Decrease	−0.54 (147)	−0.01 (54)
Increase	+0.68 (46)	+0.64 (23)

The first part of the selection hypothesis is upheld by uniformly negative associations, ranging from −0.20 to −0.54, between sociability predisposition and grades-evaluation bias of non-Freshman IE segments. The second part of the hypothesis,

however, is contravened by the findings presented in Table 6.

Here it may be seen that the stronger a respondent's sociability predisposition the more his own grades evaluation changed toward conformity with that of his non-Freshman acquaintances, whether the latters' grades evaluations were relatively high or low. This means that sociability predisposition, despite its important role in selecting acquaintances, also heightened a Freshman's sensitivity and responsiveness to the attitudes of those acquaintances, whatever the content of those attitudes. It is important to note, however, that Table 6 shows that, while a strong desire to be accepted and liked by particular non-Freshmen who placed relatively high emphasis on grades strongly inhibited the typical tendency to lower one's grades evaluation, it did not contribute to positively counteracting this tendency—in the sense of facilitating an increase in grades evaluation.

Graduate aspirations, survey and interpersonal environmental.—At the same time that the importance assigned to getting good grades was declining among Freshmen, the desire to go to graduate or professional school after college was increasing among them. This latter change also brought Freshmen into line with attitudes prevailing among non-Freshmen. Thus, in September, the per cent wanting to go to graduate or professional school was 49 per cent among Freshman males and 27 per cent among Freshman females. By November, these percentages had risen to 70 and 37 per cent, clearly toward the non-Freshman male and female percentages of 80 and 50 per cent.

Without minimizing the importance of the opposition between the grades-evaluation and graduate aspiration trends,[19] it should be noted that the change at the individual level did not fly as much in the face of presumable student knowledge that high grades are required for admission to graduate school as the trend data would suggest. Table 7 indicates that only 10 per cent of the boys and 5 per cent of the girls actually decreased their grades evaluation while simultaneously increasing their graduate aspirations. Some 41 per cent of the boys and also of the girls changed neither attitude, and 46 and 52 per cent of the boys and girls, respectively, changed only one of them. It would appear, then, that trends in the two attitudes were largely independent of one another at the individual level. Let us therefore inquire into the graduate-aspirations change on its own terms before commenting on its relationship to grades-evaluation change.[20]

In Table 8, some antecedent survey correlates of the graduate-aspiration change are shown. Here, in contrast with similar data on the antecedents of grades-evaluation change (see Table 1), consistent and qualitative sex differences are found. In fact, the direction of relationships among the Freshman men are, with one exception, reversed among women. For example, while sociability predisposition was positively related to increase of graduate aspiration among men, it was negatively related to such increase among women. Even in the case of occupational choice, where the relationship was not reversed, it is clear that the positive bearing of this variable on

[19] Edward L. McDill and James Coleman (in "High School Social Status, College Plans, and Academic Achievement," *American Sociological Review*, XXVIII [December, 1963], 905–18) report a similar phenomenon among high-school students with regard to their attitudes toward academic achievement in high school and their aspirations to attend college.

[20] Another study, reported in James A. Davis and Norman Bradburn, *Great Aspirations: Career Plans of America's June 1961 College Graduates* (Chicago: National Opinion Research Center, 1961), included students at our subject college in a national sample of graduating college Seniors This study found that 31 per cent of the graduating boys and 19 per cent of the graduating girls at this college who reported that they had not planned to go to graduate school at the time they were entering Freshmen nevertheless said they expected to attend such a school in the fall of 1961. This strongly suggests that the Freshman graduate aspiration changes that we are discussing had important long-range behavioral consequences.

TABLE 7

FRESHMAN CHANGE IN GRADES EVALUATION BY CHANGE IN GRADUATE ASPIRATION

(Per Cent)

GRADUATE ASPIRATION CHANGE	GRADES-EVALUATION CHANGE: Increase	High Stable	Low Stable	Decrease	Total
			Males		
Increase	2	8	3	10	23
High stable	3	25	5	14	47
Low stable	5	5	6	11	27
Decrease	......	2	....	2	4
Total	10	40	14	37	101 (124)
			Females		
Increase	......	3	4	5	12
High stable	1	10	6	6	23
Low stable	4	14	11	32	61
Decrease	.....	2	.....	3	5
Total	5	29	21	46	101 (145)

TABLE 8

SOME ANTECEDENT VARIABLES ASSOCIATED WITH FRESHMAN GRADUATE-ASPIRATION CHANGE (YULE'S Q)

ANTECEDENT VARIABLES	MALES: Turnover	MALES: Decrease	MALES: Increase	FEMALES: Turnover	FEMALES: Decrease	FEMALES: Increase
Importance of being accepted and liked by other students	+0.04 (143)	−0.73 (70)	0.20 (73)	+0.25 (170)	0.62 (45)	−0.03 (125)
Importance of falling in love and getting married	+ .19 (43)	− .29 (70)	.19 (73)	− .03 (170)	.62 (45)	− .21 (125)
Importance of pleasing parents	+ .08 (143)	.00 (70)	.24 (73)	− .20 (170)	.52 (45)	− .45 (125)
Occupational choice*	−0.37 (75)	−0.81 (46)	0.32 (29)	−0.21 (91)	0.64 (29)	+0.74 (62)

* The index of occupations used here is presented in Otis Dudley Duncan, "A Socioeconomic Index for All Occupations," in Albert J. Reiss, Jr. (ed.), *Occupations and Social Status* (Glencoe, Ill.: Free Press, 1961). This index was arrived at by substituting age adjusted census data on the near income (X_2) and education (X_3) of each census occupation in the following regression equation: $X_1 = 0.59X_2 + 0.55X_3 - 6.0$. Students who reported no occupational choice are not included.

graduate-aspiration change was much stronger among men than among women.

In general, therefore, Table 8 suggests that graduate aspirations carried different meanings for male and female students. For one thing, it appears that for a Freshman girl to decide that she wanted to go to graduate or professional school after college required a large measure of independence from parents and from the still typically feminine success-via-marriage-and-family formula. Among Freshman men, however, graduate aspirations appear to have been supported rather than squelched by the desire to please one's parents, as well as by the desire to establish a family of one's own. Thus, a major dimension of difference between the meanings attached to graduate aspirations by male and female students is drawn: For the male student, such aspirations conformed to parental expectations and to wider social criteria of adult status validation; for the female student, graduate aspirations were more nonconformist and perhaps more consciously rebellious. In this light, the initially puzzling findings of Table 8 regarding the opposite bearings of sociability predispositions on the graduate aspirations changes of Freshman men and women become more understandable, perhaps as one further indication of the generally non-conformist nature of feminine graduate aspirations. But such an explanation, as far as it goes, still leaves the fact of graduate aspiration increase among both Freshman men and women unaccounted for, and one must look elsewhere than predispositions to account for this.

Table 9 shows the degrees to which Freshman graduate-aspiration changes were associated with the levels of graduate aspirations in four segments of their IE's. The most important findings to note here are (1) the graduate aspirations of non-Freshman males had positive effects on the aspirations of both male and female Freshmen; and (2) the only other clear evidence of positive influence on graduate aspirations appears to be that of Freshman women on Freshman men.

The first finding means that the principal sources of peer-group influence toward raising both Freshman males' and Freshman females' graduate aspirations were non-Freshman boys. Thus, peer-group influence does not seem to have been as simply within-sex on this variable as on grades evaluation. In fact, it might be guessed that the high levels of graduate aspiration among non-Freshman women were less cultural, in the sense of being supported by the women's own social system, and rather more individual, sustained by personal attitudes of rebellion against the traditional feminine family role and by the presence of career-bound Senior men as a reference group.

The second finding, of Freshman female influence on Freshman males, also underscores the complexity and impact of intersex relations that seem bound up in graduate aspirations. In this way, our findings regarding this variable all seem to point to the following question: Why should inter-

TABLE 9

ASSOCIATIONS BETWEEN FRESHMAN GRADUATE-ASPIRATION CHANGE AND GRADUATE-ASPIRATION BIAS IN IE SEGMENTS (YULE'S Q)

IE SEGMENT	MALES		FEMALES	
	Decrease	Increase	Decrease	Increase
Freshman males	+0.44 (69)	−0.07 (73)	−0.20 (43)	−0.12 (123)
Freshman females	− .48 (68)	+ .30 (73)	− .19 (45)	− .14 (125)
Non-Freshman males	− .41 (69)	+ .36 (73)	− .17 (45)	+ .46 (125)
Non-Freshman females	+0.35 (63)	+0 32 (72)	+0.62 (45)	+0.41 (125)

sex relations play such an important part in graduate aspiration changes and such an unimportant part in grades-evaluation changes?

In tentative reply to this question and also to the problem of opposing trends in grades evaluation and graduate aspiration, it shall be argued (1) that a Freshman's attitude toward getting good college grades and his attitude toward going to graduate school after college are logically different kinds of attitudes, and (2) that different influentials affect different attitudes of a given influencee.

A student's grades evaluation is an attitude most relevant to the specific institutional context of the student's present life, while his graduate aspiration is an attitude more relevant to his orientation to life in general and particularly to his future, adult life. The desire to go to graduate school helps to define such an orientation insofar as it makes life goals definite by selecting a general means to accomplish them. Grades evaluation may, therefore, be thought of as an indicator primarily of the college student's orientation to his college career, while graduate aspiration may be thought of as indicative primarily of his orientation to his life career. Or, to put this another way, a college student's grades evaluation tells us about what kind of *student* he is or wants to be while his graduate aspiration tells us more about what kind of *adult* he wants to become. It seems quite clear that at this college, the typical respondent wanted (1) to be a student socially integrated with his peers and freer of superordinate control (via grades) by adult faculty and staff members; and (2) to become a well-educated adult of superordinate social status. The conflict between the trends in grades evaluation and in graduate aspiration can therefore be resolved in terms of a single desire for generational independence—and perhaps also personal autonomy —now and in the future.[21]

But it is clear that once past the level of generality represented by the "typical student," sex-role differentiation must be taken into account, regarding adult role aspirations and consequently also regarding student role aspirations. In other words, since sex-role differentiation directs females toward socioemotional achievement through marriage and family, girls who aspire to attend graduate school may manifest a generalized rebellion against the things that traditional culture demands of women, and perhaps also a degree of repulsion from the women who conform to these demands. To the extent that non-Freshman women fell into the latter category (by virtue, perhaps, of their heavy emphasis on dating as a value), Freshman women would not seem likely to be influenced toward stronger graduate aspirations by them.

But since Freshman girls often interacted with non-Freshman boys[22] (whom the girls probably regarded as prestigeful, free from the cultural constraints that hobble the aspirations of women, and among whom graduate aspirations were at a high level), it follows that some of these girls may have adopted non-Freshman boys as a reference group for their own adult career aspirations.

The Freshman boys, on the other hand, show the equally intriguing effect of Freshman girls on their graduate aspirations. This may be interpreted as an attempt by Freshman boys to compensate for their low

[21] The discussion here owes much to informal conversation with James S. Coleman some years ago and to McDill and Coleman's conclusion that among high-school students "college promises adult status, but scholastic achievement carries the connotation of acquiescence and subordination to adults" (*op. cit.*, p. 918).

[22] The median percentage of non-Freshman males in Freshman girls' IE's was 11 per cent, while the median percentage of non-Freshman females in Freshman boys' IE's was 5 per cent. The median changes in these percentages between November and April were + 9 and + 4 per cent, respectively. Much of the interaction between Freshman girls and non-Freshman boys was dating. By April, Freshman girls were spending more time on dates, proportionately, than was any other class of girls, while Freshman boys spent less time dating than did any other class of boys.

organizational and age status, which probably made it difficult for them to get dates with Freshman girls (not to mention non-Freshman girls),[23] by certifying that they too would become high-status, desirable men. One might well imagine here a genesis of a "woman behind the man" relationship in which the man is pressed toward higher and more concrete career goals than he might otherwise have, in order to keep up with the sexual competition.

Some speculations regarding interpersonal influence.—Having thus supported and elaborated the central hypotheses as well as the data seem to allow, consideration can now be given to one further question relating to interpersonal influence.

The usual technique for measuring interpersonal influence in a large social group begins by asking respondents to name their "best" or "close" friends.[24] In the present research, Rossi's interpersonal environment technique[25] was employed. The general problem under consideration focuses on the relative merits of these two approaches.

Underlying both there seems to be a conceptualization, analogous to that of a gravitational field, that sees ego as the center of a social environment composed of alters, some of whom are socially more "distant" and can exert less influence on him and some of whom are "closer" and can exert more powerful influence. In Rossi's technique, an attempt is made to measure social distance on scales of attractiveness and frequency of interaction as seen by ego. In the best or close friends technique, social distance is not measured but controlled, since ego is asked to name only those alters to whom he feels closest. It would appear that the reasonabless of this control rests on the asumption that personal influence on ego varies inversely and continuously with alter's social distance. It is this assumption that the interpersonal environment technique and the present data question and raise as an hypothesis worthy of direct test.

Although both the "best friends" and interpersonal environment techniques were not employed in this study, data were collected on the frequency and attractiveness of interaction, as seen by the respondent, with each IE member. Given the social-distance conceptualization of interpersonal environments, one may assume that students whom a respondent would have named as closest or best friends on campus were more likely to be included among those students whom he said he liked and spent relatively more time with than among students named in any other attractiveness-by-frequency category. If the hypothesis of inverse relation between amount of influence and social distance is valid, then ego should manifest the greatest degree of grades-evaluation homophily with this part of his IE. (Unfortunately, the case base is too small to measure the bearing of this part of IE's on grades-evaluation change.)

The data in Table 10, however, are quite different from this prediction. There is almost no relationship between the respondent's grades evaluation and that of his own-sex, frequent-interaction, liked, non-Freshman IE segment, but a much stronger positive relationship is found with that part of the same segment that was liked but with which less time was spent.

The interpretation that comes to mind runs as follows: An interpersonal environment as a whole involves less selection by an individual than does that part with which he interacts most and likes. A college Freshman, for example, seems to be rather restricted in the choice of persons with whom he will come into contact, since he is assigned a dormitory, a dormitory floor and room, most of his classes and some of his classroom seats, as well as a few non-

[23] Note the boys' positive association between the importance of falling in love and getting married and increase in graduate aspiration (Table 8).

[24] For examples, see Bernard R. Berelson, Paul F. Lazarsfeld, and William N. McPhee, *Voting* (Chicago: University of Chicago Press, 1954), esp. pp. 93–103, and Seymour Martin Lipset, Martin Trow, and James Coleman, *Union Democracy* (Glencoe, Ill.: Free Press, 1956), esp. pp. 339–40.

[25] Rossi, *op. cit.*

Freshman personal contacts ("big brothers" or "big sisters") by college administrators, instructors, Greek-letter rush committees, and the like. But given the persons with whom he is thus brought into contact, a Freshman is more free to decide which of them he likes and will spend more time with than is organizationally required.

It is suggested that for an adolescent college Freshman, the main criteria of such friend selection and the main influence of the resulting friendships may not be on attitudes relevant to his transitional institutional life as a student but rather on those attitudes that most directly bear upon the larger and often more burning problems of developing an orientation to life in general; problems of becoming an adult in an adult world; problems, in short, of life-cycle, rather than institutional, socialization. Thus, within the inner circle of a Freshman's interpersonal environment, the emphasis assigned to getting good grades, for example, may not be an important influence dimension, simply because such problems may not be the most salient faced by Freshmen considered not as college students but as persons in late adolescent stages of socialization. What may be far more salient and more relevant—especially in a Freshman's friendship with older, more experienced non-Freshmen—are attitudes toward life goals, parents, religion, sex, politics, friendship, and the like. Outside this inner circle, however, the principal dimensions of influence may shift toward problems salient to college Freshmen as students—problems less complex, more immediately practical, and more specific to life in the temporary institutional context.

A comparison of Table 11 with Table 10 shows exactly what would be expected if grades evaluation and graduate aspiration

TABLE 10

ASSOCIATIONS BETWEEN FRESHMAN GRADES EVALUATION IN NOVEMBER WITH GRADES-EVALUATION BIAS IN NON-FRESHMAN ATTRACTION-BY-FREQUENCY IE SEGMENTS (YULE'S Q)

Males		Females	
Non-Freshman Male IE Segment	Q	Non-Freshman Female IE Segment	Q
Liked and more than 6 hours ...	−0 14 (57)	Liked and more than 6 hours ..	−0 11 (54)
Liked and 6 hours or less	+ 46 (122)	Liked and 6 hours or less	+ .66 (157)
Neutral and 6 hours or less.	+ 30 (119)	Neutral and 6 hours or less .	+ 24 (144)
Disliked and 6 hours or less...	+0 06 (76)	Disliked and 6 hours or less . .	+0 37 (83)

TABLE 11

ASSOCIATIONS BETWEEN FRESHMAN GRADUATE ASPIRATION IN NOVEMBER WITH GRADUATE ASPIRATION BIAS IN NON-FRESHMAN IE SEGMENTS (YULE'S Q)

Males		Females	
Non-Freshman Male IE Segment	Q	Non-Freshman Female IE Segment	Q
Liked and more than 6 hours	+0.32 (65)	Liked and more than 6 hours ..	0 27 (68)
Liked and 6 hours or less. . .	+ 13 (140)	Liked and 6 hours or less.	47 (169)
Neutral and 6 hours or less.....	− 22 (136)	Neutral and 6 hours or less .	22 (164)
Disliked and 6 hours or less.....	+0.15 (93)	Disliked and 6 hours or less.....	0 26 (69)

were, in fact, the logically different attitudes that they have been suggested to be and if life-orientation problems were more relevant within the inner circle of an adolescent Freshman's interpersonal environment than they are outside this circle. Whereas Table 10 showed that Freshman grades evaluations were least related to the biases of own-sex non-Freshmen who were liked and with whom relatively much time was spent, Table 11 indicates the reverse among freshman boys in the case of graduate aspirations. Even among the girls only the liked, frequent interaction, non-Freshman female IE category shows stronger homophily on graduate aspirations than grades evaluations.

Essentially, the above hypothesis is simply a specification of the idea that the influence of one person on another may be attitudinally specific or general,[26] but it would imply an important distinction between (1) the organizational culture in which a given participant finds himself and (2) that participant's most proximate interpersonal environment in the organization, as independent variables predicting his change along different attitudinal dimensions. Although the hypothesis would account for the findings of this study, it clearly goes much beyond them and calls for a specific test where this data can provide only *post hoc* suggestion.

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[26] See the discussion of differences between positional and personal identification and between socialization and identification in Robert F. Winch, *Identification and Its Familial Determinants* (Indianapolis: Bobbs-Merrill, 1963), pp. 15-25. The suggestion being made is also closely related to Blau's distinction between the bearing of "friendly relations between fellow-officials" of a government law enforcement agency upon organizationally prescribed performance and the bearing of "intimate friendships" on questions of wider than organizational significance (see Peter M. Blau, *The Dynamics of Bureaucracy* [Chicago: University of Chicago Press, 1955], pp. 132-40). Where Blau, however, emphasizes on-going organizational roles as the differentiating factor, we would add extra-organizational, as well as intra-organizational, and anticipated as well as present, roles as differentiators. This addition arises from the fact that this study deals with adolescents in college rather than adults on the job and, therefore, with a preparatory rather than consummatory life phase passed within an avowedly transition-facilitating organization.

Sociomedical Variations among Ethnic Groups[1]

Edward A. Suchman

ABSTRACT

A comparison of different ethnic groups in New York City reveals significant variations in knowledge about disease, attitudes toward medical care, and behavior during illness. These differences are found to be related to the form of social organization within the ethnic groups. The more ethnocentric and socially cohesive the group on a community, friendship, or family level, the more likely are its members to display low knowledge about disease, skepticism toward professional medical care, and dependency during illness. In general, form of social organization is found to be more important than ethnicity or social class in relation to sociomedical responses.

The influence of cultural background upon a society's definition of illness and appropriate illness behavior has been well documented by numerous social and anthropological field studies.[2] In general, these studies have shown that the perception and definition of illness, the functions it serves, the medical care sought, and the adjustments made are rooted in social-group factors—religious beliefs, group values, family organization, and child-rearing practices. Zborowski, for example, studied variations in reactions to pain among Jewish and Italian patients and found that Jewish patients were more concerned with the meaning and consequence of their symptoms, while Italian patients primarily sought relief from pain.[3] This preoccupation of the Jewish group with symptoms of illness is supported in a study by Croog, who, in a comparison of army inductees, found that Jews at all educational levels reported the greatest number of symptoms.[4] While explanations for these observed differences in illness-related attitudes and behavior are usually offered in terms of an ethnic group's traditional cultural patterns,[5] very few studies have actually attempted to control for such factors in comparing variations in illness responses among ethnic groups. In one of the few studies utilizing such controls, Mechanic found little support for the hypothesis that differences in illness behavior between Jewish

[1] This investigation was supported in whole by Public Health Service Grant CH00015 from the Division of Community Health Services. Field work was done in co-operation with the Washington Heights Master Sample Survey, Columbia University School of Public Health and Administrative Medicine, supported by the Health Research Council of the City of New York under contract U-1053, Jack Elinson, principal investigator. Lois Alksne, Edward Wellin, Margaret C. Klem, and Sylvia Gilliam (deceased) played a major role in the planning of this project, while field work and analysis were aided by Marvin Belkin, Martin Goldman, Martin Smolin, Raymond Maurice, and Daniel Rosenblatt. John Colombotos and Annette Perrin O'Hare were in charge of interviewing, with Regina Loewenstein responsible for sampling and data-processing.

[2] See, e.g., Benjamin D. Paul (ed.), *Health, Culture and Community* (New York: Russell Sage Foundation, 1955); George M. Foster, *Problems in Intercultural Health Programs* (Social Science Research Council Pamphlet 12 [New York: Social Science Research Council, 1958]); Steven Polgar, "Health and Human Behavior: Areas of Interest Common to the Social and Medical Sciences," *Current Anthropology,* III (April, 1962), 159–205.

[3] Mark Zborowski, "Cultural Components in Responses to Pain," in E. Gartly Jaco (ed.), *Patients, Physicians, and Illness* (Glencoe, Ill.: Free Press, 1958), pp. 256–68.

[4] Sydney H. Croog, "Ethnic Origins, Educational Level, and Responses to a Health Questionnaire." *Human Organization,* XX (Summer, 1961), 65–69.

[5] Mark Zborowski and E. Herzog, *Life Is with People* (New York: International Universities Press, 1952); Lyle Saunders, *Cultural Differences and Medical Care* (New York: Russell Sage Foundation, 1954).

and Catholic students could be explained in terms of varying degrees of religiosity.[6]

The purpose of this report is to examine ethnic variations in health-related knowledge, attitudes, and behavior in terms of the different forms of social organization found among the different ethnic groups in an urban community. While anthropological surveys have amply documented cross-cultural variations in relation to health and illness, much less is known about such differences within a single community.[7] "Society," especially complex, modern, mass society, is not a single homogeneous group of people but is compounded of many varieties of overlapping subgroups with different attributes and intensities of cohesion. *It is our major hypothesis that, within a community with as heterogeneous an ethnic composition as New York City, significant differences will be found among ethnic subgroups in responses to illness and medical care and that, furthermore, these differences will be associated with variations in the form of social organization of the ethnic groups.*

To test this hypothesis, we propose (1) to determine how ethnic groups vary in their responses to illness and medical care, (2) to analyze these ethnic groups for differences in form of social organization, and (3) to relate any significant differences in social organization to the observed variations in sociomedical factors in an attempt to determine the extent to which such ethnic variation can be attributed to underlying differences in social organization.

METHOD OF PROCEDURE

This study is based upon information obtained by personal interviews with a representative cross-section of adults, twenty-one years of age or over, living in the Washington Heights community of New York City. Data were obtained for a probability sample of 5,340 persons comprising some 2,215 families by means of household interviews conducted from November, 1960, through April, 1961.

The first interview was conducted with an adult member of the family, usually the female head of the household. This interview obtained the basic demographic data, including ethnicity, for all members of the household, an inventory of all chronic conditions and impairments, and a record of all medically attended illnesses experienced by any family member during the past year. All adult members of this initial sample were then listed, and a random sample of 1,883 respondents was selected for a more detailed interview on medical knowledge, attitudes, and behavior. This is the sample upon which the current report is based. A weighted completion rate of over 90 per cent was obtained from all eligible respondents.[8]

According to the 1960 Census, the Washington Heights community contains about 100,000 dwelling units comprising approximately 270,000 people. Because there is a great deal of ethnic variation within the community, it was possible to study sizable samples of several different ethnic groups. The per cent non-white in the community has increased rapidly since 1930 until non-whites now constitute about a quarter of the total population. Foreign-born whites represent another quarter of the population. This proportion has stayed

[6] David Mechanic, "Religion, Religiosity, and Illness Behavior: The Special Case of the Jews," *Human Organization*, XXII (Fall, 1963), 202–8.

[7] Examples of several such studies would include Beatrice Berle, *Eighty Puerto Rican Families in New York City* (New York: Columbia University Press, 1958); Earl C. Koos, *The Health of Regionville* (New York: Columbia University Press, 1954); Leo Srole *et al.*, *Mental Health in the Metropolis* (New York: McGraw-Hill Book Co., 1962).

[8] The first round of interviewing yielded a return of 73 per cent from the eligible dwelling units. A random subsample of one-third of the remaining dwelling units was then selected, and the responses of the interviewers was given triple weight. A detailed comparison of the final sample with the 1960 Census on all available demographic characteristics revealed no major category differing by more than 3 per cent. For details of sampling and census comparisons, see Jack Elinson and Regina Loewenstein, *Community Fact Book for Washington Heights* (New York: School of Public Health and Administrative Medicine, 1963).

fairly constant since 1930, when the predominant foreign country of birth was Russia. The predominant foreign country of birth in 1950 was Germany. Today, the influx of Puerto Rican migrants to New York City has resulted in a sizable number of Puerto Ricans in the community. The increase in the non-white population is complemented by a decrease in the native-born white population, which currently makes up about one-half the population of the district.[9]

Formation of ethnic groupings.—The three major ethnic characteristics chosen for comparison were race, religion, and country of origin. At first these were to be analyzed separately, but it immediately became apparent that they were so closely interrelated as to make a separate analysis meaningless. Negroes were predominantly native-born Protestants, while Puerto Ricans were "foreign"-born Catholics. Because of this inherent overlap of race, religion, and country of birth, we have formulated the following six categories as representing the major ethnic subgroups in our study community.[10]

Ethnic Group	No.	Per Cent
Negro	442	25
Puerto Rican-born	170	9
White:		
Jewish	490	27
Protestant	165	9
Catholic	354	20
Irish-born Catholic	174	10
Total	1,795*	100

* Ethnicity could not be determined accurately in eight cases of the total sample of 1,883.

The above classification eliminates overlapping categories between race, religion, and country of birth and permits a comparison of the six major ethnic groups residing in the study community. These six subgroups, in our opinion, constitute meaningful sociocultural entities with diverse cultural traditions and social structures and among which we may expect to find differences in health-related knowledge, attitudes, and behavior.

FINDINGS

The ethnic groups included in our sample were asked a series of questions dealing with various aspects of health, illness, and medical care. These questions tapped three major areas of medical concern—(1) knowledge of disease and its prevention; (2) attitudes toward medical care; and (3) responses to illness. In each area, two indexes were developed using Guttman scale analysis techniques as follows:

Knowledge of disease and its prevention:
  Knowledge about disease
  Preventive medical behavior
Attitudes to medical care:
  Skepticism of medical care
  Physician's interest in patient's welfare
Responses to illness:
  Acceptance of sick role
  Dependency in illness.[11]

[9] Lee A. Lendt, *A Social History of Washington Heights, New York City* (New York: Columbia-Washington Heights Community Mental Health Project, February, 1960).

[10] The meaning of ethnicity in a society with a "melting-pot" tradition such as the United States has is, of course, complex. Lines of racial and national origin are apt to be crossed, and it is extremely doubtful that we can speak of ethnic-group membership in any biological sense. This is of little consequence for a study of responses to illness, since we view these ethnic-group labels mainly as indicative of sociocultural differences, and by "Negro" or "Puerto Rican" we mean, for the most part, a tendency for individuals who hold these characteristics in different degrees also to share common sets of values, norms, attitudes, etc., and to be more or less subject to similar living conditions and social experiences. It is our supposition that, as these shared values and common experiences change, so will the responses of the ethnic individuals to illness and medical care.

[11] These indexes were based upon the following specific items:

The "knowledge about disease" score was compiled by scoring 1 point for each right answer to thirteen questions on etiology, treatment, and prognosis of different diseases. The "preventive medical behavior" index was based upon the number of "yes" responses to the following three questions: (1) "Do you get periodical medical checkups when you are not ill?" (2) "Have you had any polio shots yet?" (3) "Are you very careful to see that you

The observed differences are presented in Table 1 and show quite conclusively that ethnic differences do occur in relation to each of these sociomedical factors.

To summarize, we find that in regard to "knowledge about disease," Puerto Ricans are least informed (48.2 per cent receiving a "low" score), while white Protestants are best informed (only 18.1 per cent scoring "low"). On a measure of "preventive medical behavior," the Puerto Ricans again score lowest, with the Jews and Protestants scoring highest (20.1 versus 10.5 per cent and 14.6 per cent "low," respectively). In regard to attitudes toward medical care, the Puerto Ricans score highest on "skepticism of medical care," while Protestants score lowest (38.2 versus 12.7 per cent "high skepticism," respectively). However, only slight differences occur in relation to an index of "physician's interest in patient's welfare" with all ethnic groups being quite similar except for the Jewish group, which has a significantly negative attitude in this respect. An analysis of responses to illness shows the Puerto Rican group having the greatest difficulty in "acceptance of sick role," while the Irish-Catholic group shows the highest "dependency in illness."

In general, it would seem that the greatest ethnic-group contrast in regard to sociomedical factors occurs between the Puerto Ricans on the one hand and the white Protestants and Jews on the other. In most aspects of health knowledge, attitudes, and behavior, the Puerto Rican group stands

TABLE 1

RELATIONSHIP BETWEEN ETHNICITY AND HEALTH-RELATED INDEXES

(Per Cent)

	Negro	Puerto Rican	White				Total
			Protestant	Catholic	Jewish	Irish	
Knowledge about disease:							
Low score	29.5	48.2	18.1	26.4	26.2	28.3	28.6
Preventive medical behavior:							
Low	12.8	20.1	14.6	21.2	10.5	15.6	15 0
Skepticism of medical care:							
High	23.1	38.2	12.7	23.2	16.9	17.2	21.3
Physician's interest in patient's welfare:							
Low interest	18.1	18.2	18.8	16 7	26.5	13.2	19.7
Acceptance of sick role:							
Low	40.5	50.0	42.4	44.9	37.4	50.6	42.6
Dependency in illness:							
High	26.2	37.7	17.0	31 1	20 0	34 5	26 5
Total cases*	(442)	(170)	(165)	(354)	(490)	(174)	(1,795)

* Total no. of cases in each table may vary slightly depending upon frequency of "no answer" category.

eat a balanced diet?" The "skepticism of medical care" index was based on responses (agree = "high" skepticism) to (1) "I believe in trying out different doctors to find which one I think will give me the best care," (2) "When I am ill, I demand to know all the details of what is being done to me," (3) "I have my doubts about some things doctors say they can do for you." The index of "physician's interest in patient's welfare" asked for responses to (1) "Most doctors charge too much money" (agree = negative), (2) "Most doctors are more interested in the welfare of their patients than in anything else" (disagree = negative). The index of "acceptance of sick role" was based on responses (disagree = "high" acceptance) to (1) "I find it very hard to give in and go to bed when I am sick," (2) "I usually try to get up too soon after I have been sick." The "dependency in illness" index asked for responses (agree = "high" dependency) to (1) "When I think I am getting sick, I find it comforting to talk to someone about it," (2) "When a person starts getting well, it is hard to give up having people do things for him."

out as most divorced from the objectives and methods of modern medicine and public health, while the Protestants and Jews are most in accord with them. This finding would help explain why the Puerto Ricans, and to a lesser extent the Negroes, constitute the core of the "hard-to-reach" groups in public health and medical care.[12]

The interpretation of the meaning and significance of the above differences in ethnic responses to illness has been discussed in a separate report and need not concern us further.[13] Suffice it to say that, in the present study, ethnic groups have been found to vary significantly on a series of measures indicative of health knowledge, attitudes, and behavior. Our problem now is to try to determine to what extent the observed ethnic differences in sociomedical factors are due to variations in the social organization of these groups. We hypothesize that the more ethnocentric and cohesive the social group, the more isolated and alienated it will be from the larger society and the less likely it will be to accept the objectives and methods of the formal medical care system.

In this study, we have developed five main indexes of social organization based upon degree of "in-group" identification. These indexes deal with the individual's friendship groups, his family, and his community relationship and may be identified as (1) ethnic exclusivity, (2) friendship solidarity, (3) social-group cohesiveness, (4) family tradition and authority orientation, (5) religious attendance.[14]

Table 2 shows the differences among ethnic groups for these specific indexes of social organization. In regard to "ethnic exclusivity" and "religious attendance," the Jewish and Protestant groups show the least amount of ethnic solidarity, while the Puerto Ricans and Catholics show the most, with Negroes falling in between. When we turn to the closeness of the individual's relationship with his own particular friendship group, we again find that the Puerto Ricans and Irish tend to belong to friendship groups which may be characterized as highly cohesive while the Protestants, Jews, and Negroes belong to rather loose friendship groups. Finally, in regard to the authority structure of the family, the Puerto Ricans and Irish also show the strongest "orientation toward tradition and

[12] Berle, *op. cit.*; Paul B. Cornely and Stanley K. Bigman, "Some Considerations in Changing Health Attitudes," *Children*, X (January-February, 1963), 23-28.

[13] Edward A. Suchman, *Socio-Cultural Variations in Illness and Medical Care* (New York: New York City Health Department, 1963).

[14] Each of these indexes is based upon combined scores for the following items:

Index of "ethnic exclusivity" is based on responses (agree = "high" exclusivity) to (1) "The parents of most of my friends come from the same country as my parents come from," (2) "I prefer to deal in stores where clerks are the same kind of people as we are." The index of "friendship solidarity" asked for responses (agree = "high" solidarity) to (1) "Almost all my friends are people I grew up with," (2) "Most of my close friends are also friends with each other," (3) "Most of my friends have the same religion as I do," (4) "Most of my friends come from families who know each other well." The "social-group cohesiveness" index is based on reactions to (1) "We are interested in how different people organize their social lives. I am going to ask you to tell me about your friends; you don't have to give me their full names, just the first name or nickname will do, so that we can keep them straight. Would you tell me the first names or nicknames of the people you see most often socially? Start with the person closest to you"; (2) "Are any of these people related to you?" (3) "Do all these people know one another well? Which ones do *not* know which others?" (Enter two "X's" in the proper places in the matrix); (4) "Now about No. 1 . . . does he like No. 2 *very much*, does he like him only *somewhat*, or is he *indifferent* to him?" (Enter "V," very much, "S," somewhat, or "I," indifferent, in matrix. Continue for all relationships.) The "family tradition and authority orientation" index is based on responses (agree = "high" orientation) to (1) "Everybody in my family usually does what the head of the house says without question," (2) "My family usually waits until the head of the house is present before we have dinner," (3) "In my family we think the old-time customs and traditions are important." The index for "religious attendance" asked (1) "About how often do you go to (church) (synagogue) (services)?" (2) "Did you happen to go to (church) (synagogue) (services) last week?"

authority" as compared to the Protestants and the Jews.

From what we know of the social organization of the various ethnic groups in New York City, the differences observed above in the degree and type of social integration appear valid. Other studies have noted the strong ethnocentric ties of the Puerto Rican migrants to New York City and their relative "isolation" from the mainstream of American affairs.[15] Their high degree of social integration probably reflects both this isolation and the less cosmopolitan nature of the Puerto Rican society from which they have migrated. Similarly, the lower religiosity of the Protestant and Jewish groups reflect basic differences in the values and norms of these groups as compared to the Catholics, both native and foreign-born.

The ambivalent position of the Negro in American society is also documented by our findings.[16] While the members of the Negro group tend to be isolated from the majority group, they have not developed the strong sense of ethnic identity of the Puerto Ricans. Their social-group ties are also weaker than the other minority groups as are their attachments to the family. Thus they appear to live as members of the larger American society but lack many of its social supports.

Given these findings—(1) ethnic groups vary on health knowledge, attitudes, and behavior, and (2) ethnic groups vary in social organization—we may now proceed to test the extent to which ethnic variations in health-related variables are due to underlying differences in social organization. For purposes of this analysis, we have developed combined scores of social organization and of sociomedical responses. A multivariate analysis of the five indexes of social organization and the six indexes of sociomedical responses indicated that reliable and valid combinations could be made of the following sets of indexes:

TABLE 2

RELATIONSHIP BETWEEN ETHNICITY AND INDEXES OF SOCIAL-GROUP ORGANIZATION (Per Cent)

	Negro	Puerto Rican	White				Total
			Protestant	Catholic	Jewish	Irish	
Ethnic exclusivity: High	18.1	36 5	9.7	12.4	6 9	15 5	14 7
Friendship solidarity: High	29.2	56.5	15.2	42.6	33.1	56 9	36 8
Social-group cohesiveness: High	27 5	37.6	29.3	32.7	19.6	33.7	27.7
Family orientation to tradition and authority: High	24.0	43.4	16 9	32.9	20.2	42 4	27.8
Religious attendance: High	23.2	43.5	16.0	48.9	11.1	80.8	32.1
Total cases	(442)	(170)	(165)	(354)	(490)	(174)	(1,795)

Social organization:

1. Ethnic exclusivity
2. Friendship solidarity
3. Family orientation to tradition and authority

[15] Nathan Glazer and Daniel M. Moynihan, *Beyond the Melting Pot* (Cambridge, Mass.: M.I.T. Press and Harvard University Press, 1963).

[16] Gunnar Myrdal, *An American Dilemma* (New York: Harper & Bros., 1944).

Sociomedical responses:
1. Knowledge about disease
2. Skepticism of medical care
3. Dependency in illness[17]

The combined index of social organization indicates the degree to which the individual comes from a social group that may be characterized as homogeneous and highly cohesive. We have labeled this dimension as "cosmopolitanism-parochialism," with the cosmopolitan end of the scale indicating heterogeneous and loosely knit interpersonal relationships while the narrowly confined, and closely knit "in-group" point of view, as opposed to a cosmopolitan or more worldly, progressive, "urban" or less personal way of life.

The combined index of sociomedical variables indicates the degree to which the individual maintains an informed, favorable, and independent approach to illness and medical care. This dimension we have labeled as a "scientific-popular" health orientation with the scientific end of the scale indicating an objective, formal, professional, independent approach while the popular

TABLE 3

RELATIONSHIP BETWEEN ETHNICITY, SOCIAL ORGANIZATION, AND HEALTH ORIENTATION

Ethnicity	No.	Per Cent "Parochial" Social Organization	Per Cent "Popular" Health Orientation	Per Cent "Popular" Health Orientation, by Social Organization		
				Cosmopolitan	Mixed	Parochial
Puerto Rican ..	170	60 6	51 8	24 0 (25)	35 7 (42)	65 0 (103)
Negro....... .	442	29 0	27 0	18 0 (128)	23 2 (185)	41.4 (128)
White:						
Catholic.......	354	33 3	27.1	13 7 (80)	23 1 (156)	41 5 (118)
Irish	174	48 9	24.7	12 9 (31)	20 7 (58)	31 8 (85)
Protestant ...	165	12.7	15 2	7 5 (80)	15 6 (64)	42 9 (21)
Jewish........	490	24.5	16.5	9 1 (186)	17.4 (184)	26 7 (120)

parochial end indicates homogeneous and closely knit interpersonal relationships.[18] This measure may be taken to indicate the degree of identification of an individual with a parochial or limited, traditional, end indicates a subjective, informal, lay, dependent health orientation.[19] It is our hypothesis that a cosmopolitan form of social organization will be more highly related to a scientific approach to illness and medical care than a parochial social organization, which will be more highly related to a popular health orientation. Thus we would predict that the more parochial an ethnic group is, the more likely it is that its members will adhere to a popular or non-scientific health orientation.

Table 3 reveals highly significant differ-

[17] Matrix tables presenting the intercorrelations of all indexes are given in Edward A. Suchman, *Social Patterns of Health and Medical Care* (New York: New York City Department of Health, 1963).

[18] Similar characterizations of social structure may be found in Eliot Freidson, *Patients' Views of Medical Practice* (New York: Russell Sage Foundation, 1961); Alvin W. Gouldner, "Cosmopolitans and Locals: Toward an Analysis of Latent Social Roles—I," *Administrative Science Quarterly*, II (December, 1957), 281–306; Robert Merton, "Patterns of Influence and of Communications Behavior in a Local Community," in Paul Lazarsfeld and Frank Stanton (eds.), *Communications Research, 1948–1949* (New York: Harper & Bros., 1949), pp. 180–219.

[19] Discussions of the difference between a scientific and popular health orientation may be found in Stanley King, *Perceptions of Illness and Medical Practice* (New York: Russell Sage Foundation, 1962), pp. 91–120; Lyle Saunders and Gordon W. Howes, "Folk Medicine and Medical Practice," *Journal of Medical Education*, XXVIII (September, 1953), 43–46.

ences in both social organization and health orientation among the various ethnic groups. The Puerto Rican group is highly parochial while the white Protestant and Jewish groups are highly cosmopolitan. The Irish Catholic group is also highly parochial, as are other Catholics, while the Negro group is more inclined toward cosmopolitanism than parochialism. As hypothesized, differences in health orientation parallel these differences in social organization with the Puerto Ricans, the most highly parochial group, being twice as likely to have a popular health orientation as any of the other ethnic groups. Probably this high combination of parochialism and popular health orientation among Puerto Ricans reflects both the social structure and health culture of their country of origin reinforced by their currently low socioeconomic status and minority-group treatment in the United States. White Protestants and Jews, the most cosmopolitan of the ethnic groups, are also the most scientific in their approach to health and medical care.

Looking at the effect of social organization on health orientation within each ethnic group, we find that in each case higher parochialism is associated with a more popular or non-scientific health orientation.[20] This relationship is highest among white Protestants and Jews and lowest among the Puerto Ricans. Thus we conclude that the relationship between social organization and health orientation is independent of ethnic-group membership. Both ethnicity and form of social organization contribute independently and cumulatively to health orientation with the popular approach being followed least by the cosmopolitan Protestants (7.5 per cent) and most adhered to by the parochial Puerto Ricans (65.0 per cent).

A final comparison may be made combining both the demographic variables of socioeconomic status and ethnicity. Ethnicity is, of course, related to social class, with the Puerto Ricans and Negroes belonging predominantly to the lower socioeconomic level, while the white Protestants and Jews come from the upper socioeconomic level. Looking at socioeconomic status[21] and ethnicity simultaneously in Table 4 only serves to increase the social-group differences, with both social class and ethnicity being independently related to social organization and health orientation. Lower-class Puerto Ricans are both most parochial and most popular-health oriented while upper-class white Protestants and Jews are most cosmopolitan and scientific in their approach to health and medical care.

When we look at the relationship between social organization and health orientation for the combined ethnic and social-class groups, we see (Table 5) that within each ethnic and socioeconomic group parochialism continues to be associated with a

[20] It has been suggested that this relationship may be an artifact of an "acquiescent response set." The use of scale score instead of individual item correlations decreases this possibility. More important, the observed ethnic-group variations and the relationship between social organization and health orientation holds for such non-attitudinal indexes as social-group cohesion based on number of close friends in one's friendship group, religious attendance, knowledge of illness based on informational questions, and preventive medical behavior based on actual behavioral items.

[21] The socioeconomic status index was formed from the person's education, occupation, and total family income as follows. Education was divided into five categories: some college, high-school graduate, some high school, grammar-school graduate, and some grammar school. Occupation was divided into four categories: professional and managerial; clerical and sales; craftsmen and operators; and household, service workers, etc. Total family income was divided into four categories $7,500 plus, $5,000–$7,500, $3,000–$5,000 and less than $3,000. These were scored and distributed on an index which ranged from a score of 13 for highest SES to 3 for lowest SES. Where information was not ascertained for one of the three index components, the score was based upon a linear interpolation of the remaining two components. Ethnic differences in socioeconomic status were as follows: Per cent "upper" and "upper middle"—Jews (66.8 per cent); Protestants (60.0 per cent); Irish (49.4 per cent); Catholics (44.6 per cent); Negroes (38.6 per cent); and Puerto Ricans (27.3).

TABLE 4

SOCIAL ORGANIZATION AND HEALTH ACCORDING TO SOCIOECONOMIC STATUS AND ETHNICITY

ETHNICITY	SOCIOECONOMIC STATUS			
	Upper	Upper Middle	Lower Middle	Lower
	Per Cent "Parochial" Social Organization			
Puerto Rican..	*	59 0 (39)	55.4 (83)	73 0 (37)
Negro.........	16 2 (37)	23 6 (123)	26 9 (171)	40.5 (84)
White:				
Catholic.....	17.2 (29)	17.9 (123)	43.7 (128)	55.7 (61)
Protestant...	2.9 (35)	9.8 (61)	21 6 (51)	*
Jewish......	20.8 (101)	16.7 (210)	34 5 (110)	43 2 (44)
Irish........	40.0 (20)	36.9 (65)	50.0 (60)	71.8 (27)
	Per Cent "Popular" Health Orientation			
Puerto Rican..	*	33 3 (39)	53.0 (83)	70 3 (37)
Negro.........	13.5 (37)	21.1 (123)	24.7 (170)	47 6 (84)
White:				
Catholic.....	27.6 (29)	20.3 (123)	25.0 (128)	44.3 (61)
Protestant...	2.9 (35)	13.1 (61)	27.5 (51)	*
Jewish......	8.9 (101)	13.8 (210)	24.5 (110)	29 5 (44)
Irish........	5.0 (20)	20.0 (65)	31.7 (60)	37.0 (27)

* Less than 15 cases.

TABLE 5

RELATIONSHIP BETWEEN SOCIAL ORGANIZATION AND HEALTH ORIENTATION ACCORDING TO SOCIOECONOMIC STATUS AND ETHNICITY

ETHNICITY	PER CENT "POPULAR" HEALTH ORIENTATION					
	Upper Socioeconomic Groups			Lower Socioeconomic Groups		
	Cosmopolitan	Mixed	Parochial	Cosmopolitan	Mixed	Parochial
Puerto Rican....	*	*	46 4 (28)	28.6 (14)	42 4 (33)	71 2 (73)
Negro...........	18.5 (54)	15.5 (71)	28.6 (35)	18 1 (72)	30 4 (102)	47.5 (80)
White:						
Catholic.......	16.1 (56)	20 3 (69)	37.0 (27)	9.1 (22)	24 7 (77)	42.2 (90)
Protestant.....	5.4 (56)	9.1 (33)	*	15.0 (20)	23 3 (30)	42 9 (14)
Jewish........	8.5 (142)	12.4 (113)	21.4 (56)	13.5 (37)	26 7 (60)	33 3 (57)
Irish..........	14.3 (21)	15.6 (32)	18.7 (32)	10.0 (10)	26 9 (26)	41.2 (51)

* Less than 10 cases.

popular or non-scientific health orientation. Thus, for example, at the same time that Puerto Ricans *as a group* are more parochial and more popular-health oriented than Protestants and Jews, within each of these groups the more parochial an individual is, the more likely is he to be popular-health oriented. Our extreme contrasting groups now become lower-class, parochial Puerto Ricans, 71.2 per cent of whom hold a popular-health orientation, and upper-class, cosmopolitan Protestants, only 5.4 per cent of whom have a popular-health orientation.

Looking at Table 5, we note a possible interaction effect, with social-class differences being quite pronounced among the parochial groups but small and irregular among the cosmopolitan groups. It would appear that social-class variations in health orientation are, in part, a function of the type of social organization of the group. Ethnic differences decrease greatly in importance once social class and parochial-cosmopolitanism are controlled. With the single exception of the Puerto Rican group, which represents a rather special case of relatively recent removal from a folk culture, only minor ethnic variations occur within the separate social-class and parochial-cosmopolitan groups.

On the basis of these findings, we may now reformulate our initial hypotheses as follows:

1. Ethnic groups differ in regard to sociomedical variables of knowledge about disease, attitudes to medical care, and responses to illness.
2. But (*a*) ethnic groups also differ in form of social organization; and (*b*) social organization also relates to the sociomedical variables.
3. Therefore, we now determine the way in which ethnicity and social organization are interrelated in their effect upon the sociomedical factors. We test two possible models of "causation": (*a*) Ethnicity leads to sociocultural differences which in turn lead to sociomedical variations, or (*b*) both ethnicity and sociocultural factors independently (and hence cumulatively) effect sociomedical variations.

In general, it would appear that form of social organization transcends the mere fact of ethnic-group membership in determining sociomedical variations. While ethnicity and social class are both independent contributing factors to parochial-cosmopolitanism, it is this latter variable which continues to show the highest and most consistent relationships to health orientation. Thus, our findings would point toward alternative (*a*) as more closely fitting the available data.

In the above analysis we have dealt with two combined indexes of social organization and health orientation. We now return to a brief discussion of the specific variables included in these general indexes since each one does tap a somewhat different and significant aspect of social organization and health orientation. Space limitations will require us to touch only on the main findings and to forego any detailed statistical presentation.

In regard to knowledge about disease, we find that, for all ethnic groups in general, a lack of information is associated with higher ethnic exclusivity, friendship-group solidarity, and family orientation to tradition and authority. It would thus appear that the stronger the individual's social ties, the lower his level of health knowledge. We can only speculate that such group support tends to decrease the need for a cognitive understanding of disease. It is as if the search for facts about disease provided an alternative form of security for those individuals with weak community, social, and family ties. On the other hand, it is also possible that high social-group integration and low health knowledge are both reflections of a common low level of acceptance of modern social change. Support for this explanation may be seen in the particularly large differences in health knowledge among those individuals who are highly parochial as compared to those who are highly cosmopolitan.

In general, strong in-group allegiances are also associated with a higher degree of skepticism toward medical care. Again it would seem that the stronger the social ties of an individual, the greater his distrust of outside influences, including medical care. It would appear that the individual's support from and dependence upon his social group acts somewhat as a barrier to seeking professional medical care outside the group.

The above skepticism does not seem to extend in the same manner to an evaluation of the individual physician's interest in the welfare of his patients. Among all groups, more favorable evaluations of the physician-patient relationship are registered by those individuals who belong to social groups with high solidarity and who come from families with a high orientation to tradition and authority. This is the opposite direction from the previous finding on skepticism of medical care. It would seem that stronger in-group identification becomes associated with greater distrust of the medical-care system at the same time that it becomes more favorable toward the individual physician. This could be a reflection of differing attitudes toward impersonalized, official medical organization contrasted to personalized, private care. Strong social-group allegiances appear to oppose the former and favor the latter, perhaps indicating the greater value placed upon close interpersonal relationships by those individuals who belong to the more highly integrated social groups.

It is in relation to family orientation to tradition and authority that we find the most significant relationship between social organization and acceptance of the sick role. The higher the degree of family orientation to tradition and authority, the greater the difficulty in assuming the sick role. This is particularly true for the Irish, Puerto Ricans, and Negroes. It would seem that, in general, strong family authority acts against giving in to illness. It may be that the obligations of a family member to carry on his normal activities in the face of illness is felt most strongly within families with a high orientation toward tradition and authority.[22]

Significant, and consistent, relationships are found between illness dependency and social organization for all ethnic groups. In all cases lower dependency in illness is associated with lower ethnic exclusivity, friendship-group solidarity, and family orientation to tradition and authority. Regardless of ethnic-group membership, it would seem that the degree of support one seeks and secures from one's social group is influenced by the degree of social integration of the group. The more cohesive the group, the greater the dependency of the individual upon it for support during illness.

There is an interesting reversal, however, between social-group support and family support. We find, for each ethnic group, that strong social-group integration seems to make it easier for one to turn to one's group for help, while strong family authority seems to make it more difficult to seek help within the family. As a possible explanation of this reversal, we suggest that the relinquishing of one's normal responsibilities in illness constitutes more of a threat and disruption to one's family than to one's social group. The individual usually is more necessary to the functioning of the family than to his social group, and hence the family may make it more difficult for him to assume a dependency in illness than does the social group. In other words, the reversal may reflect the fact that a close-knit family is more likely to *demand* support from the adult individual while a close-knit social group is more likely to *give* support to the individual.

The independent and cumulative nature

[22] To some extent, the presence of a sick individual in the family presents a threat to the harmony of the family (see Talcott Parsons and Renee Fox, "Illness, Therapy, and the Modern Urban American Family," *Journal of Social Issues*, VIII [1952], 31–44; see also Clark E. Vincent, "The Family in Health and Illness: Some Neglected Areas," *Annals of the American Academy of Political and Social Science*, CCCXLVI [March, 1963], 109–16).

of the contribution of both ethnicity and social organization to health orientation noted previously in regard to the combined cosmopolitan-parochial and scientific-popular indexes is supported by each of the individual measures. In all cases, ethnicity and the specific index of social organization continue to be related separately to each of the specific indexes of health orientation. The cumulative differences produced by both factors are illustrated, for example, in Table 6 which contrasts Puerto Ricans in highly cohesive social groups with white Protestants in weakly integrated groups according to knowledge about disease, skepticism of medical care, and dependency in illness.

TABLE 6

COMPARISON OF SOCIOMEDICAL VARIATIONS BETWEEN PUERTO RICANS AND WHITE PROTESTANTS OF CONTRASTING SOCIAL ORGANIZATION

(Per Cent)

	Puerto Ricans ("High")			White Protestants ("Low")		
	Ethnic Exclusivity	Friendship Solidarity	Family Authority	Ethnic Exclusivity	Friendship Solidarity	Family Authority
Low knowledge about disease	66.1	57.3	64.6	15.8	14.1	10.8
High skepticism of medical care	48.4	41.7	44.6	5.3	7.1	7.7
Low dependency in illness	9.7	8.3	9.2	47.4	45.5	49.2
No. of cases	(62)	(96)	(65)	(95)	(99)	(65)

This comparison of sociomedical responses between Puerto Ricans and Protestants belonging to groups with contrasting forms of social organization highlights one of the main implications of this study for the field of medical sociology. Of all the so-called ethnic "minorities" (a term which requires a special interpretation in New York City where such sociological "minorities" often constitute numerical majorities), Puerto Ricans show the greatest deviation from what might be evaluated as "desirable" sociomedical knowledge, attitudes, and responses to illness. The Puerto Rican–born individual lies on one end of a continuum of ethnic variations in relation to health and medical care while we find the native-born white Protestant at the other. The Negro group tends to resemble the Puerto Ricans, while the Jewish group is closer to the Protestants.

Thus, it would seem that individuals and groups ranking high in parochialism find it more difficult to accept the type of highly organized and formal medical-care system to be found in New York City. A conflict exists between a highly bureaucratic administrative system of medical care and a large segment of the population more at home with personalized care.[28] Puerto Ricans, being in general more parochial than other ethnic groups, appear to have the greatest difficulty in adapting themselves to the modern "scientific" as opposed to a "folk" approach to medical care. In a sense, social isolation seems to breed "medical" isolation. The generally restricted outlook and lower expectations of the socially withdrawn groups find expression in narrower health horizons.

This statement of the problem of providing medical care for the minority groups in our large cities—groups, incidentally, most in need of such care—may help to explain the resistance of such groups to health programs in terms of their general alienation from the dominant American

[28] Herbert Gans, *The Urban Villagers* (Glencoe, Ill.: Free Press, 1962), p. 277; Ozzie Simmons, *Social Status and Public Health* (New York: Social Science Research Council, 1958), pp. 23–29.

society.[24] Their underutilization of modern medical facilities and their lack of co-operation in community health programs may be simply one more expression of their general estrangement from the mainstream of middle-class American society. In this respect, "medical" disorganization among these groups becomes another form of social disorganization. Health problems constitute an inherent aspect of the larger social problem of poverty and social deprivation. It is doubtful that the barriers that now interfere with effective medical care for this "second America" can be removed except as barriers to full participation in other aspects of American society are also removed.

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[24] See Daniel Rosenblatt and Edward A. Suchman, "The Underutilization of Medical Care Services by Blue Collarites," and "Blue Collar Attitudes and Information toward Health and Illness," *Blue Collar World*, ed. A. Shostak and W. Gomberg (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1964).

Economic Correlates of Artistic Creativity

Vytautas Kavolis

ABSTRACT

A survey of economic correlates of historical fluctuations in artistic creativity suggests two generalizations. First, the motivation for artistic creativity is maximized by a balanced tension between the needs for achievement and for expression in the personality system. This type of motivation is most likely to be produced in relatively early stages of increasing prosperity. Second, social demand for art tends to be stimulated, in a minor mode, by disturbances of socioeconomic latency and, in a major mode, by the necessity for social-emotional reintegration of a social system subsequent to intensive adaptive activity. From these observations a general sociological theory of artistic creativity is derived.

Studies of the relationship between economic conditions and artistic creativity[1] have generally been based on somewhat imprecise historical data from the urban cultures of classical and modern Europe. They suggest the presence of a direct relationship between economic achievement and artistic creativity.[2] It may be argued, however, that the virtual exclusion of the data from preliterate cultures, together with inadequate attention to intervening variables, have frequently resulted in generalizations about the economic backgrounds of artistic creativity which lack both universality and theoretical significance.

In the present survey, a tentative attempt will be made to deal with this relationship in a somewhat wider perspective, and to formulate a conception of the mechanisms responsible for the observed associations between economic and artistic conditions. The processes to be related to artistic creativity will be conceptualized within an interpretive framework derived from the sociological theory of the phase cycles.[3]

THE PHASE-CYCLES THEORY

A general statement of the phase-cycles theory may be formulated in the following manner:

1. All social systems have a number of basic functional problems to solve. Among these, instrumental *adaptation* to the external environment, organization of the legal-institutional machinery for *goal attainment,* internal social-emotional *integration,* and *tension reduction* (and latent pattern maintenance) are of paramount importance.

2. Social systems produce only limited amounts of social resources (such as wealth, administrative power, disposable time, and popular interest) and, in addition, may be able to mobilize only a part of the resources for problem-solving action. Furthermore, specialization with regard to any particular system problem tends to some extent to circumscribe, even if resources are available, the possibility of action oriented toward the solution of any

[1] "Artistic creativity" is defined as referring to (*a*) an increase in the quality of art production in a definable socio-historical unit and/or (*b*) the culmination(s) of artistic attainment of such a unit, as judged by a reasonably general consensus of artists and art critics which, in most cases, eventually tends to emerge as "historical judgment."

[2] Shepard B. Clough, *The Rise and Fall of Civilization: An Inquiry into the Relationship between Economic Development and Civilization* (New York, 1961), p. 261.

[3] Robert F. Bales, "The Equilibrium Problem in Small Groups," in Talcott Parsons, Robert F. Bales, and Edward A. Shils, *Working Papers in the Theory of Action* (Glencoe, Ill., 1953), pp. 111-61; Talcott Parsons and Neil J. Smelser, *Economy and Society: A Study in the Integration of Economic and Social Theory* (Glencoe, Ill., 1956), pp. 242 43.

other problem. For example, when the emphasis is on tension reduction, some of the potential resources for creative action are likely to be unproductively dissipated, by virtue of the negative effects of a prevalent "latent tension-management and pattern-maintenance" orientation on the readiness to commit resources to purposive action.

3. For these reasons, basic system problems tend to be dealt with in a cyclical manner. Major emphasis is placed on one type of problem, to the relative neglect of others, at any particular time. As a functional problem is being successfully resolved, resources are increasingly recommitted to previously neglected tasks which the solution of this problem has made salient. The process can be conceptualized in terms of several typical phase sequences which can be directly observed in small-group studies and also abstracted from complex historical events. In problem-solving processes, the normal phase sequence is as indicated under statement 1.

In terms of this theory, it is hypothesized that artistic creativity will tend to be stimulated in the phase of social-emotional integration,[4] and relatively inhibited in the other phases of any type of large-scale social process. The main body of the present paper consists of an application of this theory to two types of economy-related processes. First, however, to demonstrate the need for such a theory, it must be shown that artistic creativity cannot be explained, even in part, as a natural product or a symbolic expression of concentrated wealth (or of comfortable general prosperity), as some of the existing theories implicitly suggest.

THE DEGREE OF ECONOMIC ACHIEVEMENT

To indicate the dependence of artistic creativity on economic capacity, evidence can be cited to show that an advance in prosperity[5] or the attainment of economic dominance in a culture area[6] is frequently associated with increases in artistic creativity. Contrariwise, a long-range decline in economic activity or prosperity does appear to be associated with a lowering of artistic creativity.[7]

However, these kinds of data may merely mean that changes in economic prosperity are correlated with variations in artistic creativity. No absolute level of prosperity can be identified as necessary for artistic excellence. The cave art of the European

[4] "Because the phenomenon of expressive communication" is related "to the problem of order," the discharge of motivational energy "without reference to adaptive exigencies . . . is a process peculiarly bound up with integrative exigencies" (Talcott Parsons, Robert F. Bales, and Edward A. Shils, "Phase Movement in Relation to Motivation, Symbol Formation, and Role Structure," in Parsons *et al., Working Papers . . .*, p. 190).

[5] In Athens, Pitirim A. Sorokin, *Social and Cultural Dynamics,* III (New York, 1937), 231; in China, Peter C. Swann, *Chinese Painting* (New York, 1958), p. 33; in Egypt, A. L. Kroeber, *Configurations of Culture Growth* (Berkeley, Calif., 1949), p. 240; in Mesopotamia, *Encyclopedia of World Art* (hereinafter cited as "*EWA*"), I, 856; in Rome, France, and Germany, Pitirim A. Sorokin, *Society, Culture, and Personality: Their Structure and Dynamics* (New York, 1947), pp. 549–50; in the Romanesque age and in Renaissance Italy, Arnold Hauser, *The Social History of Art,* I (New York, 1957), 183, and II, 12–13.

[6] In the Hellenistic age, Clough, *op. cit.*, p. 114; in the Italian Renaissance, Hauser, *op. cit.*, II, 29; in the fifteenth-century Low Countries, Kroeber, *op. cit.*, p. 363; in seventeenth-century Holland, George Edmundson, *History of Holland* (Cambridge, 1922), pp. 120–21, 186, 298. The British efflorescence of the eighteenth century (when Dutch art was stagnant) was preceded by the ascendancy of London over Amsterdam as the center of "international financial business" (G. N. Clark, *The Seventeenth Century* [2d ed.; Oxford, 1957], p. 45).

[7] Jean Gimpel, *The Cathedral Builders* (New York, 1961), pp. 40, 176, 178; Richard Ettinghausen, *Arab Painting* (Lausanne, 1962), p. 183. The economic welfare of Benin state, based in part on slave trade, declined in the seventeenth and eighteenth centuries, and Benin art "flourished from the 16th to the 17th century" (Roland Oliver and J. D. Fage, *A Short History of Africa* [Baltimore, 1962], pp. 122–23); *EWA*, III, 380. "In fact, historical evidence supports the proposition that . . . declines in civilization have accompanied falls in abundance per capita" (Shepard B. Clough, *Basic Values of Western Civilization* [New York, 1960], p. 55).

Paleolithic hunters, the rock art of South Africa, and the powerful image-making of the Australian natives are all associated with quite limited material resources.[8] "In Oceania . . . life is often very precarious indeed," especially "in the swampy areas of New Guinea or in the very inhospitable coral islands," yet it is "these areas in particular" which "have yielded some of the finest works in the whole of primitive art."[9]

As Redfield has observed, "such a contrast as that between the Haida and the Paiute Indians reminds us that generally speaking a people desperately concerned with getting a living cannot develop a rich moral or aesthetic life."[10] This generalization, however, points to the artistically adverse effects *not* of a low-attainment economy, but of a near-total preoccupation with economically instrumental action. Such a preoccupation may be a consequence of a low-attainment economy in areas with difficult access to natural resources, as in the American Plains before the introduction of the horse. But where a low-attainment economy is combined with more easily accessible natural resources, as in many parts of Oceania (and on the Northwest Coast of America), economic backwardness may coexist with high-level artistic creativity.[11] On comparable levels of economic development, a cultural emphasis on the accumulation of wealth appears likely to preclude an adequate commitment of social resources to artistic pursuits, as a comparison of the Tchambuli, the artistic headhunters, with the Kapauku, the capitalistic businessmen, of New Guinea suggests.[12]

Our illustrative comparisons imply that it is not the level of economic achievement, but the proportion of social resources which is allocated to non-instrumental pursuits that is causally related to artistic creativity. Artistic achievement is not proportional to the amount of wealth accumulated; nor is it impossible in the absence of a considerable economic surplus. Art *creation* is hence not primarily a symbolic projection of material prosperity (though self-conscious art *collecting* may serve this purpose).[13]

THE EFFECTS OF ECONOMIC ADVANCEMENT

While, in a static cross-cultural comparison, prosperity is not a prerequisite of artistic creativity, the latter seems to be stimulated by *recently attained* prosperity. The accumulation of urban wealth in Europe, beginning in the eleventh century, is followed in the twelfth and thirteenth centuries—an age of "magnificent prosperity" —by the period of Gothic cathedral-building.[14] The creation of the great fortunes in America during the nineteenth century is succeeded in the twentieth by the first original American contributions to visual art.[15]

[8] Hans-Georg Bandi, Henri Breuil, Lilo Berger-Kirchner, Henri Lhote, Erik Holm, and Andreas Lommel, *The Art of the Stone Age* (New York, 1961).

[9] Alfred Buehler, Terry Barrow, and Charles P. Mountford, *The Art of the South Sea Islands* (New York, 1962), p. 48.

[10] Robert Redfield, *The Primitive World and Its Transformations* (Ithaca, N.Y., 1958), p. 18.

[11] There is, however, a tendency in nomadic cultures which lack both permanent settlements and stable centers of religious activity to develop extensive ceremonial as a functional equivalent of more objectified kinds of art.

[12] Margaret Mead, *Sex and Temperament in Three Primitive Societies* (New York, 1950), pp. 168-70; Leopold Pospisil, *The Kapauku Papuans of West New Guinea* (New York, 1963), pp. 15, 18-31.

[13] Cf. Thorstein Veblen, *The Theory of the Leisure Class* (New York, 1931), pp. 36-37, 45, 74-75. But "the idea of collecting and displaying works of an earlier period was hardly known in those cultures where the need for art was strong and widely diffused" (Kenneth Clark, "Art and Society," *Harper's Magazine*, August, 1961, pp. 77-78).

[14] Gimpel, *op. cit.*, pp. 38-42, 178.

[15] In 1831, de Tocqueville still held "the general mediocrity of fortunes, the absence of superfluous wealth . . . and the constant effort by which everyone attempts to procure [comfort]" responsible for the low level of the arts in the "democratic nations," exemplified by the United States of his

In general, "the flourishing periods of fine art do not come in the periods when a rising upper class is building up its wealth and power, but afterward."[16] One linkage between the recent achievement of prosperity and artistic creativity is the leisure which the former makes possible, as in "Athens in the classic period and Florence during the Renaissance."[17] In this context, artistic creativity could be regarded either as the product of recommitment of resources from instrumental to expressive activity, or what may be only a special case of the former more general tendency, as a means of symbolic legitimation of recently acquired socioeconomic dominance. It does not, however, seem possible to account for this linkage by assuming that, in urban as contrasted with preliterate cultures, artistic creativity is generally a consequence of the accumulation of wealth and might be regarded as a response to the demand for luxury goods, which great wealth presumably generates (at least in non-puritanic cultures).

In some cases, "refined luxury" coincides indeed with "creative spontaneity" in the arts, as in the Warring States and in the late Shang periods of Chinese history.[18] At other times, by stimulating a demand for luxury goods great wealth merely increases the capacity to afford, but not the ability to create, works of artistic value. "The four centuries of the Han *Pax Sinica*, like the four centuries of the Mediterranean *Pax Romana* . . . encouraged the development of a rich material civilization. In both cases the period of creative spontaneity—in the Mediterranean world the apogee of Athens and Alexandria, in China the 'Warring States'—had ended. Civilization passed through a stagnant and apparently happy period in which the luxury arts, on both sides, played a considerable role."[19] "Luxury art," observes an art historian, "has a deadening effect. The most obvious example is the art of eighteenth-century France."[20]

The comparison of the effects of recently attained prosperity and of stabilized great wealth on artistic creativity implies that artistic efflorescences are indeed, to a significant degree, the result of spontaneous recommitment of social resources from instrumental to expressive pursuits, which the achievement of what is felt as *relative* prosperity makes psychologically possible (and perhaps even necessary). Social resources may be committed to expressive action, as in Melanesia, where economic achievement is low, provided that, within the context of the prevalent cultural aspirations, it is felt to be relatively satisfying.[21] However, within more dynamic economies the successful accomplishment of a significant economic advance apparently makes the diversion of social resources to artistic creativity more probable. But it seems to be only in the earlier (and less "luxurious") stages of such diversion that artistic creativity is maximally stimulated. To explain the frequently evident reduction of artistic creativity in the later stages of economic advancement (marked by greater "luxury"),[22] the dynamics of the achievement drive must be considered.

day (Alexis de Tocqueville, *Democracy in America*, II [New York, 1957], p. 50).

[16] Adolph Siegfried Tomars, *Introduction to the Sociology of Art* (Mexico City, 1940), p. 171.

[17] Irwin Edman, "Art," *Encyclopaedia of the Social Sciences*, II, 226.

[18] René Grousset, *Chinese Art and Culture* (New York, 1959), p. 43.

[19] *Ibid.*, p. 103.

[20] Clark, *op. cit.*, p. 77.

[21] In the long run, satisfaction with low economic attainment is likely to be reduced when direct comparisons with more economically productive cultures are possible. This may be implicit in V. Elwin's observation that "the explanation for this relative poverty of artistic inspiration" in the tribal cultures of central India "lies partly in the general depressed state of the populations that for centuries have been subjected to political and economic pressures from economically more advanced peoples . . ." (*EWA*, I, 840).

[22] Sorokin, *Society, Culture, and Personality*, p. 544.

THE DYNAMICS OF ACHIEVEMENT MOTIVATION: SOME PSYCHOLOGICAL EVIDENCE

Evidence of a change in the patterning of individual motivation with increasing posperity is implicit in a series of psychological analyses of fantasy-production samples from four historical societies. On the basis of literary and graphic evidence, Berlew and Aronson have demonstrated the need for achievement to have been strongest during the "growth" stage of classical Greek culture (900–475 B.C.), to have declined during the "climax" (475–362 B.C.), and to have been still further reduced in the "decline" phase (362–100 B.C.).[23] In Spain, the climax in painting can perhaps be placed (if El Greco is to be included) in the period from 1580 to 1660; and four different types of literary indexes of the need for achievement show it to have been declining from 1200 to 1730.[24] However, the first significant growth of modern English painting, in the eighteenth century, coincided with a period of increasing need for achievement—which McClelland attributes to the Wesleyan revival.[25] In their analysis of American children's literature, de Charms and Moeller have demonstrated an increase in the frequency of achievement imagery up to 1890 and a continuing decline in every decade since 1900.[26] In three cases out of four, the attainment of economic prosperity has been associated with a lowering of achievement motivation (and it might have been in England as well, if not for the unpredictably intrusive factor of the Methodist movement). Presumably, in those cases, the level of prosperity attained has been felt to be sufficiently gratifying to legitimate the diversion of some motivational energy toward other kinds of activities, including those of the expressive kind. The decline in achievement motivation is roughly linked with increasing artistic creativity.

However, the Greek and Spanish data suggest that it is in the transitional period of *declining* achievement motivation that artistic creativity may be most stimulated. An extremely low level of achievement motivation, after a period of continuous decline, appears to be associated with a demand for material luxury, but also with a reduction in artistic creativity. This suggests that artistic creativity may in general be linked with changing patterns of motivation,[27] and that, consequently, high-quality art may have socially integrative and psychologically stabilizing functions to perform (which luxury in itself, presumably, does not).

These inferences are borne out by the observations of an economic historian on Renaissance Italy. "After the first decades of the thirteenth century and in the first half of the fourteenth . . . the urban economy of medieval Italy may be said to have entered its prime." But "by the fifteenth century," toward the culmination of the Renaissance, "Italy no longer occupied the same place in the economy of Europe as in the two preceding centuries . . . the old power of expansion was enfeebled," partly because of what seems to be a reduced achievement drive: "the new aristocracy of money" tended by now "to withdraw their capital from industry and trade and invest it, from motives of security and social prestige, in town and country proper-

[23] David C. McClelland, *The Achieving Society* (Princeton, N.J., 1961), pp. 119, 125. The need for achievement is measured by the relative frequency of achievement-related imagery in fantasy productions.

[24] *Ibid.*, p. 131.

[25] However, he believes that this increase in achievement motivation occurred primarily in the "middle and lower classes" to which Methodism had its chief appeal (*ibid.*, pp. 139, 145–49). Achievement motivation may in fact have been declining in the ruling class, which provided the greater part of the audience for such painters as Reynolds and Gainsborough—and lost the American war, though not because of its artistic taste.

[26] *Ibid.*, p. 150.

[27] Cf. the association of role change with artistic interest (Vytautas Kavolis, "A Role Theory of Artistic Interest," *Journal of Social Psychology*, LX [1963], 31–37).

ties." This was, however, a period of continued prosperity, and "during the fifteenth century court life in Italy attained its highest point of splendour."[28] The forfeiture of artistic leadership by the Italians after the High Renaissance may be associated with a hypothetical further decline in achievement motivation.

The evidence presented in this section suggests the general interpretation that increasing prosperity tends to reduce the motivation for achievement and to strengthen that for expressive action; and that it is in the extended moment when the two types of motivation are, in some at present not precisely definable manner, balanced in strength that highest-level artistic achievements are most probable. The theoretical justification of this expectation lies in the natural dependency of creative attainment in art upon the presence, in a strongly developed form, of *both* types of motivation.

With further increase in economic prosperity, however, achievement motivation tends further to decline and expressive motivation to become still stronger. This may be sufficient, on the one hand, to terminate economic advancement[29] (and even to reduce abundance per capita) and, on the other hand, to cause the further growth of a self-conscious interest in art works, as objects of luxury to which no profound emotional significance need be attached. But, in this stage, achievement motivation would seem to become inadequate to sustain creative achievements of the highest order, even in the sphere of artistic expression. Indeed, once this stage is reached, an *increase* in achievement motivation may stimulate the capacity to create great art.

ECONOMIC ACTIVITY AND ARTISTIC CREATIVITY

It is possible to conceive the data surveyed in the previous section in terms of an extremely long-range *psychoeconomic* phase cycle. Within this framework, the historical observations of the achievement-motive school suggest that artistic creativity tends to be inhibited in both the adaptive (pre-prosperity, high-achievement motivation) phase and in that of socioeconomic latency (stabilized wealth, low-achievement motivation). Conversely, it is stimulated in what may be regarded as the integrative phase, following immediately upon the attainment of relative prosperity (when achievement motivation is comparatively balanced against the drive for self-expression).

As contrasted with this psychoeconomic cycle, defined by the motivational accompaniments of economic achievement, a *socioeconomic* phase cycle may be conceptualized on the basis of changes over time in the commitment of social resources to economic action. While the nature of the relationship between the two types of cycles may be viewed as problematic, the phase-cycles model seems applicable, with appropriate modifications, to both types. As will be seen, however, it provides more useful information when applied to the socioeconomic data to be reviewed in this section.

Our theory predicts that the adaptive phase, indicated by sharply increased activity oriented to adaption to the external environment, should be inversely associated with artistic creativity. Two strategic cases will be considered. The decline of the earliest great artistic tradition, that of the European Stone Age, appears to be due, at least in part, to the new need for re-adaptation to the post-glacial environment, which must have absorbed the social resources of the population.[30] In principle, this case is comparable to the sharply increased need for re-adaptation effected by the Industrial Revolution, both in the West and in the developing non-Western societies; and its (possibly transitional) anti-artistic effects[31] are explicable as the prod-

[28] Gino Luzzatto, *An Economic History of Italy from the Fall of the Roman Empire to the Beginning of the Sixteenth Century* (New York, 1961), pp. 85, 142, 146, 143.

[29] McClelland, *op. cit.*, p. 120.

[30] Bandi *et al.*, *op. cit.*, p. 69.

[31] On such effects in Oceania and New Zealand, see Buehler *et al.*, *op. cit.*, pp. 51, 191 "Each of

uct of a radical movement, in the socioeconomic process, into the adaptive phase.

If this is valid reasoning, the period of the industrial transformation in which economic growth proceeds at the most rapid rate should be marked by reduced artistic creativity. Such periods were 1819–48 in Great Britain, 1868–93 in the United States, and 1928–40 in Russia.[82] When the dates of artistic debuts of the most prominent painters of the period are considered, the expectation is borne out for the two European societies,[83] and, possibly though not certainly, for the American as well.[84]

While the most intensive concentration on economic action appears to inhibit artistic creativity, there are indications that the gradual *beginning* of an economic transformation, before it gets into full gear, may have an artistically stimulating effect. Rostow has placed the dates of the "take-offs" to industrialization at 1783–1802 in Great Britain, 1830–60 in France, 1843–60 in the United States, 1850–73 in Germany, and 1890–1914 in Russia.[85] In each case (with the possible exception of Germany), the beginning of a modern artistic efflorescence follows closely upon the take-off.[86] However, the artistically stimulating effect of the disturbance of relative socioeconomic latency by the beginning of a phase of intensive adaptive action tends later to be weakened by a more extreme commitment of social resources to adaptive action.

A second, and major, revival of artistic creativity should follow the transition from the adaptive (and goal-attainment) into the integrative phase of the total social process. Two sets of data will be used for identifying such transitions: (1) the beginning of contraction in the area, value, or amount of foreign trade,[87] and (2) the beginning of a long-range decline in the number of discoveries (in the case of Greece and Rome) or (in all other cases) in the percentage of the sum total of important inventions and discoveries made in a particular nation.[88] The latter index is

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the epoch-making inventions which has caused an 'industrial revolution,' has caused an immediate decline in the artistic level of the pre-existing crafts" (Sir Hubert Llewellyn Smith, *The Economic Laws of Art Production: An Essay towards the Construction of a Missing Chapter of Economics* [London, 1924], pp. 146–47).

[82] W. W. Rostow, *The Stages of Economic Growth: A Non-Communist Manifesto* (Cambridge, England, 1962), p. 40.

[83] In Britain, the "generation of 1775" (approximate average birth date of Turner, Girtin, Constable, Cotman, and Crome) appeared just before the maximum growth period (Aurelien Digeon, *The English School of Painting* [New York, 1955], p. 10). And the next artistic development, the founding of the Pre-Raphaelite Brotherhood, in 1848, coincided with its termination. The only contributions of Russia to modern visual art were all made in the first two decades of the twentieth century, although some of them were subsequently further developed, by expatriated artists, in the West (Camilla Gray, *The Great Experiment: Russian Art 1863–1922* [New York, 1962]).

[84] Winslow Homer, the most significant American painter of this period, began to acquire fame just before the maximum growth stage. There was, however, little to decline from in American art during this stage.

[85] Rostow, *op. cit.*, p. 38.

[86] In France, the "great decade" of Impressionism, which originated in the 1860's, "was 1870 80" (Peter and Linda Murray, *A Dictionary of Art and Artists* [Baltimore, 1959], p. 162). In American art, the beginning of the activity of Homer, Ryder, and Eakins follows immediately upon the take-off, though the content of their work suggests a negative reaction to industrialization. In Germany, the really significant artistic developments (expressionism) were delayed until after the turn of the century. This "delayed effect" may be due to the intense preoccupation with political organization during this period. Artistic efflorescences tend to *follow* such periods. On Great Britain and Russia, see n. 33.

[87] McClelland, *op. cit.*, pp. 120, 132, 139.

[88] Sorokin, *Social and Cultural Dynamics*, II, 148, 150. Data for Greece and Spain suggest that the movement of inventions is associated with the level of foreign trade rather than with the need for achievement. The latter declines in both countries throughout the period at hand, whereas the rate of inventions and the level of foreign trade first rise and then fall in a similar, though not identical, pattern in both countries. In England,

only a tangential indicator of the degree of commitment of social resources to economically relevant adaptive activity, and consequently, so far as the present theory is concerned, it should be a less precise predictor of periods of artistic creativity.

On the assumption that artistic creativity is maximally stimulated in the phase of integration after intensive action, *before* this phase passes over into that of social latency, it is hypothesized, for the purpose of facilitating measurement, that the maxima of artistic creativity will occur within one hundred (plus or minus fifty) years after the beginning of a long-range decline in the commitment of social resources to economic action, as defined by the two previously cited indexes. The first set of data, then gives the following *predicted* maxima of artistic creativity: Greece, 550–450 B.C.; Spain, 1560–1660; England, 1634–1734. The second set gives the following maxima: Greece, 300–200 B.C.; Rome, A.D. 100–200; Spain, 1450–1550; Germany, 1100–1200, 1525–1625, and 1875–1975; France, 1200–1300 and 1825–1925; Italy, 1350–1450; Holland, 1551–1651 (or 1675–1775);[39] England, 1100–1200 (or 1250–1350), and 1800–1900.

To provide a rough test of this hypothesis, the "artistic climaxes," as judged by Sorokin, of the respective nations will be listed, first for sculpture and then for painting: Greece, 559–350 B.C. and 450–300 B.C.; Rome, 30 B.C.–A.D. 100 and A.D. 50–110; Germany, 1120–1260, 1400–1550, and 1450–1560, 1800–1900 (?);[40] France, 1140–1325, 1450–1550, 1850–1910, and 1620–70, 1760–1880 (?);[41] Italy, 1420–1600 and 1420–1600; England, 1220–50, 1758–87, and 1715–1850.[42] The periods of greatest artistic creativity in Holland and Spain may be set at approximately 1580–1660. While there are artistic peaks which do not seem, at least in this rough comparison, to be associated with the economic variables used here, there is apparently a very consistent tendency for periods of incipient decline in the commitment of social resources to economic action to coincide or overlap with significant artistic efflorescences. This correspondence is the more remarkable in view of the imprecision of the data, which have not been collected for the purpose at hand. As has been expected, the first index seems to be a somewhat better predictor of artistic peaks, except in the case of England where the Puritan Revolution may have "postponed" an otherwise expectable artistic efflorescence (although even in this case it overlaps with the predicted period).

The hypothesis that artistic creativity is associated with the passage from the adaptive into the integrative phase can apparently also be supported, though with less certainty, by prehistoric data. The important artistic tradition of the European Paleolithic hunters was created by an "advanced hunter culture" which had reached a "dead end along a particular path of development," while "two other types of economic activity, hoeing of the soil and pasturing cattle . . . in combination led to settled agriculture," without producing, during this period, art of comparable value.[43] It was this materially adequate,[44]

changes in the need for achievement are very closely associated with the level of foreign trade; hence, both are associated with the invention rate (McClelland, *op. cit.*; Sorokin, *Social and Cultural Dynamics*). The two indexes of economic action appear to be more closely interrelated than is either of them with the measure of achievement motivation. This (*a*) provides some validation for the former, and (*b*) suggests that the "psycho-economic" and the "socioeconomic" cycles may not necessarily be closely co-ordinated.

[39] The data for Holland fail to reveal a clear pattern for the period from 1551 to 1675.

[40] This is a somewhat questionable estimate of a peak, which should probably be placed at 1900–1930.

[41] I would place the peak of modern French painting at 1820–1920.

[42] Sorokin, *Society, Culture, and Personality*, pp. 549–51.

[43] Bandi *et al.*, *op. cit.*, p. 12.

[44] Georges Bataille, *Lascaux; or, the Birth of Art: Prehistoric Painting* (Lausanne, 1955), pp. 20, 23.

yet no longer progressing, culture that was responsible for "the birth of art."[45] A parallel association of relative economic backwardness with artistic creativity presents itself in the case of rock engravings so profusely carved, in several areas, in the period of transition from the Stone to the Metal Age.[46]

Other variables being at all comparable, the lack of economic progress in one society, while other societies under similar circumstances are advancing, implies relative satisfaction with the level attained. This suggests an orientation to the integrative rather than the adaptive exigencies in the social system of the Stone Age hunters who created the greatest art of their age.

Summarizing the materials presented in this section, it can be said that artistic creativity tends to be stimulated (*a*) in the period of initial response to the beginning of economic transformations and (*b*) in the phase of social-emotional reintegration following successful adaptive action in the economic sphere. It is adversely affected (*a*) in the phase of most intensive adaptive action in the economy and (*b*) in the phase of economic latency—which has been here identified as the condition of relative equilibrium preceding the beginning of a basic transformation of the socioeconomic system.

Clearly, artistic creativity does not arise from a state of relatively stable social integration, but from a felt need to achieve reintegration. Objectively viewed, the need exists during all stages of an economic transition, but the requisite resources are made available only in its very early stages (when they are not yet primarily committed to economic action) and in its *relatively* late stages (when the results of economic action are felt to be sufficiently satisfying to justify an increasing release of resources for symbolically expressive activities, and before the need for reintegration is felt to have been adequately resolved).[47]

[45] Bandi *et al.*, *op. cit.*, p. 26.

[46] Emmanuel Anati, *Camonica Valley* (New York, 1961), pp. 76–77, on northern Italian; Bandi *et al.*, *op. cit.*, p. 90, on eastern Spanish; and p. 127, on Saharan rock carvings.

CONCLUSION

In the course of this survey, an extremely general sociological theory of artistic creativity has emerged. That economic data have been used as indexes of measurement is, in the most basic sense, incidental to the theory. The interpretations presented should have made clear that what is regarded as causally related to artistic creativity is not economic processes or achievements as such, but certain psychological and sociological conditions which can be empirically correlated with economic as well as, presumably, with other kinds of social processes.

The *psychological* factor favoring artistic creativity has been identified as a relatively balanced tension, in the personality system, between the drives toward achievement and toward expression. There is a very real problem, which our data do not help much in resolving, whether, in a highly differentiated society, this type of motivation must be present only in the personality systems of creative minorities, or whether it must be more generally prevalent to make possible an artistic efflorescence. It is assumed that this personality characteristic constitutes the patterning of individual motivation which is most conducive to artistic attainment.

The *sociological* factor which the data suggest to be an important causal agent in

[47] Artistic creativity which is caused by the beginning of an adaptive disturbance of social latency should be less likely to provide potent symbolic foci of sociocultural integration than the creativity stimulated in the "social-emotional" phase following that of intensive adaptive action, when social resources are *increasingly* released for noninstrumental pursuits. As cases in point, Romanticism may be compared with the Renaissance (and with Greek classicism). This distinction suggests an important element in the analysis of the "alienation of the artist." Because he has a smaller (and perhaps declining) claim on the socially distributed resources and for this reason can accomplish less, the artist should feel more alienated in the initial disturbance than in the reintegration phase.

artistic efflorescences is a widely felt need for reintegration of the social system. While this need has been treated here as a theoretical construct to account for the pattern of data, its existence has been experimentally demonstrated in small-group research, from which the conceptual model of this investigation has been derived. The collective perception of this need is viewed as the cause of an increased demand for art and consequently of a tendency to commit social resources to the action of artistic creation. It is not assumed, however, that the social demand for art is caused only by the integrative need.

The relationship between the two basic variables—the patterning of individual motivation and the commitment of social resources—must be regarded as intrinsically problematic. If the kind of motivation which is most conducive to artistic attainment has not been developed in the personality system, a widespread social demand for art will probably be insufficient to cause an artistic efflorescence. It is only by exploring the historically variable relationship between the two variables that a systematic sociological theory of artistic creativity can be developed.

Regarded by itself, no economic or any other social process can be expected always to have the theoretically predicted effect on artistic creativity. First, a tradition of high art must be present for any social factor to have a significant artistically stimulating effect, and such traditions cannot be taken for granted. Second, the artistically propitious phases of one type of cycle may overlap with, and in their effects be offset by, unpropitious phases of other partly differentiated cycles (political, religious, communal), or vice versa. Third, any historically real cycle movement may be interrupted by the beginning of a new cycle before the former has reached the integrative phase, in which its artistic effects should be most strongly felt.

In spite of the blurring effect which the complexities of historical data have on any theoretically conceived pattern, the evidence presented tends to indicate that the theory of phase cycles constitutes a useful approach to the study of artistic creativity, certainly one of the areas to which the authors of the theory have least intended to apply it.

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Conceptions of Organizational Worth[1]

James L. Kuethe and Bernard Levenson

ABSTRACT

This paper illustrates a technique for studying conceptions of the worth of positions in formal organizations. Basically, it involves subjects' assignment of relative salaries to positions in a set of hypothetical structures. In the experiment reported here, it was found that (1) indirect supervision of subordinates is valued more than direct supervision; (2) a position's worth increases with additional subordinates; (3) the greatest disagreement occurs over middle-level positions with no subordinates. The paper concludes by suggesting a number of possible adaptations of the technique in future research.

In administrative science and economics there is interest in the bases of compensation, particularly that of business executives. A host of factors has been advanced to explain differential salaries within organizations: profits, size of company, number of echelons, span of supervision, training and experience, autonomy, and time span of control.[2] The bases of stratification and determinants of social status, power, social worth, and similar social rankings have been a major concern of sociologists. Theories accounting for differential evaluations of positions in systems of stratification run the gamut from pure power explanations to "functional importance."

In this paper, a technique is outlined for studying the perceived worth of positions within hierarchical organizations. The emphasis is on the *structural* or *topographic* aspects of positions which affect perception of worth.

METHOD

A set of organizational charts, with their order appropriately randomized, was presented to 110 students in an introductory sociology class at Johns Hopkins University. These charts represent hypothetical organizations with four to nine employees and with three to five echelons. The head of each organization, *A*, supervises two positions, *B* and *C;* only in echelons 3, 4, and 5 do structural variations occur.[3] The structures were varied to permit analysis of the estimated worth of one supervisor with subordinates supervised directly and a second supervisor with subordinates either directly or indirectly supervised. When the organizational charts were presented to the subjects, the various positions in each structure were blank except for position *B*. Position *B* had a preassigned salary of $10,000; this was intended to serve as a reference point. The numbers in the other positions of the structures shown in Figures 1 through 11 were computed from the responses of the subjects as is explained below.

Subjects were asked to assign salaries to the other positions in accordance with the following brief instructions:

> In this experiment we want to see how you rate different types of responsibility in administrative structures. You will notice that there are different structures diagramed on the pages you have. In each case the salary of one man is given, and in each case it is $10,000.
>
> Your task will be to estimate the salaries

[1] The authors are indebted to Mary S. McDill for her suggestions and advice in the preparation of the present paper.

[2] See D. R. Roberts, "A General Theory of Executive Compensation Based on Statistically Tested Propositions," *Quarterly Journal of Economics,* LXX (1956), 270–94; H. A. Simon, "The Compensation of Executives," *Sociometry,* XX (1957), 32–35; and Elliott Jaques, *Measurement of Responsibility* (Cambridge, Mass.: Harvard University Press, 1956).

[3] The positions were not lettered when the charts were presented to the subjects but are done so here in order to facilitate discussion.

of the other members of the structure and write the salaries in the blocks representing these people. We want you to assume that the more responsibility a person has, the higher is his salary.

Work quickly, basing your salary estimates on your first impression of the value of each position in the structure. Do not try to be consistent from one structure to the next but rather judge each in its own right. In each structure you are given the salary of one man and should make your judgments relative to him.

No specifications were furnished concerning the tasks that the employees performed or the activities that the organizations pursued, although these might be specified in future studies.

RESULTS

From the distribution of estimated salaries, the median salary for each position was computed and is shown in each block in the eleven organizational charts. In Figure 11, for example, the head of the organization, *A*, was given a median salary of $20,000 by the subjects; *B* has the fixed salary of $10,000; the median salary for each of *B*'s subordinates, *E*, *F*, and *G*, is $7,000; *C* who has three employees has the same salary as *B*; the salaries of his subordinates *H*, *J*, and *L* are $8,000, $6,000, and $5,000, respectively.

The median salaries are analyzed in three ways: (1) In the second echelon the relative worth of direct and indirect super-

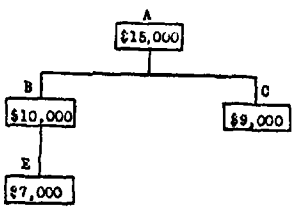


FIG. 1

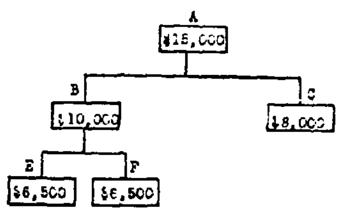


FIG. 2

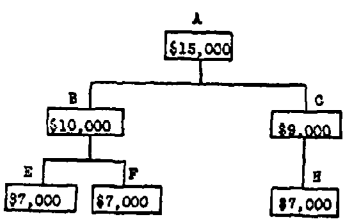


FIG. 3

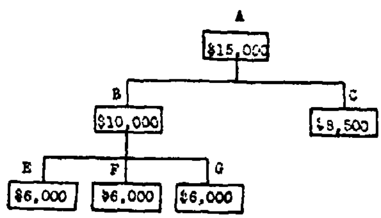


FIG. 4

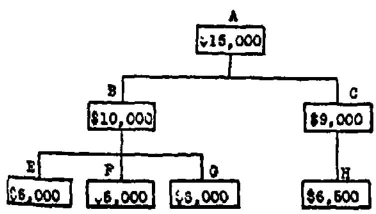


FIG. 5

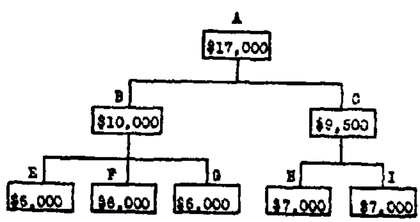


FIG. 6

FIGS. 1–6

vision will be examined; that is, positions *B* and *C* will be contrasted. (2) In the third and fourth echelons the positions subordinate to *B* and to *C* will be contrasted. (3) The estimated worth of the highest position, *A*, in the different structures, will be analyzed.

THE MIDDLE ECHELON

We turn first to the structures where *B* and *C* exercise no indirect supervision and differ only in the number of subordinates (Figs. 1–6).

In general, one would expect the salary of *C* to vary according to the relative number of subordinates controlled by *B* and *C*, the theory being that positions are judged not only by number of subordinates but also by other positions on the same level. These expected values are shown in Table 1. One would expect that *C* would have less salary when *B* has one subordinate and *C*

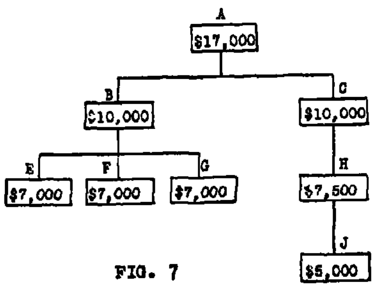


FIG. 7

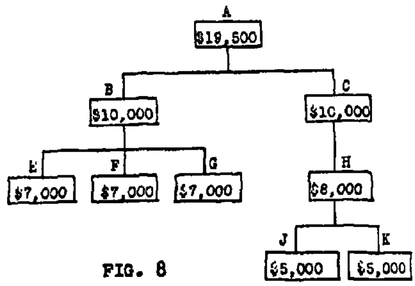


FIG. 8

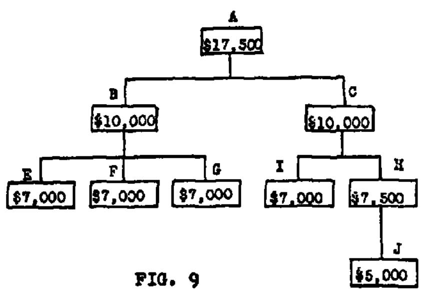


FIG. 9

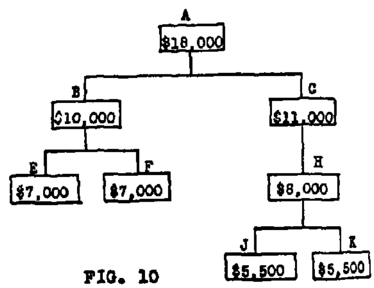


FIG. 10

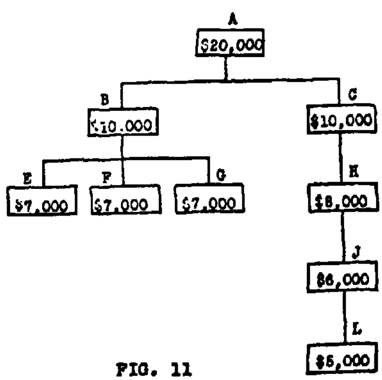


FIG. 11

FIGS. 7–11

has none than when *B* has two subordinates and *C* has one; similarly, one would expect that *C* would have less salary when *B* has two subordinates and *C* one than when *B* has three and *C* two. Moreover, one would expect that *C* would have less salary when B has three more subordinates than *C* than when *B* has two more. The actual median salaries assigned to *B* and *C* are shown in Table 2.

TABLE 1

CONJECTURED MEDIAN SALARY OF *C*, BY NUMBER OF SUBORDINATES SUPERVISED BY *B* AND BY *C*

No Supervised by *B*	No. Supervised by *C*			
	0	1	2	3
0	($10,000)	....	.....	....
1	9,000	($10,000)	......	......
2	8,500	9,250	($10,000)	....
3 ...	8,000	9,000	9,500	($10,000)

TABLE 2

MEDIAN SALARY OF *C*, BY NUMBER OF SUBORDINATES SUPERVISED BY *B* AND BY *C**

No. Supervised by *B*	No. Supervised by *C*		
	0	1	2
1 .	$9,000	....	.. .
Source ..	(Fig. 1)		
2	$8,000	$9,000	.....
Source ...	(Fig. 2)	(Fig. 3)	
3 ..	$8,500	$9,000	$9,500
Source	(Fig. 4)	(Fig. 5)	(Fig. 6)

* In organizational structures where *C* has no indirect supervision.

While they conform in a general way to the expected pattern, there are three departures as listed in Table 3. The salaries of *C* in Figures 2 and 4 are apparently an inversion. The other two departures seem less serious: one would expect differences which do not materialize.

However, Table 2 above shows only medians; not all subjects gave the medians as their estimate. When we examine the distribution of estimates, some light is shed on the apparent inversion of *C*'s salary in Figures 2 and 4.

It will be useful to see how estimates vary in several structures. Consider the three depicted in Figures 4, 5, and 6. In each of these, *B* has three subordinates; *C* has none, one, and two, respectively. As the number of *C*'s subordinates increases, so does *C*'s median salary, although in no case does the median reach $10,000. Though in each instance the majority of

TABLE 3

Departure from Expected Pattern	No. Supervised by		*C*'s Salary	Source
	B	*C*		
1	3	0	$8,500	Fig. 4
	2	0	8,000	Fig. 2
2	3	1	9,000	Fig. 5
	2	1	9,000	Fig. 3
3	1	0	9,000	Fig. 1
	2	1	9,000	Fig. 3

subjects assigned *C* a salary lower than $10,000, many assigned him $10,000, and some even assigned him more than that.

Figure 12 exhibits the proportions for the three structures. The hatched bars show the percentage of subjects who assigned *C* a salary in excess of $10,000. When *C* has no subordinates, 20 per cent assigned him a salary in excess of $10,000: apparently, they perceived his position vis-à-vis *B* in a qualitatively different way. Inasmuch as subjects were not queried regarding the reasons for their judgments, the differential conception can only be inferred. When *C* supervised no subordinates (Fig. 4), perhaps his position was viewed as staff, whereas *B*'s position was viewed as line. When *C* supervised one subordinate (Fig. 5), only 7 per cent assigned him a salary in excess of $10,000. With two subordinates (Fig. 6), the percentage assign-

ing him more than $10,000 dropped to 3 per cent. Thus, where the contrast between *B* and *C* is reduced, the percentage assigning *C* a higher salary drops sharply.

Experiments could be conducted utilizing a diversity of other organizational charts. As an example, suppose Figure 13 were included in the set of hypothetical organizations where *Q* and *S* each has six subordinates and *R* has none; then one might conjecture that the proportion of subjects to assign *R* a salary greater than *Q* or *S* would be much more than 20 per cent.

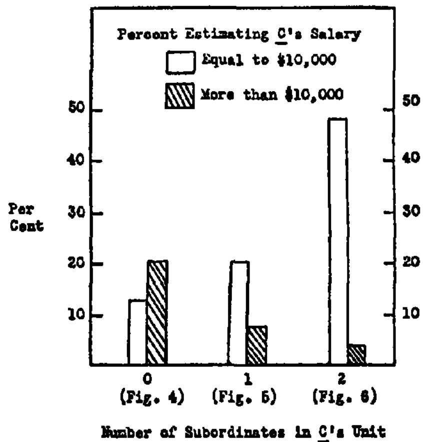


FIG. 12

This suggests that a profitable line of inquiry would be the study of the degree of consensus regarding the worth of positions. It may be that certain structural locations or certain structures contribute to greater variation in judgments of worth.

We now turn to the cases where *C* exercises indirect supervision. In Figure 6, *C* has two direct subordinates, and in Figure 7, *C* has two subordinates, one of whom is indirectly supervised. The median salary goes from $9,500 to $10,000, indicating that indirect supervision is valued more.

Consider the four remaining cases of indirect supervision. In Figures 8, 9, and 11, *C* has three subordinates and is, in each case, assigned a median salary of $10,000, the same as that preassigned to *B* who in these structures also has three subordinates. In these three structures *C*'s subordinates are under different degrees of control by him. In Figure 9 where one of them is indirectly supervised, 17 per cent of the subjects estimated *C*'s salary in excess of $10,000. In Figure 8 where two of his subordinates are indirectly supervised, *C*'s salary was set in excess of $10,000 by 33 per cent. In Figure 11 where the supervision is more indirect than in Figure 8, 35 per cent rated *C*'s salary greater than $10,000. Figure 11 produces the greatest disagreement concerning position *C*.

There are three cases (Figs. 8, 9, and 11) where *B* and *C* have three subordinates. In these cases, the salaries of both

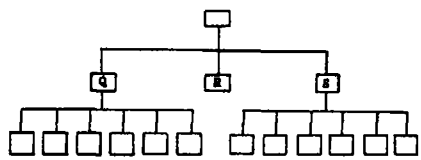


FIG. 13

are equal. One would have hypothesized that *C*'s salary would have exceeded *B*'s, insofar as *B*'s subordinates are directly supervised, whereas some of *C*'s subordinates are indirectly supervised.

While *C*'s salary did not exceed *B*'s, the presence of subordinates of *C* in a fourth or fifth echelon did have an influence, namely, in augmenting the estimated worth of *A*, the head of the organization. In Figure 9, where *C* has one indirect subordinate, *A* gets $17,500; in Figures 10 and 8 where there are two employees in the fourth echelon, *A* gets $18,000 or $19,500; in Figure 11 where there is one in the fourth echelon and one in the fifth, *A* gets $20,000, the highest salary occurring in the set. It appears that *C*'s indirect subordinates were interpreted by the subjects as being supervised indirectly by *A*. Possibly, in larger organizations, the indirect supervision

would not be interpreted as stemming from the highest position.

SUBORDINATES

Where C has direct supervision and fewer subordinates than B, he makes less than B. Paradoxically, his subordinates make more than the subordinates in B's unit. Presumably, this occurs because a scarcity value is attributed to them. In every instance where C has indirect subordinates, at least one of them is rated $7,500 or more, whereas B's do not earn more than $7,000. It suggests a situation in actual organizations that might be a source of conflict between units. That is, if B for some reason were to leave the organization, some employee from echelon 3 would most likely be promoted to fill the vacancy. There might be discrepant expectations in echelon 3 as to the unit from which the employee should be recruited. On one hand, those in B's unit might think the successor should come from the same unit as the vacancy; on the other hand, the subordinate in C's unit who had been receiving the highest salary in echelon 3 might feel that he should be next in line.

Another way of looking at the structures is to observe how the addition of a single employee affects the salaries of all positions. For example, consider Figures 5 and 7. They differ in that Figure 7 has an additional position, J. One might have expected that A, C, or H would profit most by the addition of employee J. However, the median salaries of E, F, and G increase 17 per cent, whereas H increases 15 per cent, A 13 per cent, and C 11 per cent. It is not apparent why C should have profited least. If Figures 5 and 7 represented change in an actual company, then the addition of position J, a $5,000 salaried employee, would result in a payroll change of $12,000. The point is that changes, seemingly isolated, ramify in unexpected ways to affect the perceived worth of other positions. Similar comparisons could be made with other pairs of figures.

ORGANIZATION HEAD

It is interesting to observe that the number of echelons and the size of the organization have the same effect on the salary of the highest position, A. Each was found to have a correlation near .90, and the multiple correlation is .93.[4]

Manifestly, the number of echelons and number of employees are highly correlated in these idealized structures—and would be in any empirical study of organizations. As stated earlier, the structures were varied primarily to contrast positions B and C. If one were interested in the effect of echelon versus size on the salary of the head of the organization, a set of structures could be invented such that the two variables are not as highly correlated as in the eleven structures. Thus, the technique allows for a more conclusive determination of their independent effects on conceptions of worth.

One feature about the estimates of salary of the organization head should be noted. Apparently, as far as the top position is concerned, structures are perceived homogeneously below a certain size and complexity. In Figures 1–5, the median salary of A was $15,000. Not until there were five employees in the third echelon or there was a fourth echelon did any variation in salary occur.

SUMMARY AND CONCLUSIONS

In this exploratory study, which used eleven hypothetical organizational structures, the following findings were obtained:

1. Holding number of subordinates constant, indirect supervision of subordinates confers greater worth on a position than does direct supervision.
2. Additional direct subordinates increase a position's worth.
3. A middle-echelon position without subordinates is perceived in qualitatively different

[4] The regression equations are as follows: $S = \$7{,}998 + \$2{,}462\ E$; $S = \$10{,}055 + \$929\ N$; $S = \$7{,}781 + \$1{,}638\ E + \$437\ N$, where S represents the median salary of A, E the number of echelons, and N the number of employees.

ways. Some ascribe more value to the position than to one with subordinates. In general, these positions evoke the greatest disagreement regarding organizational worth.

4. Both number of echelons and number of employees in organization are correlated with salary of organization head, echelons somewhat more so, and jointly the two variables account for 86 per cent of the variation.

Although this study was carried out with students, the technique does not require students as subjects. Indeed, it would be interesting to secure the evaluations of executives or workers in different occupations, or at different echelons, or in different-sized organizations. Numerous adaptations of the technique are possible. Other structures might be used, and instead of preassigning one middle-level position a salary of $10,000, as a frame of reference, other positions might be used, and other salaries might be preassigned. Or subjects might be asked to distribute a payroll aggregating some fixed sum, such as $100,000. In fact the results of this study could be used in future research: all of the median salaries could be used with the subjects asked only to assign a salary to position *B* in the eleven structures. It would be interesting to see to what extent this mirror experiment would reproduce *B*'s fixed salary of $10,000.

Alsc various specifications might be furnished about these organizations. The positions within the organizaitons might be described briefly. Or various background characteristics of the occupants of each position might be provided: race, sex, or age. Or the organizations might be characterized according to their institutional contexts, whether governmental, industrial, or military.

It might be that salary judgments are highly related to subjects' preferences. Some may prefer to exercise authority directly, others indirectly, others not at all. Or various psychological traits might be associated with judgments. For example, it may be that the degree to which subjects ascribe worth to indirect power may be related to authoritarianism, powerlessness, or alienation.

It would be interesting to compare these subjective conceptions of worth with the salaries in actual organizations.

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Potential Elites in Ghana and the Ivory Coast
A Preliminary Comparison

Remi P. Clignet and Philip Foster

ABSTRACT

This study examines patterns of social recruitment in two highly selective systems of secondary education in two adjoining African states with differing colonial and educational traditions. Marked differentials occur between two groups of sampled students of roughly equivalent academic status in the two areas. Ghanaian students are uniformly drawn from higher socioeconomic groups than their Ivory Coast counterparts. However, in relation to population characteristics the two systems appear to function in a similar manner. Furthermore, both groups are characterized by a considerable degree of uniformity concerning career aspirations, expectations, and perceptions of future roles.

In spite of a growing body of literature dealing with the emergent elites of the new African states, it must be confessed that we know very little about them. There is sometimes a tendency to speak of them as if they were homogeneous in character; yet much material concerning their social origins, their aspirations, and their value orientations remains extraordinarily impressionistic.

Further, the term "elite," although frequently used, is very ambiguous. An elite may be defined in terms of its common possession of a single attribute or set of objective attributes. Thus, in the African context, we could regard as members of an elite all individuals who have achieved a given minimal level of education or income or who are engaged in particular occupations. This definition has shortcomings if we are concerned with elites in terms of their decision-making abilities (political or otherwise) or their capacity to act as generalized reference groups for the remainder of the population.[1] Thus, in the following pages, when we examine the characteristics of two samples of secondary-school students in the Ivory Coast and Ghana, we shall treat them as "potential elites." Their schooling provides a necessary, but not sufficient, qualification for them to assume key positions in their societies.

In most contemporary African states, employment opportunities within the modern economic sector are dominated by the demands of government and other public agencies. This bureaucratization of the employment structure confers a high premium upon educational qualifications (usually as measured by examination success) for access to elite roles. Recruitment policies of the larger firms in the private sector also present the same characteristics.

Furthermore, in many areas, explosive rates of growth in the output of the schools are associated with a very sluggish development of job opportunities in this modern sector. This has very rapidly raised the minimal educational qualifications necessary to enter most occupations. Thus, a post-primary education becomes a critical factor in occupational success, and in no country in Africa south of the Sahara do

[1] For example, if education is used as the criterion of elite membership, it is clear that the two groups of students we compare below constitute part of the national elite. They certainly fall well within the top 1 per cent of the population in level of schooling. However, we do know that a substantial proportion of these groups will enter primary-school teaching or low-grade clerical work. Neither of these two occupations enjoys very high prestige or status within either country.

more than 2 per cent of any age cohort enter any form of secondary institution.[2] We therefore feel justified in regarding African secondary schools as "elite reservoirs" whose graduates will fill most positions of power and authority in the next few decades. A crucial problem of research is, therefore, to examine the social composition of secondary-school populations and to ascertain to what extent recruitment patterns are fluid or constricted.

A second step is to analyze how this recruitment function is affected by the residue of colonial experience. For example, although a great deal has been written about differences between French and British colonial policies, no empirical evidence exists with respect to contrasts in processes of elite selection within areas formerly governed by the two colonial powers. We have, therefore, attempted to compare the characteristics of a sample of secondary-school students in Ghana, an ex-British territory, with those of a similar group of students in the formerly French Ivory Coast. These countries provide a good opportunity for controlled comparison insofar as they are largely similar in terms of a whole range of characteristics but differ with respect to certain key variables.[3]

SOME COMPARISONS BETWEEN GHANA AND THE IVORY COAST

First, there are substantial similarities in the economies of Ghana and the Ivory Coast.[4] Although the latter is rather the less developed of the two, both countries rank highest in income per capita among the nations of West Africa, with a level of between $150 and $200 per annum. The comparative prosperity of the two states is, in large measure, attributable to an extensive development of profitable cash crops in the southern and central portions of the respective territories. The occupational structure of both countries is relatively more complex than those of other West African states (with the possible exception of Senegal).

These over-all economic similarities are reinforced by the fact that the pattern of sociocultural change in both areas has followed similar lines. European penetration was initially effected from the coastal zone with the gradual incorporation of other traditional groupings in the central and northern areas at a later date. Thus maximal change has occurred in the southern and central areas of both nations; and it is in these that the exchange economy is most developed, processes of urbanization most advanced, and formal education most in evidence. In contrast, the populous northern areas are, in both cases, far less developed, with a greater emphasis on subsistence farming, lower levels of urbanization, and a more limited diffusion of formal education. Both territories are divided into a largely Christian south and a predominantly Islamic north.

There are equally striking similarities in terms of ethnic composition. Since these nations are adjacent to each other, the Akan peoples of southern and central Ghana are closely related to the Anyi and Baoule groups of the southeastern and central Ivory Coast. Further, the Ewe of Ghana and the Kru groups of the Ivory Coast, though belonging to different ethnic clusters, do manifest substantial similarities in their pattern of social organization and stand in similar relationship to the Akan peoples of both areas. Last, some

[2] Clearly, we refer here to the African population only and exclude the Union of South Africa, where levels of education among Africans are considerably higher than for the rest of the continent.

[3] The particular appropriateness of Ghana and the Ivory Coast for a comparison of this type has already been noted by R. J. Harrison Church, *West Africa: A Study of the Environment and of Man's Use of It* (3d ed.; London: Longmans, Green, & Co., 1961), p. 344.

[4] The best general sources for the background of the two areas are, for Ghana: D. E. Apter, *The Gold Coast in Transition* (Princeton, N.J.: Princeton University Press, 1955); and for the Ivory Coast: Ministère des Finances des Affaires Economiques et du Plan Service de la Statistique, *Inventaire économique et social de la Côte d'Ivoire* (Abidjan: 1960).

of the Voltaic peoples of northern Ghana are represented in the Ivory Coast, and there are parallels between their culture and that of the nuclear and peripheral Mande of the latter nation.

Given these substantial similarities between the two countries, we must now indicate in what respects they diverge. Clearly, their patterns of administration were and are rather different. Traditionally, the French tended to develop a system of direct and internally centralized administration which was, in turn, closely integrated with their West African Federation and the metropole itself. Conversely, the British relied to a greater extent on indirect rule (particularly in Ashanti and the northern areas) and developed a less centralized pattern of administration which also deemphasized the direct links between Ghana, the other areas of British West Africa, and the metropole.

More important, although the pattern of French penetration in the Ivory Coast substantially paralleled British expansion in Ghana, it always lagged behind it. Thus, by the end of the nineteenth century, the British had effectively incorporated most of the territories that now constitute contemporary Ghana, and in the southern zone, in particular, the European impact was already considerable. By contrast, the French were not able to establish effective administration in some areas until after the close of the first World War.

THE SCHOOL SYSTEMS

Since educational diffusion is a corollary of European overrule, it is clear that this "staggered" development of the two areas had considerable implications for the growth of their educational systems. By 1950, Ghana, with a population of roughly 5 million, had 281,000 individuals enrolled in formal schools at all levels, while the Ivory Coast, with an estimated population of 3.2 million, could point to only 35,000 persons enrolled. During the last decade the difference at the primary-school level has been closing, owing to massive investment by the government of the Ivory Coast in primary education.[5] However, at the secondary-school stage the contrast between the countries remains formidable and needs further examination.

The school system in the Ivory Coast consists of a six-year primary sequence followed by a highly differentiated system of secondary studies. In 1963 there were a little under 300,000 students in primary schools compared with less than 20,000 in all kinds of post-primary institutions. These post-primary schools vary greatly in prestige, ranging from Centres d'Apprentissage offering a four-year terminal program of vocational training to the academic and technical Lycées which provide access to university studies. It is the latter group of institutions which concerns us here, since they are the crucial segment of the whole system so far as processes of elite formation are concerned.

The Ghanaian system is much larger. By 1960, there were already 512,000 pupils in the six-year primary schools and, most strikingly, no less than 225,000 in all types of post-primary education. The vast majority of the latter were in so-called middle schools: these provided a terminal four-year sequence for most pupils but also acted as feeder institutions to a small group of highly selective public secondary schools which offered a five-year course.[6] In 1960 there were over 14,000 students in these latter institutions which are, in fact, generally comparable to the Lycées and Collèges of the Ivory Coast. It is

[5] This is not to suggest that primary-school expansion in Ghana has been negligible. On the contrary, it has been substantial, but the maximum growth rate has now been passed and the system continues to grow at a slower pace.

[6] This figure excludes some 6,000 pupils enrolled in private secondary schools in Ghana. However, these institutions are, for the most part, at such a low level that they are usually more comparable to middle schools. It is noteworthy, also, that public secondary schools in Ghana are not, for the most part, "government" schools but are institutions often founded by voluntary agencies but now overwhelmingly aided from government funds.

clear, therefore, that a comparison of potential elites in the two nations can be most effectively made by contrasting Ghanaian secondary-school students with their counterparts in the Lycées of the Ivory Coast.

However, this ignores the crucial problem of obtaining comparable groups when the cycle of secondary studies is so different. With this in mind, it was decided to compare Ghanaian pupils in the final year of their five-year basic secondary course with Ivory Coast Lycée students preparing the first part of their *Baccalauréat* examination.[7] This constitutes a crucial cutoff point in both systems. In Ghana, approximately 60 per cent of the students successfully pass the General School Certificate examination at the end of five years, but of these successful candidates, only about one-third enter the sixth forms which prepare directly for the university. In the Ivory Coast, just under 40 per cent of the candidates pass the first *Baccalauréat* successfully at the first attempt and enter for the one-year course preparing for the second *Baccalauréat*, which is preparatory to university study and is thus roughly similar to the Ghanaian sixth form.

The two samples discussed in the following pages are, therefore, very comparable in terms of level of study and type of school and were drawn as follows.[8] The Ghanaian sample, consisting of 775 fifth-form boys drawn in 1961 from twenty-three secondary institutions, constituted just over 45 per cent of the entire male fifth-form population in the secondary schools at that time. Correspondingly, the Ivory Coast sample of male students sitting for the first part of the *Baccalauréat* was drawn in 1963 from the eight institutions which prepare for this examination. The sample comprised 259 cases or 82 per cent of all pupils at this educational level in the Ivory Coast.[9] At this stage the Ghanaian system contains over five times as many students as that of the Ivory Coast.

This suggests that the two systems can only be contrasted not in terms of two colonial prototypes but also in terms of their relative level of growth and "maturity."

SOCIAL BACKGROUND OF POTENTIAL ELITES

We must first consider the ethnic characteristics of these two groups of students. This is no idle academic problem in the context of contemporary African politics. With the advent of independence, ethnic minorities increasingly demand equal access to key institutions in the new states. In particular, they claim that they have obtained less than their "fair share" of crucial roles, owing to their lack of opportunities to obtain school places.[10] In both Ghana and the Ivory Coast, such inequali-

[7] Generally, the curriculum at this level in both systems is not highly specialized, though there is more "streaming" by subject groups in the Ivory Coast.

[8] In 1961, Ghana had forty-six secondary schools with fifth-form classes. The twenty-three schools selected for investigation were randomly drawn from this list stratified by region, type, and size. In the case of the Ivory Coast, the smaller size of the system made it possible to attempt a total coverage at the *Baccalauréat* level. However, 18 per cent of students were not reached, owing to temporary absence from classes. Girls were excluded from this comparison in view of their small number at this level in the Ivory Coast. Also, since their backgrounds are very different from those of boys, they certainly would have distorted the picture as far as Ghana is concerned.

[9] In practice, Ghanaian students have completed anywhere between thirteen and fifteen years of education before reaching the fifth-form level. This is because selection for secondary school can be from any one of the middle-school years. Conversely, the Ivory Coast students have theoretically completed twelve years of education. Therefore, we might expect the latter group to be younger than the Ghanaians. In fact, the reverse is the case. Ivory Coast students average 20.3 years of age and Ghanaian students, 19.2. This is due to the far greater incidence of doubling classes in the Ivory Coast system (47 per cent of the sample had doubled one or several previous classes) and the fact that children probably start schooling later than in Ghana.

[10] For Ghana, this point is discussed more fully in Philip Foster, "Ethnicity and the Schools in Ghana," *Comparative Education Review*, VI (October, 1962), 127–35.

ties in educational provision have been very evident.

Table 1 illustrates the pattern of differential ethnic recruitment into secondary schools in the two areas. To simplify the picture, we have divided both nations into three general ethnic zones (southern, central, and northern) and classified the numerous groups into one or the other of these categories. Further, to facilitate comparison, we have provided crude selectivity indexes by computing the ratio between a group's representation in the general population and its representation in the sample.

TABLE 1

ETHNIC BACKGROUND OF SAMPLED STUDENTS IN RELATION TO ETHNIC CHARACTERISTICS OF POPULATIONS OF IVORY COAST AND GHANA

Ethnic Groups	Ivory Coast			Ghana		
	Percentage* of Population	Percentage of Sample	Selectivity Ratio	Percentage† of Population	Percentage of Sample	Selectivity Ratio
Southern	31.2	50.5 (131)	1.6	47.0	64.2 (498)	1.4
Central .	30.2	21.3 (55)	0.7	20.7	28.2 (218)	1.4
Northern....	34.2	20.5 (53)	0.6	30.7	6.3 (49)	0.2
Other . .	4.4	6.2 (16)	1.4	1.6	1.2 (9)	0.8
No answer ..	....	1.5 (4)	....	.	0.1 (1)	..
Total . .	100.0	100.0 (259)	..	100.0	100.0 (775)	..

* Computed from Ivory Coast, Ministère du Plan, *Inventaire économique de la Côte d'Ivoire, 1947–1956* (Abidjan, 1958), p. 26.

† Computed from Gold Coast, *Census of Population, 1948* (Accra, 1950), pp. 367–69. These figures are not entirely reliable and no data are yet available from the 1960 census concerning the present ethnic composition of the population.

It might have been expected that in the smaller and less-established system of the Ivory Coast, inequalities in ethnic access between north and south would be more marked. In fact, this is only partially true. To be sure, in Ghana, the variations between the southern and central groupings which were substantial at all levels of education up to 1945 seem to have been largely eradicated, while in the Ivory Coast they are still very apparent. In this context it is important to note that the central portion of Ghana includes the major cocoa-producing areas and constitutes the economic heartland of the nation. The gap between economic development of southern and central Ghana is proportionally less than that between the corresponding zones of the Ivory Coast. Though the northern areas of both countries are very similar in their level of development, northern Ghanaians are far less successful in getting into post-primary education of this type than are their Ivory Coast counterparts. This is in spite of the fact that the far greater size and maturity of the Ghanaian educational system might have led us to expect a greater access of northern groups into the secondary schools. Not only are there proportionally more places available in Ghanaian secondary institutions but these places are geographically more diffused throughout the country. This suggests that the growth of an educational system does not quickly even up ethnic inequalities.

There has historically been a close relationship in Africa between the growth of the schools and the development of towns;

it was usually within urban centers that a demand for formal schooling was first manifest. The growth of an exchange economy and the development of new Western-type occupations, access to which was often dependent on formal educational qualifications, stimulated an early desire for education. Moreover, urban children tended to adjust more rapidly to the requirements of the schools. Both Ghana and the Ivory Coast have, therefore, long been faced by persistent and sharp differentials between urban and rural levels of primary schooling. It is interesting to examine these differentials at the secondary-school level (Table 2). This table indicates the nature of this relationship in the two territories, using the rather crude measure of community size as an index of urbanization. We also have attempted to show an almost identical degree of drift of the students in both samples from the smaller villages to the very largest towns.[11]

It is clear that Ghanaian students come from a more urban background in terms of both birth and residence than do their Ivory Coast counterparts. This is understandable enough in view of the rather higher levels of urbanization in Ghana as a whole. However, this tells us very little about how the two educational systems function in terms of selection when the residential characteristics of the population as a whole are taken into account. In spite of the fact that Ghanaian students are more urban in background, it could still be concluded a priori that the relationship between secondary-school selection and urban origin is looser in Ghana than in the Ivory Coast; such a pattern would not be surprising since the Ghanaian secondary-school system offers far more places in relation to population. Conversely, it could be suggested that the smaller the system, the sharper the differentials are likely to be—a pattern found in so many Western countries. However, these two assumptions seem hardly tenable in this instance. If anything, the urban/rural differentials are rather more marked in Ghana than in the Ivory Coast, particularly if we look at the

TABLE 2

BACKGROUND OF SAMPLED STUDENTS IN RELATION TO SIZE OF BIRTHPLACE AND PRESENT RESIDENCE

Population of Town or Village	Distribution of Ivory Coast Population*	Birthplace of Sampled Students	Selectivity Ratio	Present Residence of Sampled Students	Selectivity Ratio	Distribution of Ghanaian Population†	Birthplace of Sampled Students	Selectivity Ratio	Present Residence of Sampled Students	Selectivity Ratio
Below 5,000	82 3	62.5	0 8	48.6	0.6	77.0	38.1	0 5	26.8	0.3
5,000–9,999	4.3	11.6	2 7	12.4	2.9	6 0	14.3	2.4	12 1	2.0
10,000–49,999	3.7	10.8	2 9	13 9	3.8	9 3	24.4	2 6	26.7	2.9
Above 50,000	9.7	7.3	0.8	20.8	2.1	7.7	19.4	2.5	33.3	4 3
Not born in or not resident in Ivory Coast or Ghana	......	6.9	......	3.1	...	......	3.5	.....	0.6	...
No answer	.....	0.8	.....	1.2	...	.....	0.3	.....	0 5	.
Total	100.0	99.9	......	100.0	......	100.0	100.0	....	100 0	....

* Computed from Ivory Coast, Ministère du Plan, *Inventaire économique de la Côte d'Ivoire, 1947–58* (Abidjan, 1960), p 37

† Computed from Ghana, *Population Census, 1960*, Advance Report of Vols III and IV.

[11] Actually, this movement is very complex and involves not only direct migration from the very smallest to the very largest centers, but also a multiplicity of small-scale movements to towns slightly larger than the place of birth. It also correlates in some measure with a migration from the northern and central areas to the coastal zone, though this is more marked in the Ivory Coast.

pattern of recruitment from the very smallest and the very largest communities. Ivory Coast enrolments from the smallest centers are proportionally larger than in Ghana. Also, there is in Ghana a linear relationship between size of birthplace or place of residence and secondary-school access—the larger the town, the greater the proportional representation of students from these centers. This is not duplicated in the Ivory Coast; students born or resident in the very largest towns (Abidjan and Bouaké) are actually less well represented in the system than those from medium-sized communities.

We are somewhat puzzled by these unexpected disparities. Very tentatively we suggest that they may be due to differences in the geographical dispersion of towns and secondary schools in the two countries. Thus Abidjan is the only highly urbanized center in the Ivory Coast, and the overwhelming bulk of secondary schools are concentrated there. These are essentially national institutions recruiting from the whole country. Since, however, the rate of growth of the population of Abidjan has expanded faster than the school system, students coming from this city are proportionally disadvantaged as compared with individuals from other areas.

In Ghana, conversely, there is a more even dispersion of both schools and urban centers. Although secondary institutions theoretically draw upon a national population, they tend to recruit on a far more regional basis.[12] It could have been hypothesized that, under such conditions, rural boys would be proportionally more favored than their Ivory Coast counterparts. Yet this is clearly not the case—far from evening up urban/rural contrasts, the geographic diffusion of educational facilities and towns has tended to sharpen differentials.

Thus we can see that, with respect to both ethnicity and urbanization, the growth of an educational system does not necessarily, in the short run, lead to a lessening of differentials in recruitment. When, however, we examine the family backgrounds of the two groups of students in terms of paternal occupation, the picture seems to conform more closely to our expectations (Table 3). First, it is evident that although Ghana and the Ivory Coast are alike in so many other respects, their occupational structures are very different. To be sure, both are peaked and constricted at the summit, but there is in Ghana a much greater level of growth and differentiation at the higher occupational levels. The greater maturity of the economic structure of Ghana is reflected in the fact that the proportion of students from "white-collar" backgrounds is about double that in the Ivory Coast and the offspring of skilled workers and artisans are relatively more numerous in the Ghanaian group. Conversely, the children of farmers and fishermen form a larger proportion of the enrolment in the Ivory Coast sample.

However, if we relate these figures to the population base, it is apparent that the Ivory Coast system tends to favor the selection of children of professional and clerical workers to a far greater degree than does the Ghanaian system. In terms of this variable, the expansion of the Ghanaian system has tended to produce a more open pattern of recruitment than that prevailing in the Ivory Coast.

Let us finally take a look at the educational backgrounds of the fathers of our two groups of students (Table 4). As we might expect, it shows a pattern very similar to that in the preceding table and reflects the much more substantial development of formal education in Ghana. Well over half of the fathers of the Ghanaian pupils have gone beyond primary school as against just over 10 per cent of the Ivory Coast fathers. In fact, 4 per cent of Ghana-

[12] For example, within the investigated schools located in Abidjan, only 18 per cent of the sampled students actually lived in that city. By contrast, in the investigated schools of Accra and Kumasi, no less than 40 per cent of sampled students were permanent residents.

ian fathers have completed a university education, while there is not one example of this in the Ivory Coast sample. Indeed, nearly all Ivory Coast fathers in the "above-primary" category only went as far as the "École Primaire Supérieure," while almost half of the Ghana fathers in this category had gone even beyond middle school.[13]

Unfortunately, the selectivity ratios for the two systems cannot be computed for parental education, since no data exist concerning the educational level of adult males in the Ivory Coast. We do know that, in Ghana, the education of the fathers of our students is very much higher than that prevailing among the adult male population. Indeed, the gradient among selectivity ratios is far steeper than it is for either urban/rural or occupational characteristics, indicating the rather greater importance of paternal education in influencing secondary-school access.[14] We have little doubt that the situation is much the same in the Ivory Coast. We do know that the fathers of our students are more highly educated

[13] In the Ivory Coast, the structure of the educational system did not allow Africans to pursue regular secondary studies of the metropolitan type before 1928. Correspondingly, although it was difficult for Africans to undertake such studies in the Gold Coast before 1928, opportunities did exist; by that time there was a very small but highly significant group of second- or third-generation university graduates.

[14] The Ghanaian selectivity ratios vary from 0.4 for students with uneducated fathers to 13.0 for students whose fathers had attended university or its equivalent.

TABLE 3

PATERNAL OCCUPATIONS OF SAMPLED STUDENTS COMPARED WITH OCCUPATIONS OF ADULT MALE POPULATION IN GHANA AND IVORY COAST

(Per Cent)

Occupational Group	Ivory Coast			Ghana		
	Adult Male Population*	Sampled Students	Selectivity Ratio	Adult Male Population†	Sampled Students	Selectivity Ratio
Professional, higher technical and administrative	0.3	7.7 (20)	25.7	1.5	20.8 (161)	13.9
Clerical and allied (including teachers)	1.7	10.0 (26)	5.9	5.4	15.6 (121)	2.9
Private traders and businessmen	3.3	6.9 (18)	2.1	3.8	11.0 (85)	2.9
Skilled workers and artisans	0.9	3.1 (8)	3.4	11.8	10.6 (82)	0.9
Semiskilled and unskilled workers	7.0	1.2 (3)	0.2	13.4	1.5 (12)	0.1
Farmers and fishermen	86.3	67.9 (176)	0.8	62.8	37.4 (290)	0.6
Other (including police and uniformed services)	0.5	2.3 (6)	4.6	1.3	0.6 (5)	0.5
No answer	......	0.8 (2)	....	....	2.5 (19)	.
Total	100.0	99.9 (259)	....	100.0	100.0 (775)	......

* Computed from UNESCO, *Première mission du groupe de planification de l'education en Côte d'Ivoire* (Paris: UNESCO, 1963), p. 19; and from UNESCO, *Situation et perspectives de l'emploi dans le cadre du plan décennal de développement, 1963*, Table III. (Mimeographed.)

† Computed from Ghana, *Population Census, 1960*, Advance Report of Vols. III and IV.

than the adult males of Abidjan for whom data do exist, and it is certain that this latter group is far better educated than the bulk of the Ivory Coast male population.[15] We would hazard that the selectivity gradients by level of paternal education are probably even steeper than those for Ghana.

Let us now draw together our previous discussion. It is clear enough that there are considerable differences in the social backgrounds of these two very significant groups of students. Ghanaian pupils, in general, are more "acculturated" than their Ivory Coast counterparts in terms of certain background characteristics. They are much more likely to be drawn from the larger urban centers and to come from families with higher occupational and educational backgrounds. Further, in terms of religious affiliation, Ghanaian students are overwhelmingly Christian (less than 5 per cent are Moslem) while over 15 per cent of the Ivory Coast group are Moslems.

However, if we compare the characteristics of pupils with those of the general population, it seems clear enough that both systems function in a similar manner. To be sure, certain differences are apparent; the French schools are much more selective in terms of occupational background, though urban/rural differentials are a little less marked. Indeed, what is surprising about the Ivory Coast system is that it seems to be about as fluid in its recruitment patterns as is the Ghanaian system, in spite of its very limited size. We suspect that part of the explanation may lie in the fact that Ivory Coast secondary schools are normally free while even in the public schools of Ghana, substantial tuition and boarding fees are still charged.[16]

However, what is outstanding about these systems is that both recruit from very broad segments of the population. Selection is undeniably skewed in favor of particular groups, but it is noteworthy that almost 70 per cent of Ivory Coast students and nearly 40 per cent of Ghanaian students come from farming families in which parents are overwhelmingly illiterate. A great deal has been made about the "exclusive" and "aristocratic" nature of elite academic institutions of the French or British type in Africa. The plain fact is

TABLE 4

COMPARISON BETWEEN PATERNAL LEVELS OF EDUCATION OF SAMPLED STUDENTS IN IVORY COAST AND GHANA

(Per Cent)

Level of Education	Ivory Coast	Ghana
None	65 3 (169)	32.4 (251)
Some primary school	14 6 (38)	7 3 (57)
Completed primary school	4 6 (12)	2 1 (16)
Above primary school	12 4 (32)	52 9 (410)
No answer	3 1 (8)	5 3 (41)
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that they are by no means exclusive in their clientele and draw upon able individuals from most ethnic and socioeconomic groups. This suggests that the elites of the two territories are likely to be extraordinarily heterogeneous in terms of social origin for some time to come.[17] In this sense, the functional differences between British- or French-type educational systems are

[15] See République de la Côte d'Ivoire, *Recensement d'Abidjan, 1955* (Abidjan, 1960), p. 37.

[16] About 50 per cent of the Ghanaian students had bursaries and scholarships, but in most cases these only covered a part of tuition and subsistence costs.

[17] It is possible that patterns of recruitment may become more constricted in the future. However, we do not believe that this will be the case in Ghana (see Philip Foster, "Secondary Schooling and Social Mobility in a West African Nation," *Sociology of Education*, XXXVII [Winter, 1963], 165–71). Correspondingly, in the Ivory Coast, two recent administrative decisions have limited access to the second cycle of studies and imposed subsistence costs on students. It will be interesting to see what effect these regulations will have.

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minimal. Such differences as do occur are largely explainable in terms of the absolute size of the educational systems and the different socioeconomic profiles of the two nations.

Of course, it is interesting to speculate whether existing fluidity in access to secondary schooling will continue to increase or will diminish in the future. It is possible to argue that, as the number of educated elites increases, they will be able to command differential access into the schools for their children, with the result that patterns of recruitment will become more restricted. This argument has considerable force, but we would suggest, however, that elites will not be able to effectively monopolize access to secondary schooling since they are not in a position to resist or even control mass demand for education. Indeed, they themselves are ideologically committed to the provision of mass schooling at all levels.

SOME COMPARISONS CONCERNING ACADEMIC AND OCCUPATIONAL ASPIRATIONS

We may now examine how these two groups of students differ in their perceptions of their academic prospects and future occupations. This is a crucial area for research, for within the context of these slowly developing economies the possibility always arises of blocked educational and occupational mobility for later cohorts of students.

First, it is quite evident that many students in both samples tend to hold quite unrealistic expectations concerning their educational prospects. The chances of a Ghanaian fifth-form student's entering the pre-university sixth form are about one in five; in the Ivory Coast, two out of five move from the first to the second *Baccalauréat* level. However, 49 per cent of the Ivory Coast students and 37 per cent of the Ghanaians felt "certain" that they would be able to continue their full-time studies; a further 40 and 48 per cent, respectively, felt that they had an extremely good chance of so doing. Virtually no student in either sample wished voluntarily to discontinue his schooling. This is a remarkable level of commitment to a continuing education, a commitment, it must be added, which is closely linked to the occupational and status benefits supposedly imparted to schooling. However, the fact is that the educational structures of both nations are sufficiently constricted to make these hopes unrealistic; and, unless educational expansion at lower levels of the system is paralleled by an increase in the number of places available at the more advanced stage, the number of frustrated pupils is likely to increase.

Given the fact that students consistently overestimate their educational chances, what can we say about their career aspirations? Students were asked to state the job they would most like to enter if they were entirely free to choose. Table 5, therefore, really represents a profile of those jobs believed to be the most desirable within the contemporary occupational structure of the two countries.

Clearly, student aspirations are virtually identical in both samples. Most individuals aspire to professional or semiprofessional careers, as one might expect in the light of their progress so far in the educational system. Even more informative is the high proportion of students in both countries who are oriented to scientific and technological occupations. There is a persistent myth that academic secondary schools in West Africa have been busy producing a non-technologically oriented, literary elite who are committed to careers in the administration.[18] This is clearly not the case, for individuals attracted to essentially administrative functions comprise well under a fifth of the sample in both cases. It is also apparent that these students do not want to become clerks, as we are so often informed.

There are some interesting additional features. A high level of choices for medi-

[18] For a typical view see Judson T. Shaplin, "A Sea of Faces," *Bulletin of the Harvard Graduate School of Education*, VI (Summer, 1961), 4.

cine reflects the very high status traditionally accorded this occupaton in both nations, and, indeed, this has historically been one of the only professional jobs available to Africans. Conversely, the other professional occupation that has been traditionally held in such esteem in West Africa, that of law, is chosen by very few contemporary students. Only 4 per cent of the Ghanaian and less than 2 per cent of Ivory Coast pupils are interested in legal careers, whether in the administration or in private practice. One may speculate whether disinterest in this occupation, which is still very highly rated in terms of prestige and income in both countries, largely reflects the para-political position of the legal profession. Law may be remunerative; it also carries with it certain "occupational hazards."[10] This judgment would tend to be supported by the fact that very few students envisage a career in politics. In fact, less than 1 per cent of Ivory Coast pupils and only 1.5 per cent of Ghanaians even remotely contemplate political careers. This casts doubt upon some contemporary observations by political scientists concerned with Africa who argue that a high degree of political orientation characterizes potential elites. To be sure, up to the independence period, fluidity in political developments did make a political career attractive to a large number of individuals in the educated and semieducated cadres. However, at present, in both areas the stabilization of the existing regimes has led to the virtual monopolization of higher and intermediate political positions by cohorts educated in the previous period. Furthermore, careers in opposition politics are, for very obvious reasons, not attractive under present conditions.

Both groups, but especially students in the Ivory Coast, have therefore shifted their career aspirations to professional occupations very often of a scientific or technological variety, but with a very heavy emphasis on occupational security. Thus, in both samples, when students were asked what constituted the most crucial factor in determining their occupational choice, security was ranked far higher than prestige, congenial conditions of employment, pay, or promotion opportunities. Further, students were asked what type of job they could *expect* to get if they were unable to continue with their studies beyond their present class. Their answers evidence characteristics very different from their occupational aspirations (Table 6).

Very few students actually anticipate entering professional or semiprofessional

TABLE 5

OCCUPATIONAL ASPIRATIONS OF SAMPLED STUDENTS

(Per Cent)

Occupation*	Ivory Coast	Ghana
Medicine	20 5 (53)	17 3 (134)
Other professional	15 1 (39)	18.5 (143)
Higher administrative	15 8 (41)	19.4 (150)
Science and technology	39 4 (102)	28 7 (222)
Clerical	1 5 (4)	4 4 (34)
Primary-school teaching	1 5 (4)	5 0 (39)
Uniformed services	2 3 (6)	4 5 (36)
Other and no answer	3 9 (10)	2 2 (17)
Total	100 0 (259)	100 0 (775)

* Occupational categories which are not self-explanatory are as follows: *Other professional*, university professors and secondary-school teachers (in both the Ivory Coast and Ghana the latter group is accorded professional ranking), economist, statistician, etc. *Higher administrative* Executive posts within the public administration or larger commercial enterprises For convenience, we have also included politics and law within this category. *Science and technology* This includes a wide spectrum of occupations Typical are pharmacy, engineering, surveying, agricultural research, veterinary work, etc *Primary school teaching*. In Ghana, this also includes middle school teaching, which is ranked almost identically with primary-school teaching in occupational ratings made by students. *Uniformed services* Police and military.

[10] The marked "legalism" of both metropolitan powers toward the end of the colonial period insured both high income and occupational security for members of the legal profession. The politicization of bureaucratic structures since independence has greatly limited the legal practitioner's autonomous role.

occupations, while the few who expect to pursue scientific and technological careers can envisage only low-level employment as laboratory assistants or agricultural demonstrators. Overwhelmingly, both groups expect to enter only two types of occupation: clerical work and primary-school teaching, both of which are accorded only moderate ratings in terms of prestige and perceived income.[20] There is, however, an interesting reversal between the two samples regarding these two occupations. We suspect that greater Ivory Coast emphasis on primary teaching reflects the rather recently higher rate of expansion in the primary system and the subsequent increase in job openings.

TABLE 6

OCCUPATIONAL EXPECTATIONS OF SAMPLED STUDENTS

(Per Cent)

Occupation	Ivory Coast	Ghana
Medicine	0.0	0.0
Other professional	0.3 (1)	1.6 (12)
Higher administrative	1.9 (5)	0.1 (1)
Scientific and technological	6.7 (17)	8.2 (63)
Clerical	25 5 (66)	49.8 (386)
Primary-school teaching	51.4 (133)	34.5 (267)
Uniformed services	4.6 (12)	4.5 (35)
Other and no answer	9.6 (25)	1.3 (11)
Total	100.0 (259)	100.0 (775)

The table shows one thing very clearly; these students have no illusions as to the occupational currency of their education. They have attended school for anywhere between eleven and fifteen years and lie within the top 1 per cent of the population so far as educational experience is concerned. Yet, given their present level of schooling, they expect no more than clerical work or primary-school teaching. The unfortunate thing is that they are probably right.

A great deal is made of the so-called insatiable need for educated personnel in these developing areas. However, it is quite evident that the definition of "need" often depends upon an implicit comparison being made between the educational and occupational profiles of developed areas and those of Africa. However, to use Western profiles as "templates" for African development is quite unjustifiable, since in terms of the market situation in many African states the demand for educated personnel is quantitatively and qualitatively very limited. Thus in both the Ivory Coast and Ghana the bulk of new employment opportunities generated over the last few years have been in low-level teaching and clerical jobs. The paradoxical situation arises that, in spite of the limited diffusion of education in both countries, even a relatively long schooling does not guarantee high-status employment. To put it another way, the combination of limited job opportunities and a growing educational system leads to a sharp drop in the occupational returns for a given level of schooling.

These students therefore display a mixture of optimism and hardheadedness as regards their occupational future. They certainly overrate their chances of continuing their education and tend to aspire to a cluster of occupations, entry into which is essentially determined by their ability to obtain access to further studies. How-

[20] Students in both samples were asked to rank an identical list of twenty-five occupations ranging from professional to unskilled jobs. In the Ivory Coast, "primary-school teacher" was ranked ninth in terms of prestige and thirteenth in terms of perceived income. The corresponding Ghanaian rankings were nineteenth in both cases. In Ghana, in particular, the primary-school teacher is ranked only at about the level of a motor-car mechanic or petty trader. "Government clerk" was ranked fifteenth in terms of prestige and fourteenth in terms of perceived income in the Ivory Coast. By contrast, the Ghanaian group ranked this occupation eleventh in both cases. It is interesting to note that the reversed nature of the rankings corresponds to the differential job expectations of the two samples.

er, they are extraordinarily realistic in :imating what kinds of occupation their *esent* level of education will enable them enter.

In these respects the two samples are ry much alike, and they also tend to give nilar responses concerning employer eferences and location of work. The tra-:ional domination of job opportunities by vernment in both areas is reflected in substantial preference for occupations thin the public sector: over 69 per cent Ivory Coast and 84 per cent of Ghana-ı students would prefer to work for the vernment. However, significant differ-ces do occur. More of the Ivory Coast mple would prefer to be self-employed 1 per cent as against 4 per cent), while ly 12 per cent of Ghanaians would oose to work for a private employer, as ainst 18 per cent of the Ivory Coasters. . present, Ghana is rather more com-tted to the development of the economy ong socialist lines, and openings in the blic service are proportionately more :merous and more differentiated than in e Ivory Coast.[21]

Furthermore, both groups express a ·ong preference for employment in the ·gest urban centers within which the tes are overwhelmingly concentrated; nost 70 per cent of the students in both untries wish to reside in such major wns as Abidjan, Accra, and Kumasi. owever, a markedly higher proportion of ıanaians express a preference for work-g in rural areas (29 per cent as against per cent). This is surprising in view of e more urban background of Ghanaian ıdents.

SOME CONCLUDING OBSERVATIONS

Our initial problem in this paper was to licate the part played by secondary ıools in processes of elite recruitment in two new African nations. There are certainly distinct differences in the social background of secondary-school pupils as between Ghana and the Ivory Coast, and certain contrasts are apparent in some aspects of recruitment. Yet the evidence demonstrates that both systems tend to operate in a rather similar manner. In spite of a very limited number of places and formidable academic barriers to entry, the secondary schools of both nations draw upon very broad segments of the national populations. To be sure, the chances of achieving secondary-school access vary considerably as between subgroups but this does not imply that the majority of students come from privileged socioeconomic or ethnic minorities.

There has been some recent speculation regarding the political implications of the widening gap between African elites and the masses. In the case of these two countries, this concern is not justified. Post-primary education, insofar as it plays a critical role in elite formation, is still available to young people from the most "humble" circumstances and is not the prerogative of a predetermined group. In this sense, the contact between the potential elite and the masses is very marked. However, in another respect, the prolonged educational experience of this minority does set it apart from the rest of the population. How effectively secondary schooling generates a feeling of common identity among its beneficiaries is beyond the scope of this paper. It could well be that formal education creates patterns of affiliation that transcend other social and ethnic loyalties. It is only in that kind of context that we can talk meaningfully of a "gap" between the elite and the mass.

Furthermore, both groups of students are remarkably similar with respect to the nature and level of their aspirations and expectations. Moreover, both face a very restricted range of occupational opportunities. In effect, secondary education up to this level no longer allows automatic access to high-status occupations and roles. Its

[1] Probably about 60 per cent of employment hin the modern sector of the Ghanaian economy with some form of public agency, while in the ·ry Coast this figure is approximately 25 per t.

importance has come to lie in the degree to which it feeds into higher education. Completion of the fifth form or the first *Baccalauréat* now only enables individuals to enter occupations that up to a decade ago were filled mainly by primary- or middle-school leavers. Preoccupation with job security among students may well reflect their difficulties with respect to employment.

Most of the new African states share a primary commitment to economic development plus a belief in the key role to be played in this respect by the educated cadres. Yet we have seen that both samples are oriented toward government employment in the largest urban centers and attach overwhelming importance to occupational stability. Such individuals, we suggest, are not highly likely to emerge as potential innovators in the field of economic development. At the present time, indeed, African entrepreneurs are usually far less educated than members of the bureaucracy. Insofar as the latter group tends usually to limit the autonomy of economic activities, it will be interesting to see to what extent effective communication can be maintained between bureaucratic and entrepreneurial elites with very different levels of formal education.[22]

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[22] For an interesting discussion of the relationships between bureaucratic and entrepreneurial elites in the new nations, see S. N. Eisenstadt, "Problems of Emerging Bureaucracies in Developing Areas and New States," in Bert F. Hoselitz and Wilbert E. Moore (eds.), *Industrialization and Society* (The Hague: UNESCO and Mouton, 1963), pp. 167–68.

COMMENTARY AND DEBATES

On the Relation of Patron and Artist: Comments on a Proposed Model for the Scientist

In an article on employer and employee in research organizations, Professor William M. Evan has recommended the relationship of patron and artist as a model for the scientist in industry.[1] He believes that if this model were accepted the strains felt by the scientist in his present dependence on research organizations would be reduced. I should like to discuss Evan's conception of the model from the point of view of an historian of art. My aim is not to judge how adequate it is for a modern scientist, but to consider the correctness of Evan's description of the model constructed from the past. My comments will perhaps have some interest for his problem.

Evan starts from the notion of an "ideal-typical patron-artist relationship . . . in which the patron affords the artist the freedom to pursue, in his own manner, his artistic impulses. The conception of 'an autonomous, non-utilitarian art enjoyable in itself' or 'art for art's sake'—already familiar to the Greeks but forgotten in the Middle Ages—was rediscovered in the Renaissance."

The relation of patron and artist in older times was quite different from what appears in Evan's account. In the Greek and Roman world as well as in the Renaissance, most original works of painting and sculpture were made to order for a definite purpose. A sculptor produced for the decoration of a building in progress, a temple, a funerary monument, an arch or column of triumph. The subject matter was prescribed, and the materials, size, location, and other features were specified by the individual or the corporate body that commissioned the work. From the Renaissance have survived many contracts with artists, which designate the subject and other aspects of the finished painting or sculpture. All this was compatible with the concept of an art "enjoyable in itself"; there are frequent testimonies to the beauty of a work that say nothing about its use. But the old artists, unlike those of our time, produced little spontaneously, without a commission.

Beside this elaborate commissioned work, often requiring several years of effort and costly materials, there was an industrialized art for the market. Just as the jeweller and the potter made their artistic objects in their shops, anticipating the purchasers at the fairs or in the street, so there was a production of small sculptures and paintings for the public, at the artist's risk. The Tanagra terra cotta figurines, the woodcuts and engravings of Dürer, etc., were made for unknown but expected purchasers. These, too, we have no doubt, were objects of aesthetic pleasure to the owners; the buyers of ready-made prints in the sixteenth century were often connoisseurs, seeking choice examples for their collections.

Contrary to the general belief, there existed in the Middle Ages, besides the commissioned works, a practice of art for its own sake, independent of a market. In the monasteries between the seventh and the fifteenth centuries monks decorated the service books of the church and other codices which were produced in the monastic scriptoria with a wealth of ornament and imagery that were valued for their

[1] "Role Strain and the Norm of Reciprocity in Research Organizations," *American Journal of Sociology*, LXVIII (1962), 346-54, esp. pp. 351 ff.

beauty. The paintings in those precious manuscripts could hardly have been designed for the instruction of the illiterate, as is sometimes supposed. The monk-artists, like the scribes, were not paid, nor were their works usually offered for sale. They were spontaneous products of the delight in beautiful forms and colors, the outcome of an artistic impulse operating within a religious milieu on tasks set by the conditions and needs of Christian cult.[2] They offer innumerable examples of free fantasy through which the artists "lived out" desires and fears, unconstrained by the religious nature of the objects in which these were expressed. The bishop, abbot, or priest who ordered a work, whether from a monk or a paid lay craftsman, did not always stipulate the character of the decoration—in some instances the bishop or abbot himself was the artist. Much stone sculpture on Romanesque and Gothic buildings, though commissioned by the church, has no apparent theological meaning, and contemporary writings abound in statements about the charm and imaginativeness of new works of art. Through these creations a church appeared more beautiful, more impressive, and its prestige was all the greater.[3]

That artists during this period were conscious of their creative powers and affirmed the personal merit of their achievement is clear from the numerous signatures and accompanying inscriptions on works of art. I shall cite one example as evidence of the self-esteem of a medieval artist even under a controlling patronage. On the great gold altar of the church of San Ambrogio in Milan, the sculptor of the ninth century not only signs his name "*Wolvinius magister phaber*" but represents himself being crowned by the patron saint of the church in a medallion that is a pendant to the image of the archbishop Angilbertus who had commissioned the altar. Angilbertus is shown presenting the altar to Saint Ambrose who crowns the donor.[4]

Evan supposes that after the Renaissance

> patrons have tended to deviate in at least two directions from the ideal-typical patron-artist relationship: They have assumed either the status of an "employer" vis-à-vis the artist or that of a "client," so to speak. In both cases these forms of deviation from the ideal type impaired the autonomy of the artist. In the status of an "employer," the patron feels that he has the right to specify precisely the end product of the work of the artist by virtue of his formal or informal contractual relationship. He believes that he is entitled to dictate not only what the employee should do but how he should do it. Indeed, in the pre-Renaissance period, artists had a status comparable to employees; in fact, they were commonly called "artisans" rather than artists. And their work was closely regulated by the dictates of the Church and the guilds.[5]

This account of the artist's state between the Middle Ages and the post-Rennaissance period under the dictation of patrons corresponds to a view that is held by some historians, and Evan can refer to recent writings as authority for his account. However, as I have indicated already, during the Renaissance, which is described by Evan as an interlude of spontaneity of self-directed artists, the many contracts that have survived show that the painter agreed to stipulations of a most detailed kind, no less binding than those that governed the execution of many medieval works of art. Surely the artist made some works independently, but most of the

[2] For the production of precious illuminated manuscripts in the monasteries, see the richly documented work of E. Lesne, *Histoire de la propriété ecclésiastique en France, IV: Les Livres* (Lille, 1938), esp. pp. 80 ff., 319 ff. See also Vol. III, *Églises et trésors des églises* (1936), pp. 180 ff., on work in gold and silver.

[3] On this aspect of medieval art, see my "The Aesthetic Attitude in Romanesque Art," *Art and Thought, Essays in Honour of A. K. Coomaraswamy* (London, 1947), pp. 130–50; and also *Art Bulletin*, XLV (1963), 351 ff.

[4] See Arthur Haseloff, *Pre-Romanesque Sculpture in Italy* (Florence and New York, n.d. [*ca.* 1930]), p. 64 and pl. 72.

[5] Evan, *op. cit.*, p. 351.

important pictures and sculptures of the Renaissance were done to order for wealthy and powerful individuals or corporate groups. There exist very few works by Raphael or Michelangelo that were not commissioned.[6]

Yet this dependence on an order did not mean, at that time or in the preceding and following periods, that the patron specified "precisely the end product of the work." No layman could possibly do that, and not even the artist himself could say in advance what the end product would be "precisely." If the contracts prescribe the subject matter, the dimensions, the character of the frame, the use of gold and of good colors, the number of figures, or the relation to neighboring work, on all that made the result valid aesthetically for the taste of the time the contract had nothing or little to say (unless that the whole or a part be executed by the master and not by assistants). And even in those rare instances in which the patron asked for a particular style, this request was like the demand that a piece of music be in a certain mode, that a poem be a pastoral or an elegy, or that the building be in the Tuscan or Doric order—demands that artists were prepared to satisfy as traditional tasks inherent in their art and for which they possessed the necessary skills. In principle the commissioning of an artist for a desired work implied approval of his individual approach and confidence in his ability to realize the program of the commission in his own respected and admired way. When Nicholas Poussin's patron, Chantelou, complained in 1647 to the painter that the picture Poussin had done for him pleased him less than a painting the same artist had done for another patron, Poussin replied: this is what you asked for. He explained that it was the subject as well as Chantelou's disposition that accounted for the difference in effect, and that the subject he treated for Chantelou had to be represented in another expressive "mode" (which he compared to the modes in music) in order to bring out its character.[7]

There is a remarkable episode of the seventeenth century that permits us to see more clearly these relationships of painter and patron. It concerns the now-famous picture of Aristotle contemplating the bust of Homer, made by Rembrandt for a Sicilian nobleman, Antonio Ruffo. This patron, desiring a pendant to the picture, commissioned an Italian master, Guercino, to provide a second work that would go with Rembrandt's, and he asked the Italian to paint it in his old style, which was akin to Rembrandt's; Guercino, who was working then in a more classical manner, had begun as a painter in strong light and shadow. Guercino consented and produced a mate for Rembrandt's work, a picture of an astronomer touching a globe, which we know only through a drawing that has survived. It is doubtful that Guercino felt the Count's commission to be an impairment of his autonomy; on the contrary, as we learn from his letter, he was pleased to be invited to produce a work to hang beside that of Rembrandt whom he admired as a great artist.[8]

What was indeed a source of friction between artists and their patrons or clients was the notorious slowness of artists to deliver. We have many documents that speak of the long delays of the artists and the impatience of the patrons. Some work dragged on for years, interrupted by new commissions accepted by the artist. The

[6] On the Renaissance artists and their patrons, see Martin Wackernagel, *Der Lebensraum des Künstlers in der florentinischen Renaissance, Aufgaben und Auftraggeber Werkstatt und Kunstmarkt* (Leipzig, 1938), and for these relationships from the ancient Greeks to 1800, see Rudolf and Margot Wittkower, *Born under Saturn: The Character and Conduct of Artists: A Documented History from Antiquity to the French Revolution* (New York, 1963). I reviewed this last book in *New York Review of Books*, I (November 28, 1963), 7.

[7] *Lettres de Poussin*, ed. Pierre du Colombier (Paris, 1929), pp. 239, 240.

[8] For the whole story and the text of Guercino's letter, see Jakob Rosenberg, *Rembrandt* (Cambridge, Mass., 1948), I, 165 ff.

patron often paid in advance in regular installments in order to keep the artist working by making him a debtor. On the other hand, the patrons were often as slow in paying for completed work. Many are the letters of artists addressed to their patrons reminding them of a still-unpaid debt.[9]

To say that "in the pre-Renaissance period, artists had a status comparable to employees" because "their work was closely regulated by the dictates of the Church and the guilds" is to miss the point. As "artisans" they owned their own tools, they were "masters," sometimes with a large workshop employing many assistants and apprentices, and the guilds were their own organizations in which they held office. In settling disputes with a patron, the arbitrators were often artists. It is highly unlikely that the sculptors and painters felt the requirements specified in their commissions by the church or the rules of workmanship and quality set by the guilds to be infringements of their artistic freedom, no more than an engineer today regards the specifications for the work to be done as a constraint on the liberty of his profession. On the other hand, there is the episode of the great Brunelleschi whose contract to build the dome of the Cathedral of Florence was contested by the guild because he had failed to pay his dues; the officials of Florence forced the guild to waive its objection. What was in question here was not the artist's freedom to design a work in his own highly original way, but his right to undertake the job.[10]

The distinction made by Evan between patron and client, that the patron as "client" "throws himself, as it were, on the mercy of the artist in the hope that the artist, in the capacity of a competent 'professional,' will provide him with the best possible service,"[11] while the patron in the Renaissance sense asks nothing specific of the artist, simply affording the latter "the freedom to pursue, in his own manner, his artistic impulses," seems to me an imaginary, fictitious one. If whoever today commissions a work from a painter or sculptor is uncertain of the outcome, it is not because, unlike the Renaissance patron, he asks something specific of the artist and thereby impairs the autonomy which in the older period guaranteed the artistic success of the project. We know that the Renaissance patron did ask something very specific of the artist and that he had at times to regret the result. Then as today there was the risk for the architect's client that the work might be carried out with inferior materials.

The Renaissance patron was very rarely an individual who supported the artist while the latter "pursued his artistic impulses" freely from an idea of "art for art's sake"; his relation to the artist was that of a powerful client who paid the artist to carry out for him particular works of a character and scope that the artist could not possibly undertake unless the cost of material and labor (including that of assistants in the workshop) was guaranteed in advance by a contract. These works became the property of the patron who enjoyed through them (whether he kept them or gave them to a church) great prestige in a community where objects of art were signs of wealth and power, as well as of culture.

There are, of course, examples of a

[9] See Wackernagel, *op. cit.*, pp. 344, 350, 355, 357, 370 ff.; and Wittkower, *op. cit.*, pp. 35 ff., 40 ff. (on the dilatoriness of artists), 59 ff. (on "creative idleness"). These reports are worth remembering with regard to Freud's interpretation of Leonardo's slowness of work as a neurotic sign. It was evidently a more common characteristic than Freud suspected. If one believes that Leonardo's failures to deliver were greater than those of other artists, it is also true that he has left us more writings, scientific observations, and technical inventions than any artist of the Renaissance, and perhaps of all time.

[10] The guild of stone and wood workers had Brunelleschi arrested and imprisoned for debt while he was serving as architect of the cathedral in 1434, but the authorities of the project got him out quickly and punished the guild officials by imprisonment (see Wackernagel, *op. cit.*, p. 308).

[11] *Op. cit.*, p. 351.

patron with a personal interest in an artist, approaching true friendship, like Vasari's account of the Florentine Martelli inviting the young Donatello to live as a guest in his house. But this was exceptional in the Renaissance and did not affect the typical economic and legal relations of artist and patron during that period, just as in the nineteenth century the many examples of the young protégé artist did not change the prevailing character of painting as work done for an open competitive market. It has happened in the last hundred years that an individual has supported a painter (more often a poet or composer) with an ideal conception of their relationship, with a disinterested generosity inspired by a love of art for its own sake, and free from thought of self-advancement through the association with art or possession of valuable works. More common is the episodic patronage that expresses faith in a young or unknown artist by purchasing his completed work. Often, in what is called patronage of art today, we discover various ambitions, operating with established values, or a parallel activity of the enthusiastic worldling of culture, like that famous Count Kessler who commissioned works for his private collection, published books illustrated by the artists, and participated thereby in the movement of the arts.

Even such patronage has been extremely rare. I would venture the guess that if there are twenty thousand persons in this country who can be called professional painters and sculptors, less than one hundred or one-half of 1 per cent enjoy the continuous support of an undemanding patron. I do not include those students and young artists who are maintained by their families during an early period in their careers before they have established themselves as original artists.

On the other hand, there does exist a system of patronage in art today which provides an artist prolonged support without prescribing or commissioning particular works. This is the relation of dealer and artist: the former agrees to pay the artist a fixed sum monthly in return for which the artist will deliver to him either all his work or a certain number of works, of which the market value, agreed upon by dealer and artist, will equal the sums paid in advance. Such understandings rest on the dealer's confidence that these are good artists and on his expectation that their work will be recognized in time. Should he be disappointed, the contract will not be renewed. Often such agreements include a clause that the dealer has the exclusive right of exhibition and sale of the artist's work during the period of the contract. The painter is not the dealer's employee, nor is he a professional who renders the dealer a service; the relation is that of two entrepreneurs, one a merchant, the other a producer in some respects like the artisan or manufacturer who sells a product in quantity to a retail house. An important difference from the latter is that the best artists today produce freely and inventively, without thought of adapting their work to a particular demand, and continually risk failure by trying out new ideas of form and color. In this they are more like inventors and industrialists who experiment with new products.

There is another system in which the artist agrees to exhibit and sell only through a particular dealer and receives occasional advances on work in progress; or without such advances, he finds in the relation to the dealer a certain security, since the dealer will not only give him a one-man exhibition, but will continue to show his work to clients throughout the year. The dealer, who sets the prices, usually with the artist's consent, takes a third or more of the sales price for himself. The dealer pays the cost of the catalogue, the advertising, insurance and storage, and has, of course, the expense of the rent of the gallery and its staff. From less-established artists the dealer may ask a larger share of the return.

In this contractual relation are many strains and difficulties. The dealer may be

dissatisfied with the artist, either because he has produced too little altogether or falls below the expected level of quality. The artist may not approve the looks or location of the gallery, the method of publicity, the clientele, the dealer's personality and calculations; he may feel less favored by the dealer than other artists in the same gallery who are more successful in attracting purchasers; he may suffer because the works of the others cast a shadow on his own, because they are better or worse, or in a style with which he feels no sympathy. There are then many tensions within this freely chosen relationship of artist and dealer. In general, an artist desires a dealer who admires his work wholeheartedly and will continue to show it even though there are few sales; who gives no sign of disapproval when the artist changes his style and passes through a period of searching or experimentation, without fully realized results; whose publicity is dignified, original, or personal in a way that appeals to the imagination and taste of the artist; and who respects the personality of the artist. There have been dealers for whom the art business was a genuine personal expression, a means of participating in the deeply interesting, often exciting world of original artists, with whom they have formed lasting friendships. Such dealers are uncommon (I may cite those I have known personally—D. H. Kahnweiler, Curt Valentin, J. B. Neumann, and Betty Parsons) and many artists have a tolerant or resentful attitude to their dealers, according to the degree of help they receive from the galleries.[12]

In all these types of relationship one feature remains constant; the artist is a producer who possesses his own tools of work and is personally responsible for his entire product, without any division of labor. (For the sculptor as for the graphic artist there is sometimes the collaboration of the reproducing craftsman—the caster, founder, or printer—but the result as a totality is recognized as the artist's.) As such, he is an exception in modern society, an anomalous survival of the medieval artist.[13] He produces for an uncertain market, but is sustained by the thought that if he is successful the reward both in money and in social esteem will be very great, beyond that of any craftsman, ancient or modern. Since the value of his work is uncertain, and he must often produce for long periods without hope of sales, his profession is speculative. From this situation arise certain features of the artist's outlook and behavior or, at least, the situation seems to justify and support them: his feeling of separateness, his sympathy with the marginal, his informality of manners, and greater freedom in other contexts of moral and social life.

From this account of the complex of artist-patron-dealer and the art market, it will be clear that the relations of the scientist to his employer or institution are of a different order. The scientist, except for the mathematician and perhaps the theoretical physicist, requires costly instruments of research, laboratories and libraries and a staff of technicians, which make his activity part of a collective task, often with a high division of labor. His products are not material objects that possess a market value like pictures and statues, as unique works of art, but contributions to a common fund of knowledge in which they are quickly absorbed and transformed by the work of other scientists. Apart from

[12] The reader interested in the relations of dealer and artist will find the following books instructive: L. Venturi, *Les Archives de l'Impressionisme* (2 vols.; Paris and New York, 1939), which includes the correspondence of the Impressionist painters with their dealer, Durand-Ruel; Van Gogh's *Letters*, mainly to his brother Theo, who was a dealer; Ambroise Vollard, *Souvenirs d'un marchand de tableaux* (Paris, 1937); and particularly important for contemporary art, Daniel-Henry Kahnweiler, *Mes galéries et mes peintres. Entretiens avec Francis Crémieux* (Paris, 1961)—a book rich in details of the story of the Cubist painters of whom Kahnweiler was the wonderfully understanding first supporter and spokesman.

[13] I have discussed this aspect of the painter's profession in *Art News*, Summer, 1957.

increases in salary, he has no financial advantage from his results once these have been published, unless they include something that can be patented as a commercially exploitable product or process. Nearer to the artist is the old-fashioned inventor who worked alone, expecting a high reward for a single result that had a price in the market. The scientist, on the other hand, has for several generations been a teacher, a university professor, an employee of a research laboratory or institute, and in adopting his profession anticipates a secure connection with a stable source of livelihood. He is himself an important part of institutions that support his work; without him, they would not exist. The scientist in research organizations or industry is better compared perhaps with the commercial artist who has acquired a rare skill which he offers for sale, and not with the "fine" or "pure" artist who offers in the market his finished work, not his skill or labor power.[14]

If government, industry, or foundation in our society adopts the role of patron of science without setting definite tasks, and allows the scientist himself to choose his problems, it is doing something that has never been done on a large scale in the field of art, where the patron of this kind has been, in the past, a solitary and exceptional individual. In small countries, where an artist has appeared to be a great genius and added luster to the name of a community, he has sometimes been given a studio and a pension. But this could not be the model for the totality of artists.

The recognition of science as a good in itself that requires and deserves support is a product of culture, not economic calculation, although this recognition has advanced through the economic calculation (and often masks the latter).

What Evan describes as an ideal patron-artist and patron-scientist relation exists to some extent in the Soviet Union, where the state supports basic research and the arts without setting definite tasks. It is assumed that the artist shares the government's conception of art and is therefore not constrained by it. His "artistic impulse" is supposedly in harmony with the requirements of the state, as the artistic impulse of the painters of the Renaissance was in harmony with the outlook of their art-loving patrons. But while basic science and technology of a high order have been produced in Russia, we have seen during the last thirty or more years how this organized patronage limits the freedom of artists and has made Russian painting a mere instrument of the state—sterile as art, though perhaps useful in promoting official beliefs. Yet in the first years after the Bolshevik Revolution, Russian painting and sculpture, continuing along the paths of advanced art of the previous decade, were in the forefront of original European art. Supported by the state, many avant-garde artists felt enthusiasm for the Revolution and were not yet restricted by demands of ideological conformity in the style of their work.[15]

MEYER SCHAPIRO
Columbia University

[14] Exceptional is the practice of corporations which order works from outstanding artists. To advertise their product the Container Corporation has commissioned painters and sculptors to produce works interpreting a philosophic statement by a leading thinker in the Western tradition. Such projects are welcomed by certain artists, but as isolated occasions have little or no effect on the practice of art or the workings of the art market.

[15] See Camilla Gray, *The Great Experiment: Russian Art 1863–1922* (New York, 1961), pp. 215 ff.

Reply

My admiration for Meyer Schapiro's erudition in the history of art is such that I consulted him—even before I wrote my article—on the question of the utility of the Renaissance patron-artist relationship for understanding the working relations of

the modern scientist. His foregoing comments and anecdotal observations raise some additional questions which, of course, are no substitute for a systematic study of the evolution of the patron-artist relationship from the Renaissance—or even earlier historical periods—to the present.

Whether the ideal type of Renaissance patron-artist relationship—as I have inferred it from the work of some art historians—has some relevance for an analysis of the role of the scientist in the twentieth century is an empirical question. In raising this question in my article, it was my hope that it might stimulate others to undertake the complex but intriguing study of the factors accounting for the changing nature of the patron-artist relationship.

WILLIAM M. EVAN

Massachusetts Institute of Technology

LETTERS TO THE EDITOR

Lott, Negro and White Youth

June 24, 1964

To the Editor:

We would like to comment briefly on Thomas Pettigrew's review of our book, *Negro and White Youth,* which appeared in the March, 1964, issue of this *Journal.*

Mr. Pettigrew reported that he was frustrated by "unimaginative analysis," and went on to offer the following one example to his readers: "For instance, there was less sex differentiation in interests among Negroes than among whites—a finding the authors interpret as a function of the greater absence of fathers in the Negro homes. Yet no comparisons within the Negro group are presented that test whether in fact the father-absent students reveal less sex differentiation (as they do in other research)." While Mr. Pettigrew's report of our finding is correct, his version of our interpretation is not. The suggested test of his hypothesis is thus quite inappropriate as a test of ours.

In the final chapter of our book, under the heading "Sex Differences as Related to the Negro-White Variable," after reviewing the relevant data, we wrote:

> All these findings, considered together, suggest that the usual sex-typed goal orientations found among white youth do not exist as clearly among Negro youth. The greater similarity between Negro boys and girls may be related to less sharply delineated and differentiated sex roles of father and mother within Negro, than within white, families. The relative instability of the Negro home, the higher probability that a Negro mother is contributing to the family income by outside employment, and the higher probability that the Negro father is absent from the home, contribute to the lesser likelihood that father and mother roles in the Negro home are as distinct as they are in the average white home [pp. 161–62].

Our emphasis is clearly not on the higher probability that the Negro father is absent from the home. This is but one of several conditions which we believe may contribute to less distinct adult sex-role models being available to Negro than to white children, thus resulting in greater similarity between Negro boys and girls than between white boys and girls. Congruent with this general hypothesis is our finding "that among the Negro leaders, the sexes are somewhat more distinct than is the case within the total sample of Negroes." We pointed out that "home conditions of leaders were found to be relatively more advantageous than those of the other Negro seniors; fathers are better educated and working at higher status jobs, for example, and homes

tend to be less crowded. It can be assumed that such homes offer a somewhat better possibility for the learning of differential sex roles."

What is significant, in our view, is not necessarily a father's presence in, or absence from, the home, but rather his role within the home relative to the mother's (his status, his contribution to family comfort and well-being, etc.). This idea is, we believe, contained quite unambiguously in the above-quoted material as well as in other places in the book where the question of sex differences is treated.

ALBERT J. LOTT
BERNICE E. LOTT

University of Kentucky

Reply

July 13, 1964

To the Editor:

The chief burden of the few sentences selected by the Lotts for detailed comment is simply that the analyses provided in their volume are disappointingly unimaginative. The authors apparently do not contest this point so much as the incompleteness of the example cited in the review. Indeed, the example is incomplete, but the point remains. Unfortunately, a brief review does not furnish space for as thorough coverage of each point as does the above letter. Had the space been available, however, the basic point could have been even better illustrated. Father absence *is* part of the Lotts' explanation for less sex differentiation in the test results of Negro sample members, and it is never directly tested. But other parts of their explanation—working mothers, general family instability, less distinct roles of father and mother—are never directly tested either. (Comparisons between the leader and general sample are neither direct nor adequate tests.) Many interesting theories have been applied to Negro American personality trends; what is needed most now are thorough and skilful data analyses to test these theories—and these, regretfully, *Negro and White Youth* fails to present.

THOMAS F. PETTIGREW

Harvard University

July 10, 1964

To the Editor:

The Sidney Siegel Memorial Fund Committee wishes to announce that free copies of the following books—Siegel and Fouraker, *Bargaining and Group Decision Making;* Fouraker and Siegel, *Bargaining Behavior;* Siegel, with Andrews and Siegel, *Choice, Strategy and Utility;* and Messick and Brayfield (eds.), *Decision and Choice, Contributions of Sidney Siegel*—are available to students, in the United States or abroad, having a scholarly interest or being engaged in research in the area of individual-choice behavior and bargaining behavior. To obtain copies of these monographs, the student should ask his major professor to write to Preston Cutler, Center for Advanced Study in the Behavioral Sciences, 202 Junipero Serra Boulevard, Stanford, California, stating the name and address of the student and specifying which book is desired. Contributions to the fund have been made by Fellows of the Center for Advanced Study in the Behavioral Sciences of the classes of 1957–58 and 1961–62.

LAWRENCE KOHLBERG

Department of Psychology
University of Chicago

August 11, 1964

To the Editor:

I should like to amplify my review of Professors Coale and Zelnick's *New Esti-*

mates of Fertility and Population in the United States in the July issue of this *Journal.* The statement with which I ended was in no way a qualification of the one at the start of the review—that the figures of this book will be the standard source on the subject they cover. That my own attempts to duplicate the results were ineffectual is not a comment on them but an argument for authors providing arithmetical examples along with the discussion of the general rules they apply. The reader who can reproduce at least some of the final tabular numbers in the counters of his own desk calculator will have a confidence in the results that accords with the care with which they were compiled for this book.

NATHAN KEYFITZ

University of Chicago

Supplementary List of Doctor's Degrees in Sociology, 1963 and Doctoral Dissertations Newly Started in 1963

The following names were submitted to the *Journal* subsequent to the listing in our July, 1964, issue:

STANFORD UNIVERSITY

Doctor's Degrees Conferred in 1963

Edward Joseph Glass, A.B., A.M., Stanford, 1946, 1951. "A Study of Institutional Religious Behavior and Security-Insecurity Feelings: A Comparative Study of Religious and Non-religious Respondents."

Irving Tallman, B.A. University of California, 1948; M.S.W. Wayne State, 1950. "An Experimental Study of Normlessness."

Leslie Carleton Waldo, A.B., M.S.W. University of California, 1949, 1952. "The Educational Aspirations of Early Adolescent Boys: A Sociological Study."

Doctoral Dissertations Newly Started in 1963

John Robert Howard, B.A. Brandeis, 1955; M.A. New York University, 1961. "A Progressive Commitment: A Theory of Organized Involvement."

Thomas Ferdinand Mayer, A.B. Oberlin, 1959. "Non-linear Models for Multiprocess Theoretical Systems."

Thomas Edward Ryther, B.S. University of Kansas, 1955; M.A. Stanford, 1962. "Social-Psychological Factors in the Response to Pain-relieving Drugs."

Gary I. Schulman, B.A. Brooklyn College, 1959; M.A. Stanford, 1961. "The Two-Step Flow of Mass Communications: A Test of a Reformulation."

Paul Verden, A.B. Harvard, 1958; M.A. Stanford, 1960. "Linguistic Dimensions of Social Deviance."

Donald Whiteside, B.S. Wisconsin State College, 1958; M.S. University of Wisconsin, 1960. "Kinship Ties and Social Mobility."

UNIVERSITY OF WISCONSIN

Doctor's Degree Conferred in 1963

Jeffrey Keith Hadden, B.A., M.A. Kansas, 1959, 1960. "A Systematic Study of the City as a Unit of Analysis."

Doctoral Dissertations Newly Started in 1963

Gustavo Jimenez Cadena, B.A. Colegio Maxima, S.J.; M.A. Universidad Javeriana, 1953. "The Role of the Rural Priest in Central Colombia as an Agent of Social and Economic Development."

Kenneth John Duncan, B.A., M.A. Toronto, 1950, 1951. "Racial Identification and Academic Performance on Two Indian Reservations."

Franklin Oscar Leuthold, B.S., M.S. Ohio State, 1959, 1960. "Acceptance of New Farm Technology: A Test of a Theory of Social Interaction."

Herbert Hoover Lindsey, B.A. Talladega College, Alabama, 1955; M.A. Wisconsin, 1962. "Indigenous Migration Related to the Formation of the Labor Force in the Union of South Africa."

Charles Henry McCaghy, M.A. Wisconsin, 1962. "Psychiatric Decision-making in the Sex-Offender Commitment Process."

James Ronald Pinkerton, B.A. Carroll College, 1954; M.S. Wisconsin, 1962. "The Residential Redistribution of Socioeconomic Strata in Metropolitan Areas."

Herbert Lavon Smith, B.S., M.A. Houston, 1957, 1962. "Work and Authority Patterns and Expectations in Wisconsin Farm Families."

Subhash Ramanna Sonnad, B.A., M.A. Bombay, 1950, 1956. "Part-Time Farming in Wisconsin."

Betty Isabel Sperling, B.A. Brown, 1961; M.A. Cornell, 1962. "Social and Psychological Factors Influencing Material Success."

Donald Wesley Thomas, B.S., M.S. Ohio State, 1960, 1961. "Area Setting as a Factor in the Growth and Decline of Small Towns in the United States."

BOOK REVIEWS

The First New Nation: The United States in Historical and Comparative Perspective. By SEYMOUR MARTIN LIPSET. New York: Basic Books, Inc., 1963. Pp. xi+366. $5.95.

This is surely one of the few most important books by an American sociologist to appear in a long time. It seems to me to signal a new level of maturity both in the work of its author and a highly significant recent trend of sociological work, namely, the attempt to treat societies in the large and to do so in a comparative and historical framework.

With respect to the maturing of Professor Lipset's own thinking and research I may first state that, as high as my opinion of his *Political Man* was, I had found it in certain respects a disappointing book, above all because it seemed to be too much a collection of relatively discrete essays that had not been adequately integrated with one another. Since I had the privilege of seeing an earlier manuscript version of the present book I had similar misgivings about it at that time. The author has, however, very substantially improved this earlier draft and has, I think, succeeded in notably integrating his materials so as to present a very impressive picture of some of the most important aspects of our society. It seems that the focus on the one particular society has helped him greatly in this achievement. But equally important to my mind is the fact that he has not presented just another historical essay on the United States, but has taken the method of comparative and developmental analysis with the greatest seriousness. The comparisons, first, of early American society with the contemporary new nations, and second, with the other variants of Western democratic society, first in the English-speaking world and then in other European societies, are both genuinely illuminating and furnish the most important empirical control on the arbitrariness of his own interpretive judgments of the immensely complex material.

Lipset's effective use of comparative and developmental methods depends, of course, not only on his impressive command of the empirical materials and their sources, but also on explicit theoretical analysis. In this respect the book can be said to present one of the best available demonstrations of the usefulness of such analysis and at the same time to provide one of the most impressive empirical validations of the type of theory he uses that has so far been achieved.

It is of particular interest to the present reviewer that Lipset has taken the concept of values as a constitutive part of the structure of the society as the focus of his theoretical analysis. He delineates a broad pattern for the American case which involves taking account of both the religious background along the lines of Max Weber's analysis of the values of the ethic of ascetic Protestantism, and the values expressed in the documents of the founding period of the Republic. Then, particularly by judicious use of the comments of foreign observers at different periods, he is able to build a strong case for the essential constancy of the main pattern through all the immense structural changes the society has undergone. Related to this in turn is his mobilization of evidence that many of the traits currently adduced to demonstrate a change of values, notably "other-directedness" and an emphasis on conformity, are as old as the society itself, as many observers have noted.

In terms of content Lipset's analysis turns on the balance between two primary components, those of equality and of achievement, both, of course, against the background of a basic individualism. The important point here is the analysis of a *balance* of components that are partly inherently linked with each other but at the same time partly in conflict in such a way that behavior interpreted as a way of dealing with the conflict provides some of the most important clues to understanding otherwise puzzling features of the American scene; for example, a certain exaggerated social snobbery among Americans is interpreted as dynamically related to our equalitarian values under the conditions of competition for oppor-

tunity and at the same time, valuation, of individual achievement.

Lipset is able to use his comparative evidence with particular effectiveness in this sort of problem. Thus he shows that the British, and to a lesser extent the Canadians, have had very different characteristics from Americans because of the greater elitist as distinguished from equalitarian emphasis in their values, whereas Australia has been in certain respects more equalitarian than the United States and has therefore treated the problem of recognition of achievement differently. In all this Lipset is anything but a naïve "emanationist" advocate of the importance of values, as his eminently realistic treatment of a whole range of problems, such as that of national unity in the earlier phases of American development clearly shows.

It goes almost without saying that such a book will also take an important position in the current debate among American intellectuals—and their brethren in other countries—about the nature of American society in relation to the conflict between what in some sense are "conservative" and "revolutionary" values. Here Lipset takes a definite and explicit stand, which he backs with an immense accumulation of evidence, that the basic American orientation not only has been, but remains, liberal if not revolutionary. Indeed it is his view that American radicalism, if one may use that term, is more authentically at the core of the main liberal trend of modern society than is the Marxist version. A most important point noted by Lipset in this connection is that the only seriously "alienated" large group in American society is the radical right, the "dispossessed," as Daniel Bell calls them.

In sum, this is an impressive and a very challenging book. It will certainly be sharply attacked by the group that basically disagrees with its diagnosis of American society, but its level of both empirical and theoretical competence and achievement is such as to present a formidable task to him who would refute its main generalizations.

TALCOTT PARSONS

Harvard University

Social Structure and Personality. By TALCOTT PARSONS. New York: Free Press of Glencoe, 1964. Pp. 376. $8.50.

Here are twelve papers, each published previously and, in most cases, written within the last eight or ten years. Together, and for one area of its application, they show the mature power of Parsons' theoretical program as sketched just after the war.

Most sociologists have read three or four of these papers: "The Superego and the Theory of Social Systems," "The Incest Taboo . . . ," perhaps "Definitions of Health and Illness in the Light of American Values and Social Structure" or "Social Structure and the Development of Personality. . . ." The remainder are not so well known. Each applies the theory of action to problems of personalities in society and of the society in personalities.

There are, for example, four essays on stages of the life-cycle. These analyses move appreciably beyond familiar interpretations of the early stages of childhood. One deals with the organized classroom of the elementary and secondary school, sketching its structure and its role in the wider society. By developing and evaluating the child's moral and intellectual abilities, the schools train and select participants for adult roles. Peer groups train children for later relations with equals. Sex segregation in latency reinforces the individual's identification with his sex role and assists him in overcoming incestuous attachments to his family. By having to cope with several teachers, the child learns to relate to the role of official socializer rather than merely to particular individuals. Indifference to school represents not alienation from cultural values but difficulty in approximating values to which one is committed, values that must be realized through success in a competitive situation in which the stakes are very high.

A second study of the life-cycle tells something of what it means to be a young man or young woman in modern America. America's predominant values have remained relatively stable. The dominant pattern is one of instrumental activism. American society and culture are becoming more inclusive, differentiated, upgraded, and generalized. As a consequence, skills and responsibilities required of young adults are greater than those imposed on any earlier generation. Another essay, with Winston White, reviews Riesman's typology of character. The shift from tradition direction to other direction reflects a social commitment

to ever more generalized and differentiated institutional guides for conduct rather than a collapse of normative relations. There also is a paper on the ambiguities and opportunities of old age.

In introducing works by Weber, translators typically remark on the author's convoluted style, and one of them has declared in extenuation that Weber sought by this method of presentation to represent through language the flowing, multidimensional quality of social life. These clear and graceful papers of Parsons show his gradual mastery of an expository method true to social complexity yet embodying analytic possibilities which language alone confers. This style of exposition is no accident. It seems to me a direct outcome of his prolonged theoretical efforts, these now affording him a generalized framework and vocabulary suited to his purpose. Weber, limited far more to historical particulars, could not achieve this same objective.

The "theory" of action which permits these new analyses is a perspective on, or characterization of, behavior and social organization rather than an explanation of them. It is not *just* another conceptualization, but represents instead a successful outcome after generations of debate and of failure. Linking organism, mind, and organized social life, it allows a description of each in terms of its place as an act whether by an individual or by many actors. The gains for our work arise when this elementary characterization is employed to generate an increasingly comprehensive and coherent conceptualization of the phenomena we study—to provide well-founded and systematically interdefined definitions and classifications. Such conceptualization is not to be despised nor is it easily achieved. It is an indispensable step toward systematic explanation, systematically grounded. As we all know, the theory of action is itself not Parsons' contribution. His contribution is its exploitation in the manner just described, and with each new, plausible extension that he makes there arises a fresh sense of the scheme's value for our purposes.

And sometimes this is enough. The clean distinction in Parsons' eleventh essay between mental illness and spiritual malaise enables a discussion of the roles proper to psychotherapists and clergymen that should benefit both. As we read it, we too can take pride in the power and suggestiveness of our basic characterization of man and social interaction when properly elaborated. This single essay incorporates a vast body of knowledge, generalized far beyond the case in hand but detailed enough to afford guidance for policies concerning that case. One must add that Parsons has usually done his homework. A strength of this and other essays lies in its consistency with the major empirical literature and in shrewd assessments of the meaning of that literature.

But classification sometimes fails. It fails when, as for the problems of relating "mind" and "body" treated in his essay on psychosomatic relationships in health and illness, we need causal connections, not conceptualization. It fails when, as in his study of the father symbol and in many of the essays already described, Parsons moves from conceptualizing action or historical particulars in their status as action to tell us what those particulars now are at given times or places. Is it true, for example, that the most basic American values have not changed for 350 years? Should we see Riesman's typology (pp. 184–85) as deriving from Weber? (I suggest Park and the urban ecologists as a more likely source.) Does the evidence cited support a conclusion (p. 133) that "the main process of . . . selection . . . that occurs during elementary school takes place on a single main axis of *achievement*"? Are rebellious adolescents fundamentally committed to the basics of our social order? Not semantic or lexical coherence but a detailed examination of the fit between our explanations and our data is required to decide such issues. Parsons almost always has something of interest to say about the problems he tackles. We need, however, to discipline our speculations by determining which are "true," not merely which are interesting.

GUY E. SWANSON

University of Michigan

Madame de Staël on Politics, Literature and National Character. Translated and edited by MORROE BERGER. Garden City, N.Y.: Doubleday & Co., 1964. Pp. 371. $5.50.

Morroe Berger has done intellectual life a handsome service by selecting from Mme de Staël's writings certain sections of contem-

porary interest, then translating these scrupulously, and in addition providing an Introduction of some 90 pages concerning the author, the influences that shaped her thought, and the impact that she has had. The result is not quite comparable to the volumes in the Viking Portable Library, such as *The Portable D. H. Lawrence* or *The Portable Conrad,* for the emphasis remains on Mme de Staël's work and not on her letters or love life. But for sociologists and other academicians this should become, as it has for the reviewer, a reminder of the tradition of French social and literary criticism, alive to this day, of which Mme de Staël has been one of the prototypes.

In ways scarcely fashionable now she sought to relate literature and the arts to the social setting in which they developed and to do this on a comparative basis, comparing after the fashion of her times the Greeks and the Romans, but also penetrating French ethnocentrism by generous discussion of German literature, philosophy, and social thought; in fact, as Berger points out, she helped interest Americans in German academic developments and thus encouraged early nineteenth-century scholars such as George Ticknor to attend German universities rather than Scottish or English ones. She also insisted on moralizing literature in a way that has been pretty well demolished by the New Criticism, contending that what is eloquent and elevated will also be true and conducive to private and public virtue. I do not hold these preoccupations against her; still, her reviews of the stages of literary history from the ancients to her own time seem to me to have something of a schoolmarmish quality, overgeneralizing from small items and jumping readily from a book to the spirit of a nation or an age. She makes such statements as, "It seems to me that by studying history we become convinced that all the most significant events lead to the same end, the civilizing of the world" (p. 180). Or: "We may ask why the English, who are contented with their government and customs, have an imagination so much more melancholy than was that of the French. The answer is that liberty and virtue, those two great results of human reason, require meditation, and meditation necessarily leads to serious pursuits" (p. 204). She does not transcend the eighteenth-century view that Shakespeare was commonly in "vulgar taste" (p. 198).

However, when her materials are her own impressions based on her travels during the time when Napoleon exiled her from France, her comparisons of cultures and customs are often remarkably vivid and penetrating. As already indicated, she responded to German social and intellectual life with an uncommon sympathy. She was not uncritical. For example, her discussion of "The Spirit of Conversation" in chapter xi of the section on Germany compares the ponderousness of the Germans unfavorably with the grace and flirtatiousness of French society; thus:

> The talent for telling a story, one of the great charms of conversation, is very rare in Germany. The audience is too tolerant and not quickly enough bored; so the *raconteurs,* confident of their listeners' patience, are too relaxed in telling their stories. In France every speaker is a usurper conscious of being surrounded by jealous rivals and trying to keep his ground through success; in Germany he is a legitimate proprietor who can peacefully exercise his acknowledged rights [p. 289].

Mme de Staël did not assume that national character is immutable or grounded in climate or race. Like Tocqueville later, she insisted on social explanations of those aspects of the human condition that men could change, particularly their political institutions. In her liberal optimism, as Berger points out, she shared the spirit of the French Enlightenment. She believed in progress, as one of the quotations given above indicates, and she anticipated progress in what we today would call the social sciences, both as the result of greater attention to political arithmetic and the laws of probability (after the fashion of Condorcet) and through a refinement of moral sensibility. Though she deprecated terror and fanaticism, she never turned against the French Revolution, nor was she ever persuaded that Napoleon, through his despotism, was restoring law and order; her sharp criticisms of Napoleon, not vindictive but shrewd, are among the highlights of the volume. I agree with the editor that the belief in progress needs defenders today when the news constantly pours in as to mankind's cruelties and stupidities and the disasters resulting from both good and bad faith. Indeed, to read how Mme de Staël could travel in Russia, befriended everywhere, even while Napoleon

was ravaging that country, or to read the courteous correspondence between her and Napoleon's chief of police, is to make one wish for an age when nationalism was still somewhat moderated by aristocracy, and when Frederick the Great (to Mme de Staël's disapproval) could surround himself by writers and artists who spoke only French and shared his contempt for the Prussians he dominated.

At the same time, Mme de Staël's life and work are themselves a source of progress. Though herself passionately French (and hence in her typology, which stressed French rationality, not unequivocally French), she maintained a vision of a cosmopolitan world in which people trained in languages could converse with spirit about serious ideas of relevance to the common good. She not only ran a salon, as other vigorous women had done, but made her own vigorous contribution not confined within traditional feminine areas. Daughter of a mother who wrote in a more formal fashion, she was very much the child of her father, Jacques Necker; an only child, she took upon herself her father's political and intellectual concerns. In speaking of her as "a great intellectual and emotional liberator" (p. 51), Berger seems just. One comment she makes about the education of the poorer strata shows how much she still has to say for our time:

"There is something revolting about the necessary in every domain when it is meted out by those who enjoy the superfluous. It is not enough to be concerned about the lower classes only in regard to their useful employment; they must also share in the pleasures of the imagination and the heart" (p. 302).

DAVID RIESMAN

Harvard University

Abundance for What? and Other Essays. By DAVID RIESMAN. Garden City, N.Y.: Doubleday & Co., 1964. Pp. xiv+610. $6.50.

Some years ago Sir Isaiah Berlin classified writers and thinkers in terms of a distinction made by a Greek poet: "The fox knows many things, but the hedgehog knows one big thing." Berlin called hedgehogs those who "relate everything to a single central vision, one system less or more coherent or articulate . . . —a single, universal, organizing principle." In contrast, foxes are "those who pursue many ends, often unrelated and even contradictory, connected, if at all, only in some *de facto* way, for some psychological or physiological cause." They "entertain ideas that are centrifugal rather than centripetal, their thought is scattered or diffused, moving on many levels, seizing upon the essence of a vast variety of experiences." If Talcott Parsons is American sociology's foremost hedgehog, David Riesman is surely among our leading foxes.

This collection of essays, written in the last ten years and heretofore scattered in a variety of publications, reveals again Riesman's catholic interests. He can comment with brilliance and even occasionally with profundity on the Cold War and on leisure in the New Suburbs, on Veblen and Tocqueville or on the sociology of law. He seems equally at ease when discussing the problems of suburban homemakers, contemporary college students, or Turkish peasants. He comments perceptively on abundance for what and on abundance for whom, on the American national character and on the sociology of the interview. What holds this book together is not its themes but rather the author's scintillating expository skills and his acuteness of observation and commentary.

To be sure, Riesman's informal sociology, his self-conscious eschewing of rigorous method and sustained inquiry, has its drawbacks. The reader, while often left with a series of striking or at least evocative observations and comments, also often feels unable to recall at the end of a paper what it all was intended to deal with at the start. The illumination provided in a paragraph or a few sentences often tends to get lost in the effort later to recall what the whole essay was meant to convey. An almost studied disorderliness of method frequently distracts from cumulative impact. But then, one shouldn't expect from foxes that they exhibit the peculiar virtues of hedgehogs.

The single, most notable change in Riesman's orientation between his earlier work and this volume is his new political involvement. In his earlier writing he seemed only peripherally interested in political questions, feeling, perhaps, that the "real issues" concerned metapolitical transformations such as

the changing American character structure, or shifts in life styles and interpersonal relations. Riesman has now become much more concerned witth the political realm, in fact, he is among the relatively few American sociologists—the species is much more frequently found in Europe—who might be called *sociologues engagés*. An earlier optimism about the future of America has now given place to an anxious preoccupation with the nightmares of the Cold War and the permanent war economy. Riesman continues to be concerned with Park Forest and Crestwood Heights, but the Pentagon has also become a major focus of his attention. Some may deplore this straying from the narrow path of sociological detachment; I prefer to see in it a welcome widening of vision and of sympathies.

LEWIS A. COSER

Brandeis University

Strangers Next Door: Ethnic Relations in American Communities. By ROBIN M. WILLIAMS, JR., in collaboration with JOHN P. DEAN and EDWARD A. SUCHMAN. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1964. Pp. xiv+434. $12.00.

Rarely does a book combine such good theory, comprehensive scholarship, meaningful empirical data, and practical suggestions for social policy. Robin Williams has produced one of the best monographs in several years.

Yet the book is likely to be partly misunderstood and somewhat ignored. It is generally thought that Williams is a functionalist; nothing could be further from the truth. His theoretical orientation is traditional sociological structuralism, with a sprinkling of ideas from modern social psychology. Unlike so many of our contemporaries, he makes reference not only to his personal friends but to most of the pertinent literature.

The researcher makes use of a variety of methods: participant observation, direct observation, informal experiment, analysis of documents, surveys of representative samples with preformulated questionnaires. Williams and his collaborators seek to find out what is, and the social and psychological forces behind social reality; not to adduce evidence for a particular point of view. There is no slighting of value premises, and no fear of making policy suggestions. While there is no single research "design," the research is solid, intelligent, and well informed.

Starting with an intensive survey of one community, Elmira, New York, the study expanded to include twenty communities (six of which were given special attention), and 248 cities were superficially surveyed by mail. While the concentration is on attitudes toward Negroes, Jews, Mexican-Americans, and Italo-Americans, the authors sought to test a variety of sociological and social-psychological hypotheses. The amount and variety of data, collected over a period of eight years (1948–56), are almost bewildering.

Only a few of the many sociologically pertinent findings can be summarized here. (1) Prejudice varies with region (most in South, least in West), education (least among educated), class (upper more anti-Jewish, lower more anti-Negro), church attendance (most among occasional church attenders), age and sex (elderly and women most prejudiced). (2) Most prejudice is a response to interaction with in-group members, and seldom becomes operative in behavior. "The rigid, hostility-ridden bigots form only a small proportion of the population" (p. 110), but perhaps half the population is "prejudice-prone." (3) "The greater the frequency of interaction with members of another social category who are of approximately equal status in respects other than membership in this category (education, occupation, etc.), the less the tendency to accept derogatory stereotypes, to feel sentiments of social distance, or to favor public discrimination" (p. 191). Thus, the broader the range of contacts, the less prejudice.

(4) Because the Negro is so aware of white prejudice, he does not attempt more interracial contact than is absolutely necessary, and considers his closest white contact to be an extreme exception. He develops a reciprocal prejudice, including stereotypes of the white man which, however, are more accurate than are white stereotypes of the Negro. The social categories of Negroes with the most prejudice against whites are practically the same as the social categories of whites with strong prejudice against Negroes, except that upper-status Negroes feel more social distance than do lower-status Negroes (among whites it is the other way). (5) Opportunities for Negro-

white interaction are most frequent at work, least in organizations and informal social life. Joint Negro-white social activity is associated with low social distance prejudice; this is true even among Negroes who express generalized suspicion of white persons. Despite the forces breaking down prejudice, *within* each group there is expression of outgroup prejudice, which "provides a legitimized mode for the management of otherwise disruptive or uncomfortable intragroup aggressions, supplies a common universe of discourse, reinforces a sense of belonging, and serves as a set of credentials of membership" (p. 363).

Despite the high quality of this book, it does have a few defects. First, some facts and interpretations are out of date: for example, it is found that a large majority of Negroes believe they should not go into public places where they are not wanted (p. 249). The data were collected mainly in the period 1948–52, and like many another sociological study they have become dated by 1964. The availability of too many research funds apparently caused unnecessary extensions of the research; this may be a factor in the delayed publication, although the premature death of John Dean was undoubtedly an important cause of delay. Second, after specifically identifying the communities studied, Williams adopts the annoying device of referring to them by fictitious names. The use of fictitious names for towns and cities makes it difficult for the reader to recollect what kind of area the author is talking about, and yet the anonymity of these towns is lost for those who have some reason to look them up.

So comprehensive is this book that there will be little need for another like it for some time. The field of intergroup relations ought now to seek out new subject matter.

ARNOLD M. ROSE

University of Minnesota

Community, Character and Civilization: Studies in Social Behaviorism. By DON MARTINDALE. New York: Free Press of Glencoe, 1963. Pp. 467. $7.50.

This work consists of a collection of twelve essays, written during the period from 1947 to 1962, and deliberately left unrevised in this book. They are considered by the author to express a central point of view, "social behaviorism," which is thought to stand in sharp opposition to a less satisfactory approach called "functionalism." The book has its own specific conceptions of these postulated schools of thought. Social behaviorism is traced from Tarde and Le Bon to Cooley, G. H. Mead, and W. I. Thomas. Macrofunctionalism allegedly was founded by Parsons and Merton under the influence of British anthropology and of Durkheim and Pareto. Microfunctionalism is said to come from Kurt Lewin.

It is asserted that functionalism has subscribed to two basic assumptions: "(1) The causal priority of the whole, and (2) the functional interdependence of parts within the social whole" (p. 22). Many practitioners of functional analysis no doubt will be surprised to find that marcrofunctionalism is characterized repeatedly as conceiving of social systems as "organismic unities"—an allegation manifestly not applicable to Pareto, or, for that matter, to Merton. This work repeatedly imputes to functionalists views that some of the most prominent American functionalists have explicitly criticized and rejected (cf. pp. 25–26).

The essays are loosely aggregated into six parts: an Introduction on social behaviorism; theoretical and methodological perspectives; institutions and communities; national character and social structure; sociology of culture (with an essay on Max Weber's sociology of music); and a final section, "Social Disorganization: The Conflict of Normative and Empirical Approaches." The great variety of particular topics treated precludes a brief summary of content. At one extreme is the breezy, "A Postscript for Those Who Hate Essays Which End Negatively or Inconclusively," which concludes the essay on "The Status of American Goals and Values." At another pole of a different dimension is the exceedingly brief and highly negative essay, "Talcott Parsons' Theoretical Metamorphosis from Social Behaviorism to Macrofunctionalism."

In his eagerness to do battle with "functionalism," the author sometimes seems to misunderstand the positions he attacks. Thus, Martindale also asserts that functionalism *assumes* that "(1) the organic-type system is the primary fact in socio-cultural events, which can be analyzed only from the stand-

point of the whole; (2) any item in the socio-cultural system is in functionally interdependent relations within the whole; it is in reciprocal determination with the whole" (p. 69). This is a position Merton has rejected and has criticized at considerable length. But then Martindale also maintains that social behaviorists are philosophical nominalists, who hold as basic tenets that "(1) the only things that really ever exist are social acts; (2) all social group structures and the like are merely as if realities, fictions or conveniences of speech but not new entities with causal powers of their own apart from the causal aspects of the individual acts that make them up" (p. 70).

These characterizations of two extreme theoretical positions seem enough to give most practicing research sociologists nightmares, particularly since the author informs them that social behaviorism and functionalism are the two major patterns of theory in contemporary sociology. A difficult choice, indeed, if these are our only options.

The author is at his best when setting forth his own insights and reasoning; he is less successful in his summaries and interpretations of the ideas of others, where he sometimes falls into oversimplication. For instance, in his "translation" of the Parsonian pattern-variables (p. 52), Martindale apparently does not see that these concepts refer to moral *norms* at the level of collectively integration, not to the full concrete motivations or interests of actors. At the same time when Martindale applies his erudition and critical incisiveness to substantive problems, he produces solid and thought-provoking contributions. Whether this is social behaviorism or just good sociology does not seem to this reviewer to be of crucial importance.

ROBIN M. WILLIAMS, JR.
Cornell University

The Urban Villagers. By HERBERT J. GANS. New York: Free Press of Glencoe, 1963. Pp. xvi+367. $7.00.

Drawing upon eight months of participant observation in Boston's now renewed West End, Gans presents an analysis of second-generation Italian Americans. He had close contacts with twenty individuals and eventually got to know more superficially around 150. He lived in the area, participated in informal activities, attended meetings of organizations, and even sat a number of evenings as a somewhat silent observer in one of the neighborhood bars.

Gans finds that second-generation Italians in this predominantly working-class neighborhood are primarily working class rather than Italian, despite some remaining distinctively Italian characteristics (e.g., food habits, the cult of the Madonna, and some knowledge of Italian dialects). These distinctive working-class characteristics are partly structural in character (e.g., the organizations of the neighborhood into peer groups based largely on an expanded notion of kinship) and are partly characterological, arising out of the nature of socialization in a subculture in which children are treated very much like adults and in which the individual is scarcely ever alone (e.g., fear of losing self-control, inability to empathize, etc.). A rather lengthy ethnography is given of this interesting subtribe, touching upon the organizations of the family, relations among generations, relations to community institutions, etc.

Gans's volume also has a message: When applied to an area which is not seriously deteriorated physically and which is occupied by a population with a functioning social structure that is satisfying to the members, urban renewal can work hardships. Planners and urban renewal officials applying their middle-class and non-sociological perspectives in the evaluation of a neighborhod can misjudge an area like the West End as a slum when it is in fact a satisfying (to its inhabitants) locale inhabited by a subculture which has its rights to survive.

Gans's analysis of the social structure of the West End is insightful, often ingenious, and, although somewhat sentimental, ranks with the best of its genre. Because of the high quality of Gans's sociological sensibility, I find it difficult to make a clear-cut evaluation of this volume. On the one hand, his description of a second-generation Italian-American community often rings very true, much of what is written applying very well to my memories of my childhood and adolescence in New York City during the twenties and thirties. On the other hand, this work can

only be considered the start of an analysis of either working-class life or Italian-Americans. His subjects, after all, were a small handful, the remainders of what was once a very flourishing community whose descendants are dispersed all over Greater Boston with those remaining in the West End in the late fifties possibly quite different from the "Doc Nortons" who now live in Medford and Somerville. They are hardly representative of the American working class, and his attempts to bolster his analysis of West Enders as essentially working class through the citation of other literature equally defective in coverage does not lend much weight to this argument. There is much more characteristically Italian (or perhaps more generally Latin) in the social organization of the West End than Gans would have the reader believe.

Despite his ready admissions to the limitations of his study, Gans's frankness does not gainsay the fact that only comparative studies of representative working-class communities of varying ethnic origins can settle the question whether this descriptive account and insightful analysis apply to the working class in general or only to specific types of working-class groups of particular ethnic origins. Certainly the catapulting of working-class Jews into the middle classes and higher during the same period when the Italians made only very tiny hops suggests that the conditions of working-class life are not always a handicap, or at least handicap ethnic groups quite differentially.

I confess also a certain amount of impatience with the sentimentality lying behind Gans's criticism of urban renewal in this particular case. In the past fifty years, the West End has turned over several ethnic groups and in the past twenty years it has lost most of its population. In the face of these long-term trends, it is hard to maintain that the West End as it was before urban renewal was a highly desired neighborhood. Granted that many of its residents in the late fifties did not want to move; I suspect that another decade would have seen their departure one by one. Although the provision of low-cost housing for persons of limited means is a worthwhile social goal, the preservation of the West End for this purpose at a point in time when it was in fact being rejected on the housing market would have been in error. One can sympathize with the trauma of broken peer-group ties, but someone has to mind the metropolitan store.

PETER H. ROSSI

National Opinion Research Center
University of Chicago

Clinic Nursing: Explorations in Role Innovation. Edited by HERMAN TURK and THELMA INGLES. Philadelphia: F. A. Davis Co., 1963. Pp. 192. $5.00.

This volume consists of a series of papers, several published previously, which report the results of a three-year program of practice and exploratory research in the Outpatient Clinic of Duke University Medical Center. Medical sociologists will find some of the papers resourceful. The data, although modest, are frequently suggestive and are treated imaginatively by some of the authors. Followers of Durkheim perhaps will pause at the use of a fifty-seven-item check list of clinic activities to test his propositions, but Turk and McKinney's conclusion that prior consensus on specialization between role partners is directly associated with cohesion exemplifies their efforts to cast the research in the framework of theory.

Thirty student-physician–student-nurse clinic teams were randomly paired and studied for three-week periods. The objective was to introduce an expanded nursing role concerned with comprehensive patient care. Generally, student nurses expected a larger scope of authority than student physicians were willing to grant them. However, convergence increased with rates of interaction (the measure based on an analysis of student nurses' diaries). But the fact that the nurses expected more autonomy after the experiment, while their partners would grant them less, is a demonstration of the institutional context in which these changes were occurring; both types of students were subject to the respective influences of clinic nursing instructors and staff residents who were reinforcing their own professional interests. Oddly enough, role consensus was less likely to develop when both types of students shared ideologies of comprehensive patient care than when student physicians held a "bureaucratic" orientation toward patients (measured with a rules-and-procedures scale).

Written from several perspectives for diverse audiences and in a sequence which rep-

resents the chronology in which the ideas developed, the volume suffers from redundance and lack of coherence. Attitude questionnaires and scales have familiar shortcomings. Nor is it entirely clear that students, who are without a clear role in the clinic, provide an adequate test of the capacity of nurses to compete for the authority of student physicians. But the problem of role innovation is a significant one; exploratory studies in organizational change like this one are welcome.

The project establishes useful guidelines for future research. Turk's series of discussions on the dynamics of jurisdictional boundary formation illustrate a fruitful approach to the study of professionalization. The study as a whole demonstrates that role innovation involves more than socializing role incumbents: Future studies of the dynamics of role changes will be obliged to recognize the institutional constraints and the influence of personnel in other parts of the system.

Turk concludes that it is surprising that so little has been written about the relations between physicians and nurses. One can only concur.

RONALD G. CORWIN
Ohio State University

Expectant Peoples: Nationalism and Development. Edited by K. H. SILVERT. Preface by KENNETH W. THOMPSON. (Prepared by the American Universities Field Staff.) New York: Random House, 1963. Pp. xxi+489. $6.95.

This is a rather unusual study of the developing countries in that it is not merely another collection of regional reports but an attempt to analyze a certain body of data within a definite theoretical framework. The editor's aim is "to view the processes of total development through the lens of the formation of the nation-state" and to evaluate the significance of nationalism as one of the factors in this process.

Most definitions of nationalism, observes Silvert, implicitly contain tones of approval or disapproval and are but partial statements of the total subject. Nationalism is frequently treated as if it were only an ideology or a state of mind, even though such a notion is meaningful only if it is extended into the realm of action, that is, if it is treated functionally. From a functional point of view, one could reasonably argue that nationalism is intrinsic to the development process, that it discharges a strategic function in a particular kind and phase of change, and that it acts as a necessary mechanism of social cohesion and integration.

This view of nationalism requires its empirical definition in terms of dominant ideology and social values. If nationalism is defined as "the acceptance of the state as the impersonal and ultimate arbiter of human affairs," its varying levels of development in individual countries could be meaningfully compared.

Such a comparison is attempted, with a considerable degree of success, in the twelve regional studies which constitute the bulk of the book. These studies are divided into several categories according to the specific obstacles to economic and sociopolitical modernization which are faced by particular developing countries.

Thus three of the studies are devoted to countries struggling to overcome tribal-regional or social class division (Afghanistan, Saudi Arabia, Bolivia); two deal with cultural attitudes hampering national integration (the Arab world, Brazil); two are concerned with caste and racial cleavages (India, South Africa); and the remaining five discuss various problems of the elite (Argentina, Israel, Japan, Indonesia, the Philippines).

Although the arrangement of these case studies into categories is not always convincing and although some of them (e.g., Indonesia or the Philippines) look more like "field reports" than genuine interpretative essays, the book as a whole maintains a high level of analysis and represents a significant contribution to the study of contemporary nationalism and of the problems of development and modernization.

In his brief concluding chapter, the editor observes that the book, "true to its theme as well as its authors, was conceived and written with passion as well as discipline." This reviewer concurs in this judgment.

KONSTANTIN SYMMONS
MacMurray College

The Nature of Cultural Things. By MARVIN HARRIS. New York: Random House, 1964. Pp. viii+189. $1.95 (paper).

The Nature of Cultural Things is the third volume in the series, "Studies in Anthropology." The author emphatically rejects the traditional anthropological view that culture is essentially a symbolic system. He argues that anthropology will never become a behavioral science if it continues to search for patterns of meaning. In his opinion, values, beliefs, and norms cannot be the subject matter of a discipline that seeks to formulate laws that are valid cross-culturally. In fact, he believes that anthropology is doomed to remain relatively speculative as long as it attempts to determine an actor's goals or ends. He asserts that an individual's intentions and the meanings that he presumably attaches to his acts are by their very nature impossible to verify objectively. Even if a willing informant reveals his motives to the ethnographer, Harris maintains that the observer has no way of knowing whether they are the true or only motives involved in the act. And even if this is disregarded, the observer cannot objectively determine whether they are related to an item of overt behavior. In essence, Harris is saying that while we can objectively describe what people do, we can never be certain about what they feel and think and how this might affect their behavior. Consequently, he thinks that approaches based upon the actor's orientation to the situation rest upon "Platonic essences and tautologies." He proposes to rescue anthropology from this metaphysical quandary by defining cultural units operationally. That, in his terms, makes them both "intersubjective" and "culture free."

In order to define cultural units operationally, Harris states that we must conceive of them as empirical entities. He locates these entities in the stream of external non-verbal human behavior. The author rejects the familiar distinction between natural or concrete and analytic units. He claims that the reality of a given unit of analysis derives from a classificatory scheme that leads to the discovery of empirical regularities. Thus he feels that it is methodologically unsound for anthropologists to take their categories either from common-sense experience or from the statements of their informants. While natural languages are adequate guides in dealing with the exigencies of everyday life, they cannot provide categories that are at once "culture free," logically coherent, and operationally valid. It is for this reason that the author feels constrained to create a data language for anthropology.

Harris develops a complex classification of cultural units; each level in this hierarchy corresponds to a more abstract conception of the empirical data. The "actone" is the basic unit; it is "a behavioral bit consisting of body motion and environmental effect which rise above the threshold of the observer's auditory and visual sense" (p. 37). The concept of an actone relates an actor's physical movements to the transpositions of other physical objects (including his own body and other actors) within the context of his immediate natural and social environment. The entire notion of an actonic approach to the study of human behavior is based upon the assumption that the primary processes of human adaptation to the natural environment can occur on a purely non-verbal level. For Harris, insofar as human activity sustains life it is not only grounded in but is determined by the ways in which men must move objects in order ultimately to derive energy from the environment. Consequently, he asserts that each link that an observer posits between human behavior and environmental effect must be subject to the test of physical and logical necessity; the movements induced in objects must be produced "in conformity with known macrophysical laws" (p. 40). Each of the higher levels of cultural analysis is built out of actones connected in this manner. For example, complex behavioral events are described in terms of chains of episodes: An episode is an actone which is distinguished by certain types of actors and objects and is anchored to recurrent times and places. Social interaction is defined by series of episodes that are functional (logical and physical) but not necessarily causal requisites for the behavior of more than one actor.

If the author's basic premises are accepted, his classification of cultural units is remarkably cogent. But it is questionable that we can legitimately treat the entire ideational component of culture as superstructure. In his final chapter, he makes the explicit assumption that "to insist upon the importance of an actonic approach, and the separation of verbal from non-verbal behavior is to insist that thinking, wishing, yearning, believing, and

praying cannot alter the environment" (p. 170). He further claims that if there is a determinate relationship between what the actor experiences as meaningful and the necessity of adapting to the physical world, the former is always determined by the latter. If all Harris means by this statement is that individual cognitive or valuative acts do not immediately shape the natural environment, then he is right. But, of course, this is only a truism. I think he has gone much further. He has denied that thought and feeling have crucial evolutionary significance. This overlooks the fact that technological systems, which are certainly responsible for radical transformations in the environment, are largely the result of the interaction between human needs and goals and certain modes of conceptualizing possibilities inherent in the physical object world. The material world must be made meaningful before man is able to shape it.

GARY SCHWARTZ

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Education and Culture—Anthropological Approaches. Edited by GEORGE D. SPINDLER. New York: Holt, Rinehart & Winston, 1963. Pp. xx+571. $4.75.

This volume brings together twenty-five papers, the bulk of which have been published elsewhere in recent years, on a theme that ranges from an explication of the philosophical assumptions underlying both anthropology and education to a case study of a single teacher. Almost all the authors are anthropologists; and about a third of the contents is contributed by the editor, who also edited *Anthropology and Education* (1955).

The reader is well organized, with "previews" by the editor to introduce each of three parts: "The Articulation of Anthropology and Education," "Education in American Culture," and "Education Viewed Cross-Culturally." But a compilation which attempts to cover a youthful field of inquiry is bound to result in a certain unevenness in quality; the prospects which are opened up in the first section and in the previews are not consistently fulfilled by the ensuing papers. The section on "Education in American Culture" is the weakest in this respect. This disharmony between prospects and accomplishments makes one feel that the volume is premature. Yet it leaves no doubt that the potential applicability of anthropology to education is great, particularly in broadening our understanding of socialization through cross-cultural studies.

At the present time, however, the anthropology of education seems to be in no better shape than the sociology of education. Convincing evidence for the authors' conclusions is rare; systematic conceptualization is lacking, with the chief exception of the editor's commentary; and familiarity with the complex legal, economic, and political structure of American education is virtually absent. Quite obviously, the volume was intended to be an introductory text, and it might be valuable in reducing some cultural biases in teachers. But the shortcomings of scholarship probably stem from the newness of the field, rather than from any intention of presenting a watered-down version of the anthropology of education to college students.

There is a less forgivable shortcoming, however, which concerns the uncritical application to industrial society of theories derived from the study of more homogeneous cultures. We are repeatedly reminded that there are dominant cultural themes which directly shape the personality. The assumption has two serious consequences: first, it means that the investigator does not need a representative sample of personalities in order to infer cultural themes; and second, when confronted with the multiple themes of modern society, there is the unavoidable implication that personalities in our society must lack integration. For example, one author concludes from findings about the "character-ideals" of *students of education* as follows:

> The materials just cited . . . indicate rather clearly the fabric of the value pattern that seems to be emerging as the dominant core of the social character values in American culture (providing one can assume, as I am here, that the middle class culture is the core of our way of life [p. 135].

And since many students expressed *both* "traditional" and "emergent" values, the author considers them "confused." In another paper, the same author has recourse to "core

values" to explain the apparent stability which characterizes the complex role-set of school administrators (p. 238). In short, almost no attempt is made to understand how diverse values are allocated to diverse social relationships and settings.

With respect to the methods of research applied to American education, we are compelled to conclude that the low reliability of the anecdotal materials is not compensated for by ingenuity in conceptualization or in spotting research problems.

SAM D. SIEBER

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Columbia University

The Employed Mother in America. By F. IVAN NYE and LOIS WLADIS HOFFMAN. Chicago: Rand McNally & Co., 1963. Pp. x+406.

In the flood of books appearing in the past decade that deal with the changing status of women, remarkably few have been based upon empirical research or have been marked by serious scholarship. Women have been variously described as the lost sex, the second sex, the superior sex, as a newly discovered minority group, and as a group given to a mystique rather than to the reality of modern life. All of these descriptive phrases have done more to reveal the particular axe the author is grinding than to increase our understanding of social processes.

The present book is a welcome exception. In its focus upon the movement of mothers into paid employment, it deals with the phenomenon that epitomizes the transformation occurring in women's roles; but its point of view is strictly the researcher's, not the polemicist's.

The opening chapter presents some of the background facts: for example, that almost 40 per cent of all mothers of children aged 6 to 17, and 20 per cent of those with preschool children are now in the labor force; and that, concomitant with the upgrading of occupations, with changes in values, and other related factors, this movement increasingly involves middle-class families. The book includes some 22 well-selected empirical studies on the effects of maternal employment upon children, upon husband-wife relationships, and upon the adjustment of the working mother herself. With only a few exceptions the studies reflect a generally high level of research sophistication and compare favorably with other contributions in sociology in which more rigorous designs and sharpened empirical methods are appearing. A large variety of psychological and social variables and a considerable range of research methods are illustrated, from matched-pair designs to analyses of national survey data, and from mailed questionnaires to projective test responses to observations of child behavior in school settings. On the whole the studies dealing with effects upon children are more sophisticated than those in the other two categories: an indirect reflection, perhaps, of the fact that in the field of family research more investigators have given attention to child development than to adult development.

On balance there is little evidence in these studies that maternal employment produces adverse effects, a finding which, given the present research basis, is of no little significance on the present social scene. If anything, the evidence is on the side of employment rather than non-employment of mothers. This generalization by the reviewer is, however, subject to the same criticism that may be levelled against these studies and one that the authors themselves expound: namely, that maternal employment is not itself a variable which produces meaningful findings. Hoffman suggests, for instance, that the pendulum has swung from the point of view current in the 1940's, that the effects of maternal employment on the child are all bad, to the view that maternal employment has no effects at all; but that in truth the effects may be good, bad, or incapable of evaluation, depending upon a host of other factors—mother's reasons for working, whether she works part-time or full-time, age of child, sex of child, socioeconomic status, attitudes of husband, to name only a few of the factors already shown to influence the relationships.

The conclusion that maternal employment is not in itself a meaningful variable might have been foreseen from some of the basic facts stated at the outset. If some 35 per cent of all women aged fourteen and over are in the labor force, for instance, then it is to be expected that maternal employment

will be associated with many different configurations of social variables, and its effects —if "effects" is a proper term at all—will vary accordingly.

In terms of research design, these studies represent attempts to isolate the relationship between maternal employment as the independent variable and one after another dependent variables when one or more controls are introduced. These designs are bound to produce a set of findings difficult to integrate. Given the impetus of the present book, and as suggested in the concluding chapter by Nye, the next set of researches in this field might well make better use of theory in the selection and interpretation of variables. Over and above Nye's suggestion, furthermore, that contingent conditions and test variables will need to be better defined, future studies might well make use of multivariate statistical techniques in attempting to elucidate the complexity of the relationships involved.

These last comments are not intended, however, to detract from the value of the present book, which makes an important contribution not only in terms of its substantive findings but, more important, in making clear where and how the next investigations should be made.

BERNICE L. NEUGARTEN

Committee on Human Development
University of Chicago

A Long Journey: The Autobiography of Pitirim A. Sorokin. By PITIRIM A. SOROKIN. New Haven, Conn.: College and University Press, 1960. Pp. 327. $5.00.

To write an autobiography is an arrogant act, or at least one marked by uncommon self-confidence. And we all dream of writing one. Few men in our profession have so good a claim on our personal attention as Pitirim Sorokin. He was a young Russian revolutionary who had to flee the Bolshevik terror. He was a value-free empiricist who became an ideologue for an integrated, Christian society. He helped introduce American sociologists to European scholarship and became a pre-eminent American scholar.

I was rather reluctant to accept the task of reviewing Sorokin's autobiography, for he was my mentor and remains my friend. The latter situation is neither unique nor uniform for Sorokin has a way of alienating students (and others) as well as attracting them. He mixes conceit with humility, harshness with solicitude. His judgments are not always just, and his criticisms occasionally reflect personal bias and personal disapproval rather than scholarly objectivity. He lashes out and lays on the loving hand.

But here he has mellowed, or at least attended to life experiences more than to scholarly disputes. If this is an *apologia pro vita mia*, it is stated more in personal than professional terms, and he has kept his weapons of scholarly harassment concealed. Virtually two-thirds of the book is devoted to the time prior to his migration to the United States, when he was a very young man. His career as a guest lecturer at various institutions and then as a professor at Minnesota and Harvard is recorded in pages that state his part in the contemporary discipline of sociology without quite catching the polemical flavor of his intersection with the American sociological scene.

This is an intensely personal document, without being quite personal in the ordinary sense. Sorokin is concerned with his "philosophy" and his scholarship, but not with his attachment to flowers, friends, and family. I used the term "intersection" advisedly, because I am not sure that Sorokin ever had the kind of total professional commitment that makes us most comfortable with colleagues. He brought to sociology both intellect and integrity, but the latter was occasionally subject to dispute because he was guided by other and, on balance, more cherished orientations.

I do not mean this to sound like an obituary, but an autobiography encourages contemplative nostalgia. Meanwhile, Sorokin flourishes and scolds, thinks and feels deeply. His personal history rewards reading.

WILBERT E. MOORE

Princeton University

Survey Research. By CHARLES H. BACKSTROM and GERALD D. HURSH. Evanston, Ill.: Northwestern University Press, 1963. Pp. xix+192. $2.95 (paper).

This delightful "cookbook" might easily have been entitled "The Joy of Surveying" for the thoroughness of its content and the enthusiasm with which it is written. It is the second

of a series of research handbooks designed for use by undergraduates in connection with substantive courses in political science and political sociology. No pretension is made, however, that this work will serve these students as a substitute for thorough training in the theory and methods of survey research if they are planning a career as either producers or consumers of such research.

The authors make use of a model survey of political attitudes in order to illustrate systematically the techniques of the large-scale sample survey. In the first chapter they consider the formulation of the problem, the selection of procedures to be employed, planning for the materials and personnel needed and the financing of the study. Even a model grant request is included. The second chapter is devoted to the design of a random cluster sample of housing units and a sample of potential voters within these units. Some attention is given to alternative designs before selecting this technique (as a compromise between theoretical rigor and practical limitations) but the authors are insistent on noting that the treatment has been somewhat superficial. They caution the reader to "take it upon yourself to challenge knowledge too easily acquired, which glosses over important technical and theoretical entanglements" (p. 32). The third and fourth chapters are concerned with framing questions for use in the field and planning the format of the questionnaire. The fifth chapter reveals to best advantage the authors' ability to make explicit much of the art of carrying out a successful field survey. In this chapter they consider the details of pretesting, the training of interviewers in general and briefing them about the study at hand, the techniques of getting data from respondents, controlling the data-gathering process and verifying the accuracy of the data once it has been gathered. The last chapter deals at some length with coding and processing the data for analysis but touches only briefly on the interpretation of the data, this subject being reserved for a future volume in this series.

There is one difficulty that may arise from following the hints and helps provided by the authors when the investigator tries to have his data processed on anything but a unit-record machine. Following an honored but obsolete custom, the authors repeatedly advise compressing all relevant data onto a single card to facilitate cross-tabulations. To achieve this they suggest many situations in which single-card columns may be punched with arbitrary combinations of numbers. Unfortunately, such cards are difficult to "unmultiple punch" and are useless for processing in high-speed computers. Since the authors recognize that many researchers must rely on outside data-processing sources, it is unfortunate that they make this suggestion; there are many occasions when it is far cheaper to employ a computer for a few minutes than to use a counter-sorter for several hours.

Despite the focus of this manual on the study of local political processes, it will undoubtedly be of considerable use to any instructor trying to execute a field study as a pedagogic device for his students. In addition, this handbook provides a fine collection of checklists of things to think about and to do when conducting a survey that will be useful even to the most advanced scholar.

DAVID NASATIR

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Universidad de Buenos Aires

Sociological Research: Volume I, *A Case Approach;* Volume II, *Exercises and Manual.* By MATHILDA WHITE RILEY. New York: Harcourt, Brace & World, 1963. Pp. xx+777; ix+195. $9.50.

When a man gives his wife a cookbook, he hopes that the family cuisine will be bettered and, perhaps, a whole new line of dishes added. In an age when learning by apprenticeship is difficult and unfashionable, the printed page must serve for the transmittal of the arts—culinary and sociological—and the test of its counsel is whether the cook learns more than the printed recipe. Does the work enlarge the vision and encourage the imagination, so that faced with the issue of the persons to be fed, their several tastes, the money in the purse, and the produce in the refrigerator and on display in the store, the cook produces a resolution most pleasing?

In his Introduction, Robert K. Merton assures the reader that this is no dull and plodding textbook but that he will experience "a distinct sense of intellectual excitement in working his way through it." The indorsement is also an identification of the type of cuisine

for which the reader will find instruction, although this restriction is more evident in the range of skills presented in the *Manual* than in the cases excerpted for analysis in the first volume.

Of the five sections of the *Manual*, the first is devoted to observing and recording social interaction; the second to coding and machine tabulation; the third to mathematical types of measures (from measures of central tendency to elementary Guttman scaling); the fourth to measures of association among several variables; and the fifth to sampling and elementary principles of probability. Clearly, the emphasis is toward methodological sophistication of a narrow sociological variety, rather than the development of disciplined skills as an individual field worker or acquaintance with the full range of techniques used in the social sciences. The student does have early assignments for simple observing and unstructured interviewing, but these assignments are short and the interviewing is to be conducted among fellow collegians (as would have to be the case given his lack of training), so there is little of the excitement of intimate interaction with unusual peoples and situations. Rather than the emotion Merton says he felt, I experienced the soggy realization that the student was being prepared for the role of research director of a project studying middle-class America from the vantage point of the bureaucratic desk.

True, Volume I does contain excerpts from a fine diversity of sociological researches, including Durkheim, Weber, LePlay, Thomas and Znaniecki, Sorokin, Stouffer, Bogue, Merton, Lazarsfeld, and many others. Moreover, the first excerpts, being from Malinowski and Whyte, do remind the student of what an individual researcher could achieve with limited resources and abundant curiosity, but the reader is quickly cautioned as to the limitations of their techniques and their researches are converted into period pieces, rather than upheld as exemplars of techniques that have been successfully and brilliantly used on the contemporary scene by such persons as Erving Goffman, Meyer Fortes, Howard S. Becker, and Claude Lévi-Strauss. Likewise, the excerpt from *The Protestant Ethic* is followed by a discussion, not of historical methods and the perception of cultural pattern or ethos, but of ideal types and multidimensional measures. Such omission might be tolerable if the student received fair warning of the limitations of the research techniques being favored and, most important, of the "management bias" that tends to cumulate about the insulated position of research director.

The issue is not ethnographic or historical methods versus laboratory procedures and statistical refinements, but the basic fact that no research method is free of bias and limitation. Social research is characterized by a kind of Heisenberg principle, so that the more refined the techniques, the less they deal with the true focus of interest—social interaction. Archetypical in its elegance is survey sample polling, which reduces society to its constituent atoms and then elaborately rediscovers such elementary social facts as the political influence exerted by people on each other.

Via its excerpts, this pair of books does make a commendable effort to show the variety of studies fitting under the rubric of "sociological research," but like some fancy cookbooks, the more appealing of the illustrations are *cordon bleu*, while the recipes and comment are Howard Johnson.

MURRAY WAX

Emory University

The Community in America. By ROLAND L. WARREN. Chicago: Rand McNally & Co., 1963. Pp. 343.

This is by all odds one of the best books written on the community in the last decade. Its merit lies in exposition of the general framework for the description of community phenomena rather than in its application to an existing body of empirical research.

Warren defines a community as a "combination of social units and systems which perform the major social functions having locality relevance" (p. 9). By definition then, a community is comprised of social units and systems. The problem posed by Warren is what is the nature of the relationship among these units that distinguishes them as community. His answer lies in the *organization* of five major functions on a *locality* basis—production, distribution, and consumption; socialization; social control; social participation; and mutual support. The basic formulation owes much to a seminal paper by E. T. Hiller ("The Community as a Social Group," *American Sociological Review*, VI [April, 1941], 189–202).

The author incorporates into his treatment of communities the thesis that communities in modern societies are becoming more implicated in extracommunity systems than in local ones with a corresponding decline in community cohesion and autonomy. Unlike many writers on the community he pays more than lip service to the thesis. This leads him into the favorite sport of sociologists—neology. He distinguishes between a community's "vertical pattern" and its "horizontal pattern" of integration rather than to external and internal patterns. Vertical patterns are structural and functional relations of community units and systems to extracommunity ones, and horizontal ones (you guessed it) are their relationships to one another.

Though it is clear to this reviewer that the distinction has no theoretical advantage, the fact that the author pays particular attention to the problem of the community as a unit of analysis by treating its relationship to other types of system units sets his treatment apart from most texts on the community. Unfortunately the author does not take advantage of a rather substantial and growing body of empirical literature that can be brought to bear on this formulation. Although he has an easy "out" for this by suggesting that the treatment can easily become diffuse if one treats a large number of social units or systems, the chapter on the community's "vertical pattern" cites only a few empirical studies. There are several substantial bodies of empirical investigation about the relationship of communities to other social systems that he might have used by way of illustration—for example, the literature on school reorganization and integration. Though he acknowledges the early work of J. H. Kolb, he does not employ his more recent *Emerging Rural Communities* which deals with extracommunity relationships.

To this reviewer, a main disappointment lies in the author's disregard of a body of literature that attempts to deal with the relationships of communities to one another in a system framework. There are for him only two types of community relationships: relationships among units within communities and relationships of communities to other types of units. Should the community units also be related to one another even though the author defines this problem away? There is a vast body of empirical literature on metropolitan regional relationships germane to this problem, for example, that of Isard, the Duncans, Lieberson, Scott and Winsborough, Hawley, and Schnire.

Despite this failure, anyone teaching a course on the community would be well advised to consider this book seriously as a text. Lest it be dismissed solely as a text, I hasten to add that no one working on theoretical or empirical problems about communities should fail to read it.

ALBERT J. REISS, JR.

University of Michigan

Four Cities: A Study in Comparative Policy Making. By OLIVER P. WILLIAMS and CHARLES R. ADRIAN. Philadelphia: University of Pennsylvania Press, 1963. Pp. 334. $6.00.

Prospective readers of this book should not be dissuaded by its unattractive jacket and unglamorous title; this is a first-rate contribution to the fields of community politics and local government. There is much to interest both traditional students of local government and social scientists more concerned with community power structures and decision-making.

The study deals with four middle-sized (50,000–70,000) core cities. All have a council-manager form of government, non-partisan local elections, and, while not blessed with the same economic well-being, all rest on a manufacturing base. Unfortunately, the authors felt the need to disguise the true locales of their observations. Apparently, however, the cities are Kalamazoo ("Alpha"), Jackson ("Beta"), Muskegon ("Gamma"), and Bay City ("Delta")—all in Michigan.

Using a common research approach, the authors seek to characterize the modes of policy-making in each city and among the cities. This leads them to a consideration of such topics as the mechanisms for elective leadership recruitment, the effects of these mechanisms on policy-making, voting in local and non-local contests, and voting in referendums. It also leads them to rather detailed examinations of the place in policy-making of downtown businessmen, civic organizations, labor groups, neighborhood interests, and city administrators. A wide variety of issue-areas is discussed for each community.

While such concerns are not unique prima-

rily because they are cross-community in nature, the same cannot be said for the authors' attention to what might be called the "life-styles" of the four polities. Four basic types are posited: (1) promoting economic growth ("boosterism"); (2) providing life's amenities (going beyond bare necessities); (3) maintaining traditional services ("caretaker" government); and (4) arbitrating among conflicting interests (the "delegate" role). The relationship of these styles to political, social, and economic institutions in the four communities is skilfully delineated.

A few caveats may be raised, despite the generally fine scholarship. Some sociologists, for example, may feel that these two political scientists have devoted excessive attention to the formal institutions and officials of local government. Other readers, including this reviewer, might prefer more elaboration on the research techniques and problems involved. Then, too, there are occasional operational procedures which are puzzling. For example, in dealing with voting patterns in referendum elections the authors use a party allegiance index (derived from voting results in partisan elections) as a surrogate for a socioeconomic index (p. 107). Finally, the exclusion of school politics is regrettable, inasmuch as this is one of the most salient arenas of community politics.

These reservations are not meant to detract from the book's signal contribution. As the authors acknowledge, generalizing on the basis of a four-community study is indeed premature. Nevertheless, they have taken us one step further in evolving some theories about comparative community politics.

M. Kent Jennings

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Urban Life and Form. Edited by Werner Z. Hirsch. New York: Holt, Rinehart & Winston, 1963. Pp. 248.

This book contains papers read at a faculty seminar on Foundations of Urban Life and Form (1961–1962) sponsored by the Institute for Urban and Regional Studies at Washington University in St. Louis, Missouri. The contributors represent a wide variety of disciplines, including law, city planning, history, sociology, philosophy, and political science. Each author wrote on the subject of urban form from his particular perspective. Urban life and form were generally given a spatial referent, with some exceptions. None of the papers reports new research. A few represent attempts to summarize some fairly extensive body of literature. For the most part, however, the articles are essays presenting the authors' ideas on one or another aspect of urban life.

In my opinion the papers are generally well conceived and worthwhile. Students and professionals alike will find it rewarding to read and ponder over the ideas in these papers. Personally, I found three articles to be particularly interesting: "A New Vision in Law: The City as an Artifact," by William L. Weismantel; "The City in History—Some American Perspectives," by Richard C. Wade; and "Urban Form: The Case of the Metropolitan Community," by Leo F. Schnore. The last-mentioned article contains a sound bibliography on the work of midwestern sociologists on urban and interurban form.

The basic problem with a book of this type is that it spans so many disciplines. There probably are very few scholars who would find most or all of the papers to be of interest. Furthermore, in some of the articles it is difficult to discern just what topic is under consideration, and a few are largely negative in tone. I can generally recommend this book to all urban scholars, however. Each should find something of value to him within its pages. If this book is any indication, the conference from which it came must have been a success.

Theodore R. Anderson

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Beyond the Ruling Class. By Suzanne Keller. New York: Random House, 1963. Pp. 354. $5.25.

Suzanne Keller has attempted to clarify the characteristics of a specific set of elites, namely those that have a general and sustained impact on industrial societies, particularly the United States. These "strategic elites" include not only political, economic, and military leaders, but those from moral, cultural, and scientific fields as well. Drawn from a range of classes, these elites constitute a core group responsible for the material and moral well-being of others within the national community.

Together, they stand as caretakers whose numbers can be defined, functions specified, origins traced, and destinies predicted.

In order to describe the various facets of this core group, the author presents a taxonomy and illustrates it with numerous examples, the most illuminating being studies of elite groups found in the contemporary United States. Unfortunately, much of this material is the appendixes. Certainly the importance of these findings would warrant placement in the main body of the book. For example, Miss Keller indicates that the strategic elites of the United States are selected, to a high degree, from white-collar—particularly business and professional—families of northern European and Protestant background. She hastens to add trend information purporting to describe a broadening of the recruitment base. With few exceptions, however, the documentation fails to support her qualifications.

The appendix contains weaknesses but nonetheless offers meaty material. By contrast, much of the remainder of the book stands as a tribute to irrelevance. For example, the author's assumption that strategic elites are responsible for the well-being of others contributes nothing to our understanding of their behavior, since she does not demonstrate how this notion might lead to testable hypotheses. In short, the body of the book fails to connect theory and data but abounds with statements on how the author feels about America today. Presumably, for example, "men are equal in status and in rights before the law in political life and with respect to a basic minimum of economic welfare and security. . . . Increasingly, individuals have equality of opportunity to achieve unequal statuses" (p. 271), and so forth.

Perhaps less elaboration of values and more investigation of a scientific nature would have strengthened the book. Some sections certainly suffer from lack of depth. By way of illustration, Miss Keller's treatment of the general evolution of elites and classes fails to refer to the writings of Childe, Fried, Gumplowicz, Landtman, Pearson, Polanyi, Sahlins, Service, or White, to mention some obvious omissions. Perhaps these oversights explain her inability to deal systematically with the problem of how property relations might be related to the emergence of core groups and classes. Rather than take up this puzzle, she simplifies the question by stressing the pristine derivation of both from the division of labor, the growth of which presumably rested upon "food surplus" and/or "climate."

Sociology should discourage thin treatments of evolution and should stress the importance of developing a scientific theory of elites. This strategy has prompted several sociologists, Janowitz among others, to test not only their own ideas but also those of writers such as Lasswell. Indeed, a refinement of the latter's formulations on the garrison state might constitute a good starting point for contemporary elite analysis. Remarkable in this regard is Miss Keller's reluctance to deal with Lasswell's classic essay. Although she reviews some of his works, notes their importance, and comments elsewhere on Eisenhower's speech on the garrison state, she writes as if Lasswell's article does not exist, for she fails to refer to it in the text, footnotes, or bibliography. In so doing, she avoids controversy but invites criticism.

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Symbolic Crusade: Status Politics and the American Temperance Movement. By Joseph R. Gusfield. Urbana: University of Illinois Press, 1963. Pp. viii+198. $5.00.

In this tightly reasoned work covering the historic sweep of the American temperance movement, Professor Gusfield has made a major contribution to the sociological study of social movements. Here his earlier work on the Woman's Christian Temperance Union is broadened to include the total movement, not just one organizational manifestation of it.

The central theme of the book is summed up in the title: to Gusfield the temperance movement represents an example par excellence of a movement oriented to the preservation and reaffirmation of the prestige and way of life of status groupings. The author affirms the importance of status as opposed to class or economic conflicts in American history and he argues that the issues of status movements have a strong symbolic element; symbols are important for the definition of prestige, respect, and deference. As an example of status politics the temperance movement was not concerned with instrumental self-oriented goals; instead, it was concerned with getting

others—individual sinners or culturally alien groups—to accept the values of the temperate and abstainers. When in its early period the normative definitions of the Protestant middle and upper classes dominated the society, the strategy of the movement was that of assimilative reform—education, persuasion, and changing the social conditions that create the need to drink. However, as normative consensus declined, as urbanization, capitalism and non-English immigration rose, tactics changed from assimilation to "coercive reform." The fight for prohibition represented the attempt of the "old middle class" from rural areas and smaller communities to enforce their norms, symbolically if not fully in actual behavior, on the rest of the population. Finally, after prohibition was repealed and the norms of the society shifted clearly away from abstention and support of the old middle classes, the movement turned to some extent to political conservatism, right-wing alienation, and a general critique of the sins of modern society—especially of the sins of the upper classes and the so-called decent people.

Over the almost two-hundred-year sweep discussed by Gusfield the temperance movement has at times drastically shifted its social base. Based at an early point on the aristocratic Federalists of the East Coast and hostile to Jacksonianism, it later was linked to Midwestern populism. During the 1850's it is linked to the Abolition movement and then in the late nineteenth century to the Progressives and Mugwumps. Finally, some parts of the temperance movement ended up linked to contemporary right-wing extremism. In each case the author nicely summarizes the cultural context and coalitions which surround the movement.

As an interpretation of the temperance movement, the book is an unqualified success. But when Gusfield turns to general statements about the nature of status politics and the role of symbolic battles and victories he is on less firm ground. Although he admits that a given social movement at a particular point in time may have both class and status elements, he still wants to argue that status politics have a fundamentally different character from class politics—groups fighting to maintain their prestige are more likely to be satisfied with and to seek symbolic victories. Both the temperance movement *and* the civil rights movement are seen as examples of status-oriented social movements fundamentally concerned with symbolic and token battles. Temperance settled for an ineffective prohibition civil rights for *de jure* desegregation. However, because de facto desegregation in the North was not the first target of the civil rights movement does not mean that other, more instrumental targets will not be sought later, as events have shown. Similarly, the groups behind prohibition could not have known in advance how much evasion there would be and, anyhow, Gusfield quotes Jellineck to the effect that during prohibition alcohol consumption was cut in half. Prohibition was not all purely symbolic. On the other side, Gusfield ignores the fact that many economic or class-based conflicts transform themselves into status politics. When ideologies develop to justify differential claims on the distribution of rewards, prestige and the normative order are not long ignored. While there are differences between status politics and class politics the differences are not as sharp as Gusfield maintains. Another limitation of this book is a sin of omission: because of the focus on status politics the fascinating interplay of organizations is described with only little theoretic attention to the coalition processes, growth rates, and other organizational phenomena of the movement.

These criticisms aside, in an area of study that has been characterized by either glib statements about the social bases of movement or fixation on phenomena of goal displacement and the iron law of oligarchy, this study represents a signal contribution.

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Change in the Contemporary South. Edited by ALLAN P. SINDLER. Durham, N.C.: Duke University Press, 1963. Pp. x+247. $7.50.

Assuming that the main motivation in arranging the conference at which these papers were delivered was to assess the prospects for a two-party system in the South, the papers may be divided into two groups: those by Thomas D. Clark, Joseph J. Spengler, Edgar T. Thompson, and James P. Frank deal with the "background" of political changes; those by Donald R. Matthews and James W. Prothro, Robert J. Steamer, Donald S. Strong and Philip E. Converse with the changes in politi-

cal tendencies. The editor, in an epilogue, gives a lucid summary of the papers and adds his own comment on the main problem. He rightly states that the Negroes' right and power to vote will not solve the problem of integration; we may add: because the Negro, with few exceptions, will remain "visible," a fact which distinguishes his situation from that of other "ethnic groups." We also agree with Sindler's skepticism concerning the penetration of Republicanism to the state and local levels (p. 237), because the economic interests of the classes which are leading in "presidential Republicanism" are well served by most of the Democratic state and local administrations.

Clark's paper on cultural change represents conventional, non-sociological historiography and leaves important questions unanswered. For example, the emergence of southern writers of world-wide recognition cannot be explained if one fails to differentiate between them and those of mere regional significance. The latter kind is to be found in every region of the United States. We are inclined to believe that the "southern renaissance" had some connections with social change and the resulting challenges to the traditional value system of the region. Spengler's discussion of demographic and economic change is a masterpiece of theoretical analysis and should be of great value to sociologists, in spite of its rather abstract character. It clearly shows the factors which impede economic development and discusses the often neglected differentiation between "active" and "passive" sources of economic growth.

Frank reviews the legislation and court decisions in race relations and adds some skeptical but realistic comments on their effectiveness and on problems still to be solved.

Thompson's fine paper on the "second emancipation" of the Negro from the race idea (which has served as a substitute for the institution of slavery) views race relations realistically as a status problem. While he draws parallels to the European immigrants' rise in status, his appraisal of the Negro's prospects remains skeptical.

Matthews and Prothro present a statistical analysis of Negro voter registration. Their conclusions are cautiously stated. But there are some strange hypotheses (esp. pp. 133 ff.) which might have been eliminated by a bit of a preliminary ecological approach. Instead, the authors rely entirely on correlations, most of which are extremely weak. The interpretation fails to give social stratification and class antagonism adequate consideration as affecting Negro registration. The two papers by Steamer and Strong deal with the two aspects of the central theme of the series: disaffection with the national Democratic party and the prospects of "durable" Republicanism in the South. Both rely on election returns; both use the ecological approach to voting analysis; and both pay attention to the intensity of campaigning. Steamer, after a general discussion of his topic, intensifies his analysis by drawing on the research that has been done on Louisiana, where in spite of bifactional tradition the Republican party is growing roots primarily among the more recent urban managerial and business classes and their adherents.

Strong's paper pursues the analysis of Republican growth through the ecology of traditional and recent Republicanism in the region, giving, so to speak, a mirror view of the preceding paper. He emphasizes the increasing weight of economic interests in the Republican vote now that the national Democratic party no longer protects white supremacy. But he too, like Steamer, sees little prospect for a rapid growth of a full-fledged Republican party machine in the South.

Converse seems to come to practically the same conclusion. His paper is based on very small numbers of interviews all over the South drawn from nationwide survey samples. It shows all the weaknesses resulting from this technique. Consequently, most of his discussion is really more or less speculative.

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The Louisiana Elections of 1960. By WILLIAM C. HAVARD, RUDOLF HEBERLE, and PERRY H. HOWARD. Baton Rouge: Louisiana State University Press, 1963. Pp. 126. $3.00.

Often in southern politics, economic cleavages are severely blunted by a lack of cohesive political factions and the interjection of emotional racial issues. However, in Louisiana for nearly three decades this was not the case. There a relatively cohesive bifactional political system existed in which the Long machine was able to organize the voters along economic

lines (hill farmers versus planters, labor versus capital, etc.). This monograph is a study of the breakdown of this system in the 1959 gubernatorial and 1960 presidential elections, a study executed through a combination of political ecology, correlation analysis, and journalistic description of the structure of Louisiana's political elites.

The core of the analysis is that the ecological patterns of voting in the state changed from a have–have not cleavage to a Protestant-Catholic, rural-urban, Anglo-Saxon culture–French-culture cleavage under the impact of both long-run trends and such immediate factors as the dissolution of Long's machine during his mental breakdown and the rise of the race issue. Around this core is woven a discussion of the political history of Louisiana in relation to its social base, a prognosis for its political future, and so on.

There are two features of this study that should be noted: (1) While the factors which account for the elections are complex (rural versus urban, religious, racial, etc.), the methods used are unable to sort them out clearly, and in particular to assign relative weights to them. Part of the difficulty is inherent in ecological data. In the rural areas, for instance, was it Davis' folksy style that gave him such predominance over the urban liberal Morrison, or was it segregationist sentiment? Only interview data can give final answers to questions such as these. But, in addition, a good part of the difficulty is that maps were used rather than such techniques as multiple regression which help partial out the effects of different factors and assign a quantitative value to them. (2) The study stays largely at the level of a description of Louisiana state politics. It does not address itself *in a general way* to what are implicitly general questions raised by the events in the state: questions such as the exact conditions under which racial and religious antagonisms override economic ones and which elements are most susceptible to these diversions, or what are the relative effects of changes in the political superstructure as opposed to changes in the ecological base, etc.

To state these limitations, however, is not to suggest that the monograph is without merit, but only to suggest exactly what can be expected from it. Although the factors causing the change in Louisiana politics are not fully sorted out, still a constellation of factors is delineated and persuasive empirical evidence marshalled for their importance. This should prove valuable to sociologists and historians alike. And if this monograph does not deal in a direct manner with general questions, it does deal with them implicitly; and there is nothing to prevent a sociologist from using the wealth of empirical data presented to build, in conjunction with similar case studies, propositions transcending Louisiana state politics.

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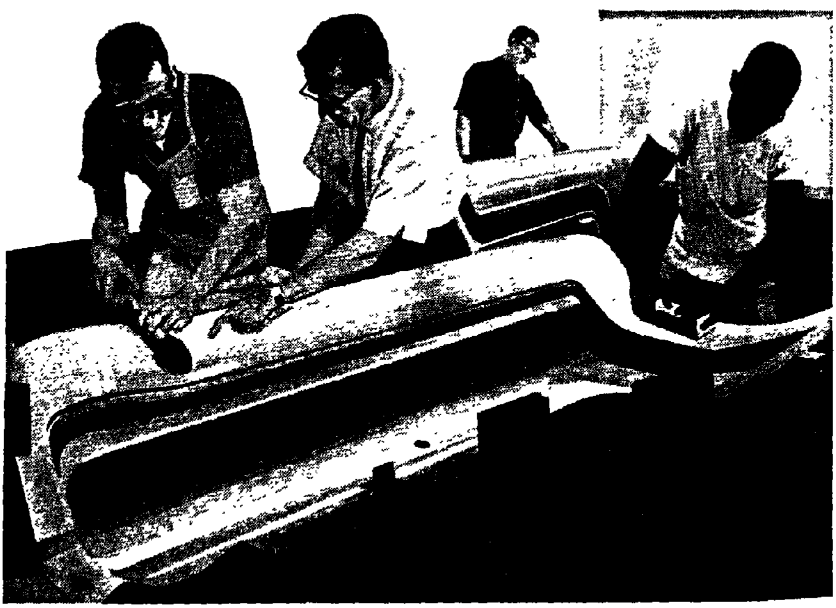
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The Implications of Pentecostal Religion for Intellectualism, Politics, and Race Relations[1]

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ABSTRACT

The teachings of Pentecostal evangelist A. A. Allen are examined with reference to the relationship between Pentecostal religious thought and three areas of secular life. We find that the strict scripturalism and belief in miraculous healing sharply delimit the area of experience for which science is regarded as relevant. The eschatological teachings encourage withdrawal from political activities rather than the espousal of right-wing political views often imputed to fundamentalist Christian groups. Literal interpretation of the Bible and emphasis on spiritual criteria of personal worth result in a strong opposition, on strictly religious grounds, to racial segregation. The movement is analyzed in terms of the history of Pentecostal groups and inferences about the social composition of Allen's adherents. We conclude that the movement is primarily a religious solution to problems of illness, poverty, and status deprivation. These problems are conceived of in individualistic terms and solved in personalistic religious faith. Religious groups such as this ought not to be confused with quasi-political groups that mix general Christian beliefs with anti-Communism or segregation.

Work in the sociology of religion, in an attempt to sustain the Weberian tradition, frequently focuses on the consequences of religious beliefs for various aspects of secular life. One of the most persistent generalizations about American Christianity in this vein is that religious conservatism causes social and political conservatism. Unfortunately this hypothesis has not been supported by a direct examination of the implications of religious beliefs. Instead the relationship has been inferred from observed associations between certain kinds of church membership and attendance patterns and political indexes such as party preference and voting.[2] Sometimes the conservatism hypothesis is expressed in terms of a stereotype in which fundamentalist religion is linked to a thoroughgoing anti-intellectualism, a penchant for extreme right-wing politics, and strong segregationist sympathies.[3]

This paper examines the teachings of

[1] I am especially grateful to Professor Raymond J. Murphy for invaluable advice and encouragement. This paper is based on a paper prepared for a colloquium conducted for National Institute of General Medical Sciences trainees. I wish to acknowledge my indebtedness to Professor Charles R. Wright, director of the N.I.G.M.S. program at UCLA, and to the Institute. I also wish to thank Amelia Fitts for a critical reading of an earlier version.

[2] Benton Johnson has demonstrated the association between fundamentalist affiliation and Republican identification and voting ("Ascetic Protestantism and Political Preference," *Public Opinion Quarterly*, XXVI [Spring, 1962], 35–46; and "Ascetic Protestantism and Political Preference in the Deep South," *American Journal of Sociology*, LXIX [January, 1964], 359–66).

[3] E.g., see David Danzig, "The Radical Right and the Rise of the Fundamentalist Minority," *Commentary*, April, 1962, pp. 291–99; Daniel Bell (ed.), *The Radical Right* (Garden City, N.Y.: Doubleday & Co., 1963).

a fundamentalist religious leader, Pentecostal revivalist A. A. Allen. The beliefs articulated in Allen's sermons, booklets, and tracts are analyzed in an attempt to ascertain the relationship between Pentecostal religious thought and the complex of intellectual, political, and racial conservatism.

We shall follow, in broad outline, the theoretical approach of the classical students of the sociology of religion: Engels, Weber, Troeltsch, Niebuhr, and Pope.[4] This approach suggests three steps. First, the location of the movement in historical and structural context—what is the history of the movement? Where do its members fit in the social structure of the United States? Second, an analysis of the manifestation of historical antecedents and structural factors in the religious teachings of the movement—what are the basic beliefs of the movement? What is the content of its eschatology and its theodicy? What are its moral teachings? Third, what is the relationship between the religious group as established in the study of its history, place in society, and teachings and the special problems with which we are concerned? In this case, what are the implications for anti-intellectualism, rightist politics, and race relations? This paper draws upon an array of recorded and printed data from the Allen movement in an attempt to answer these questions.[5]

HISTORICAL AND STRUCTURAL BACKGROUND

The Pentecostal churches and the Allen movement (which is essentially Pentecostal in outlook) are the latest heirs to a long line of popular evangelical movements which have marked American religious history. Both American history and the history of European Christianity have been characterized by popular religious movements in opposition to the established churches.[6] The established church is generally characterized by a well-developed liturgy, a trained ministry, and, most important of all, an intimate relationship to the secular centers of society. The adherents to the established churches are drawn from the strata which dominate the economic, politcial, and cultural life of the society. The religious opposition is scriptural and evangelical, rather than ritualistic, in theological emphasis. Its ministry is closer in background to the "common people." Its adherents are members either of a newly ascending group or of a weak and oppressed group. Richard Hofstadter, in *Anti-Intellectualism in American Life*, traces the specific pattern of conflict between the liturgical and evangelical forces in American church history. He finds the traditional churches with elaborate creeds and formal liturgy far weaker in America than in Europe. He concludes that

> in America religious culture has been largely shaped by the evangelical spirit, for here the balance of power between evangelicalism and formal religion was long ago overwhelmingly tipped in the direction of the former.[7]

[4] The relevant works are Friedrich Engels, *The Peasant War in Germany* (New York: International Publishers, 1926), esp. pp. 50–92; Hans Gerth and C. Wright Mills (eds.), *From Max Weber* (New York: Oxford University Press, 1958), pp. 267–301 and 323–62; Ernst Troeltsch, *The Social Teachings of the Christian Churches* (2 vols.; New York: Harper Torchbooks, 1960), esp. pp. 328–49; H. Richard Niebuhr, *The Social Sources of Denominationalism* (New York: Meridian Press, 1960); Liston Pope, *Millhands and Preachers* (New Haven, Conn.: Yale University Press, 1942), esp. pp. 49–140.

[5] Use was made of seventeen tape-recorded radio programs, four booklets, fifty-four tracts, and five issues of *Miracle Magazine*. Further, the generalizations made in this paper are consistent with limited observations of Allen revivals held in Los Angeles in May, 1964. A full appendix of sources of data on the Allen movement is available on request.

[6] American background for this study is derived largely from Richard Hofstadter, *Anti-Intellectualism in American Life* (New York: Alfred A. Knopf, Inc., 1963), esp. pp. 55–117. Troeltsch, *op. cit.*, provides the basis for the European historical outline.

[7] Hofstadter, *op. cit.*, p. 47.

Evangelical Christianity repudiated elaborate theological scholarship in favor of emotional religious experience. It greatly reduced the standards of scholarship thought relevant to the ministry. These factors, combined with the pragmatism of frontiersmen, farmers, and businessmen, which distrusted theory and learning as foolish, effeminate, or subversive, contributed heavily to the history of American anti-intellectualism. In the political realm, evangelical religion has generally played a conservative role.[8]

The Pentecostal churches, in addition to being part of the evangelical stream, have a history of their own. It is a history of evangelical Christianity adapting to the social circumstances of the most deprived groups in American society. The revivalism of the nineteenth century succeeded in enlisting into the ranks of the Baptist and Methodist denominations a large part of the huge unchurched population of both the middle and the lower class. The Holiness movement is a case of evangelism within evangelism. It is essentially the development of a group of distinctively lower-class sects.[9] Pentecostal churches in their original form represent the absolute minimum in formalism and ritual and the absolute maximum in emotionalism and free expression in American religious history. They also exchange for the middle-class conservatism of the established evangelical groups an intense otherworldly orientation which dismisses politics as on a par with bobbed hair and movies—far too much a thing of this world.

The great theological issue in American religious history is fundamentalism versus modernism.[10] Although many of the established evangelical groups have maintained a formal commitment to fundamentalism, the beliefs of both congregants and clergy have probably weakened as a result of exposure to scientific influences in education.[11] However, the extremely low educational level of both the clergy and the members of the Pentecostal groups has probably resulted in actual beliefs as rigidly fundamentalist as official doctrine.

The other great source of religious conflict has been a running controversy on moral standards which roughly parallels the fundamentalist-modernist debate in theology. The main issues have been alcohol, sex, and tobacco, in about that order. In the case of each of these sensual pleasures, the Holiness sects have proven more ascetic than their Baptist and Methodist parent groups.

Hofstadter summarizes many of these issues in his characterization of the evangelical tradition in the twentieth century as embattled in a "revolt against modernity." The conflict between a traditional, fundamentalist, rural way of life and the forces of urbanism and secularism was especially severe for the groups attracted to the Holiness churches.[12]

THE ALLEN MOVEMENT

The Allen Revival Movement is a flourishing evangelical organization with home base at Miracle Valley, in Arizona, near the Mexican border. The principal activities of the organization are frequent large revivals, a nightly radio broadcast heard on more than fifty stations, and circulation of A. A. Allen's numerous writings. The movement is conventionally Pentecostal in doctrine with unusually heavy emphasis on faith healing. Since Allen is doctrinally a fairly orthodox Pentecostal leader, it is hoped that the study of his movement will be of considerable relevance to an understanding of the approximately two

[8] *Ibid.*, p. 111.

[9] See Pope, *op. cit.*, pp. 126–40, for history of Pentecostal sects in Gastonia.

[10] Herbert W. Schneider, *Religion in 20th Century America* (Cambridge, Mass.: Harvard University Press, 1952), pp. 131–33.

[11] Hofstadter, *op. cit.*, p. 117.

[12] John B. Holt, "Holiness Religion: Cultural Shock and Social Reorganization," *American Sociological Review*, V (October, 1940), 741.

million adherents to various Pentecostal and Holiness groups in the United States.[13]

STRUCTURAL CONTEXT

A number of sources of data make it possible to infer, with reasonable confidence, the social characteristics of Allen's followers. More than half of the stations over which Allen's programs are heard are in the South and Southwest. Recent revivals have been held in Houston, Dallas, New Orleans, Detroit, and Chicago—all either southern, southwestern, or cities with large populations of recent migrants from the South, Southwest, and rural Midwest. Pictures of revival congregations and home towns and the speech patterns of persons heard on the radio broadcasts confirm our imputation of regional background and further indicate that many followers are from rural and small-town backgrounds and that a sizable portion is Negro. The majority of participants are women. The participants' appearance, grammar, and allusions to financial problems indicate the lower-class character of the movement.

All available evidence indicates that the movement draws its adherents from the same segments of the population from which the Holiness groups have traditionally drawn their members.[14] The groups most peripheral to social and economic success—lower class, poorly educated, Negro, "white trash," Puerto Rican—are well represented in the Allen movement.

RELIGIOUS TEACHINGS[15]

A direct and detailed examination of the major religious teachings of the Allen movement will provide a sound basis from which implications for secular life can be drawn.

We have divided the beliefs into three categories: basic theological and moral principles, religious participation, and the fruits of religious faith.

BASIC THEOLOGICAL AND MORAL PRINCIPLES

The basic theological and moral principles deal with Scripture, salvation, demonology, ascetic morality, and eschatology.

1. An examination of any of the Allen publications impresses one immediately with the central role the Bible plays as the principle of legitimacy. The Bible is interpreted in accord with strict fundamentalism—every word is taken to be literally true. Every aspect of the movement is justified with at least one and often a whole array of scriptural quotations.

2. Although Pentecostals along with other fundamentalist groups expect converts to be "saved" or "born again" and then to be baptized in water, Pentecostals emphasize the distinct and additional experience of baptism with the Holy Ghost.

3. An extremely literal belief is held in devils and demons. One of Allen's records is advertised as follows:

> Recorded in an actual service where A. A. Allen is actually casting out demons and these demons are talking and saying, "I will not come out; you cannot cast me out!" etc. . . . Proves demons are real today!

The belief in demons is central to the Manichaean theology which views the

[13] Rough total for groups generally regarded as Pentecostal from "National Church Membership Statistics," *1962 Yearbook of American Churches,* reprinted in Leo Rosten (ed.), *Religions in America* (New York: Simon & Schuster, 1963), pp. 227–32. See Elmer T. Clark, *The Small Sects in America* (New York: Abingdon Press, 1949), pp. 85–129, and Morton T. Kelsey, *Tongue Speaking* (Garden City, N.Y.: Doubleday, 1964), pp. 69–95, for a discussion of the beliefs of American Pentecostal groups.

[14] For assessments of the social composition of Pentecostal groups see Benton Johnson, "Do Holiness Sects Socialize in Dominant Values?" *Social Forces,* XXXIX (May, 1961), 311; Holt, *op. cit.,* pp. 741, 746; Pope, *op. cit.,* p. 136; St. Clair Drake and Horace R. Cayton, *Black Metropolis* (New York: Harper Torchbooks, 1962), II, 633, 636.

[15] This section is based on materials listed in an appendix available from the author. Quotations and summary statements not otherwise noted are drawn from these materials.

world as a struggle between Good and Evil. Allen uses the literal belief in demons to provide a causal explanation for a wide variety of evils—especially physical sickness and "the curse of madness."

4. Allen advocates an ascetic morality and sharply criticizes America for sinfulness. He emphasizes alcohol, sinful (extramarital) sex, narcotics, tobacco, pornographic books and entertainment.

In the apocalyptic booklet, *My Vision of the Destruction of America,* Allen devotes many pages to a documented catalogue of American sinfulness, citing statistics for tobacco and liquor consumption as well as other worldly pleasures such as baseball, bingo, table tennis, and movie attendance. He concludes,

> We are living in a pleasure-crazed age. Sensuality has become the dominating passion of America. She is so intoxicated with the quest of pleasure that moral and spiritual consequences are seldom considered.

5. Directly related to Allen's moral indictment of America is an apocalyptic eschatology which warns that the destruction of America, a just retribution for sin, is at hand. Allen had a vision of the destruction in which the instrument of disaster was nerve gas. *My Vision of the Destruction of America* contains a vivid description of the vision, proof that it is inevitable.

> It is my firm conviction, based upon the authority of God's word, that any nation which forgets God to the extent that America has forgotten Him—any nation that rebels and refuses to walk in obedience to God's word as America has refused and rebelled shall be destroyed, and that without remedy.

RELIGIOUS PARTICIPATION

Direct, overt, and emotional forms of religious experience are taught as the correct form of participation in religious life. Speaking in tongues is particulary encouraged.

1. Speaking in tongues is accompanied by baptism of the Holy Ghost.

2. The overt emotional behavior that earned Pentecostals the label "Holy Rollers" is not wrong or incidental but prescribed by the Bible. Allen cites scriptural support for praise, noise, clapping of hands, upraised hands, dancing, leaping, laughter, weeping, praying together, falling prostrate or into a trance, and speaking in tongues.

FRUITS OF RELIGIOUS FAITH

In addition to otherworldly salvation, there are wordly rewards for religious faith. Primarily emphasized are bodily healing and financial prosperity.

1. Miraculous healing is a major "gift of the spirit," can and should take place frequently, and is absolutely without limits. No disease is beyond healing. God does not heal through doctors or medicine but in a distinct, thorough, and instantaneous miracle. It should be emphasized that Allen perceives absolutely no physical or medical barriers to healing. In fact, he has entertained the possibility of reviving the dead. Failures are explained entirely theologically—as the result of sin or lack of faith. As he says, "Numberless people today are asking the question, 'Why wasn't I healed?' The answer is simple: no faith, or insufficient faith." Most of the healings dealt with on the radio program are of cancer. A mass healing of "tumors, goiters, hernias, and ruptures" concluded as follows:

> Now, Satan, I come against you in Jesus' name. You foul devil of tumors, goiters, hernias, growths, you foul tormentor, in the mighty name of Jesus Christ of Nazareth, I command you and I charge you, GO!

At the word "go" the rest of the congregation wails and screams in total frenzy. When the crowd quiets down, Allen calls for raised hands and "praise the Lord" and then proceeds to elicit testimonies from the people just healed.

2. Financial prosperity, like physical health, can be gained through scriptural means, namely, tithing and additional of-

ferings. To the non-believer, Allen's statements on financial success sound like cruel efforts to exploit the religiosity of his followers to the advantage of the Allen organization. He not only promises financial miracles in return for generous donations, but he also warns of dire consequences for those who "rob God" by contributing insufficiently.

Unlike earlier Pentecostals, Allen does not transvaluate poverty into a positive virtue.[16] While insisting that the "real reward is the shining crown at the end of the journey," he nevertheless affirms the value of financial prosperity in this world. However, as in the case of healing, the hard and imperfect means of the everyday world are denigrated in favor of an enchanted world of miracles for God's saints.[17]

The nine beliefs summarized in this section cover most of the material discussed in the broadcasts, booklets, and tracts. The general emotionality of evangelical religion is clearly shown in the frenzied healing services as well as in the "Pentecostal manifestation" of singing, clapping, dancing, shouting, etc. Like its historical forerunners, the revivals of Finney, Moody, and Sunday, the Allen movement is salvation Christianity: religion of the heart that leaves little role for abstract theology. As a religion of the oppressed it offers not only distraction—"we're having a time"—but concrete promises of healing and financial success. The reliance on miracles makes it possible for people who can objectively do relatively little for themselves in wordly efforts to seek, by spiritual means, health and wealth. Many participants in the movement appear to suffer from what might be called compound deprivation. In addition to the economic and social deprivation shared by most members, some individuals are burdened with "organismic deprivations" in the form of serious physical and psychological ailments. It may well be these compoundly deprived who make possible the highly intensive atmosphere of Allen's services.[18]

For the weakest and least powerful segments of society, Allen offers a God who is "a miracle worker . . . powerful, mighty and strong for those whose heart is perfect." In addition to these benefits, the movement allows for a set of spiritual criteria of worthiness to supplement those worldly criteria on which Allen's followers would appear lowly indeed.

IMPLICATIONS FOR SECULAR LIFE

A religious movement may have implications for a non-religious aspect of social life in three ways: (1) The religious teachings may deal directly with the topic—as in Christian Science's attitude toward medicine. (2) Religious doctrines may have indirect implications—as democratic church organization implies ground for democratic political practices. (3) By silence, religious teachings may indicate that behavior in some area of life is either not open to religious judgment or too inconsequential to be worthy of serious contemplations—as the silence of some churches on race relations. The likely effect of silence on any issue by a religion which claims to offer the answer to the meaning and purpose of life is that those topics on which the church is silent will be regarded as trivia of "the world" and ought not to be bothered with.

The Allen movement has implications in all three senses for intellectualism, politics, and race relations.

[16] For Pope's observations of transvaluation see Pope, *op. cit.*, p. 137.

[17] Despite the sobriety of the moral standards and the support given financial success as a goal, we cannot agree without major qualifications to Benton Johnson's assertion (*op. cit.*) that Holiness religion "socializes in dominant values."

[18] For an interesting treatment of deprivation, including organismic deprivation, see Charles Y. Glock, "The Role of Deprivation in the Origin and Evolution of Religious Groups," in Robert Lee and Martin E. Marty (eds.), *Religion and Social Conflict* (New York: Oxford University Press, 1964), pp. 24–36.

INTELLECTUALISM

The positive emphasis on the supernatural infallibility of every word of the Bible has inevitable consequences for human intellectual effort. Given a revealed source of perfect truth, other sources of truth are—if consistent with the Bible—second-rate. If other sources of truth—science, reason, intuition, etc.—are deemed contrary to the Bible, it is proof of the weakness and undependability of human knowledge. With respect to the view that God heals through medicine, Allen writes,

> If it is Bible truth, there must be some substantiation in the Word. If it is just theory, it would be better left out of such sermons and literature because one man's theory is as good as another man's theory. God's command is "Preach the Word," not "Preach theory."

This division of knowledge into biblical truth and theory, with the judgment that in the realm of theory one man's guess is as good as another's, is squarely in the tradition of evangelical anti-intellectualism. Compare Allen to William Jennings Bryan:

> All the ills from which America suffers can be traced back to the teaching of evolution. It would be better to destroy every other book ever written, and save just the first three verses of Genesis.[19]

Whatever value may be found in intellectual endeavor, the first rule is that it take a back seat to Scripture.

A movement which deals as extensively with healing as Allen's does must make an explicit peace with scientific medicine. The form this peace takes has implications for science and reason beyond the medical sphere. Allen teaches that medical healing is uncertain, imperfect, temporary—in all ways inferior to miraculous healing. God absolutely does not heal through medicine. Medicine, doctors, and hospitals are all part of man's foolish effort to heal without God.

> As far as I am concerned, medicine and drugs are not God-given or heaven-sent—they are the accomplishments of man.[20]

> I am not condemning you who still take such medicines, but if you will receive God's deliverance and trust Him you will not need them either.[21]

> Many people have been healed in Miracle Revival services who have had as many as four or five operations which had not effected a cure. I am not saying that physicians have not helped people. But I am saying there are some cases where they are of no value.[22]

> The surgeon's knife or radium may bring temporary relief, but it is a known fact that generally it [cancer, etc.] will show up in another part of the body shortly. There is but one SURE method of deliverance. That is God's way, the Bible way! "They shall lay hands upon the sick and they shall recover." Mark 16:18.

A content analysis of references to doctors, medicine, or medical science in *God Will Heal You* indicates that medical science is treated in one of two ways: either it is denigrated or it is accepted for purely diagnostic purposes. This finding is consistent with the mentions of doctors and related topics on the tapes. Doctors are not regarded as appropriate sources for truth as to the cause of disease, since the causes are invariably demons and curses as discussed in the Bible. Neither are doctors a very respected source of advice or treatment; they often fail, they are costly, their results are imperfect. Most important of all, they are unnecessary since there is a religious means of perfect healing. Despite this minimization of the importance of doctors, they are accepted as a source of information about the actual state of a patient at any given time. "My lungs were gone, they hung in two ribbons, but now [after being healed by Allen] from a fluoroscope, the doctor told me my lungs were perfect."

A woman healed of cancer of the stomach testified before the healing that she had

[19] Quoted by Hofstadter, *op. cit.*, p. 125.

[20] *Ibid.*, p. 22. [21] *Ibid.*, p. 21. [22] *Ibid.*, pp. 6–7.

been to the Mayo Clinic where doctors told her that "they did all they could do but not to give up, miracles happen, they can't do anything but God can." After this statement, Allen said, "Doctors have declared they can do nothing further but that they believe in miracles. Is cancer incurable with Him? [Crowd shouts] No!"

The assignment of the scientist to a small and inferior role in human affairs is not restricted to Allen's treatment of medical doctors. Our content analysis of mentions of scientists, authors, or other intellectuals in *My Vision of the Destruction of America* indicates that this procedure extends to non-medical scientists.[23] In his attempt to prove that the annihilation of the American population by nerve gas or other monstrous technological ingenuity is scientifically possible and not unlikely, Allen quotes many scientists. Among the authorities cited are such irreligious and politically liberal figures as Albert Einstein, Leo Szilard, and Harrison Brown. The religious or political professions of these men are of no consequence to Allen because, as in the case of doctors, their analyses of causes and solutions are of no interest. The causes of the destruction of America are theological—wilful disobedience of God's commandments. The solution is also theological—salvation. Scientists merely support the contention that the revealed vision described in the book is conceivable in terms of their knowledge.

Truncation of the area of scientific influence is not peculiar to A. A. Allen. More moderate religious groups reserve for theology the notions of First Cause and Ultimate Purpose. What is unique about Allen's teachings is the extremely small area in which science is deemed relevant once theological claims are staked out.

In addition to scientific evidence for a statement as to the existing state of the world and its immediate implications, Allen mentions scientists, writers, and intellectuals in another context: as usurpers of the moral role of church and debasers of public morality. *The Grapes of Wrath* and *Forever Amber* are branded damnable and rubbish—presumably for their treatment of sexual relations. Bertrand Russell and "social scientists" are taken to task for their influence on sexual mores and held responsible for the deterioration of old-fashioned chastity.

In summary, medical and natural scientists are a source of information about the present state of the world; their views on causation and future policy are of no interest since these matters are in the theological domain. Literature deals with alcohol, sex, and tobacco as do movies—it generally makes light of or actually indorses sin. Social scientists and psychiatrists marshal various worthless theories and demonic notions to undermine puritanical sexual morality—especially among young people.

It is hard to imagine how this view of various intellectual endeavors could have any effect other than to discourage even the most modest intellectual or educational aspirations among Allen's adherents unless such aspirations were narrowly vocational in nature.

However, we ought not forget that for many of Allen's followers this is probably not a new anti-intellectualism but an articulation of their long-standing ignorance and distrust of things of the mind. It is not inconceivable that for some of Allen's readers, the limited discussion of scientists' comments on nerve gas and atomic bombs is the largest dose of scientific opinion they have ever been exposed to. Allen's criticism of *The Grapes of Wrath* seems more likely to provide a rationale for people who habitually avoid literature than to discourage prospective readers. Allen's treatment of physicians probably has a deleterious effect on members' confidence in medical science and use of it. However, even here Allen's stand is quite moderate when compared with Mary Baker Eddy's. Mentions of medical attention, though rarely positive, are indeed

[23] Space limitations prevent presentation of the formal content analysis. It is available on request.

frequent. This seems to indicate that Allen followers make considerable use of doctors. Medical attention appears to be especially common in cases of serious illness.

From the standpoint of positive evaluation of science and intellect, the Allen movement seems to encourage the worst in its members and provide little or nothing in the way of adding to their appreciation for, or interest in, intellectual life. Literature, social science, psychiatry, and philosophy are rejected out of hand. Biological and physical sciences are consigned to relatively insignificant roles in human affairs.

RIGHTIST POLITICS

Recent sociological analysis of the far right has assigned fundamentalist religion a major role in the rise of "the new American right." David Danzig has argued that fundamentalism is a major force in extreme anti-Communism, antiminority, antilabor, anticity, and anti-internationalist politics.[24] Many of the contributors to Daniel Bell's *The Radical Right* see a similar tie-in between fundamentalist religion and reactionary political and social movements.[25] This section attempts to ascertain the relationship between the Allen movement and conservative politics.

First of all it must be noted that there is very little discussion of political and social issues in the Allen data we have studied. Only one statement in all the Allen Revival Hour broadcasts we have heard had any political significance. Even the single instance does not appear to be significant—it was a passing reference to "the Jews" in which Allen asked, and rhetorically answered, "Who's got all the money? It's the Jews."[26] The only portion of the Allen literature which deals at length with issues that have political implications is the booklets and tracts on eschatology. The major document in this category is *My Vision of the Destruction of America.* An analysis of mentions of political topics, Russia, and Communism, establishes that the message is highly apocalyptic. America will be destroyed totally and instantly by Russian atomic bombs or nerve gas. This is the only possible outcome in view of American sinfulness and scriptural passages which deal with retribution. Most important of all for our purposes is the fact that Allen sees no conceivable military, diplomatic, or political solution to this problem. In fact, it is not problematic but inevitable. "Military strategy, no matter how accurate or well directed, cannot avert the judgment of God bearing down upon a sin-filled world."

Neither should we assume that the crisis is due to internal Communist subversion (probably the central doctrine of the radical right):

> The most treacherous foe in America isn't Communism (as perilous as it may be), Nazism, Fascism, or any alien ideology, but MODERNISM. Can a country which has denied God a place in its government, its schools, its business, its homes and its churches escape the judgment and wrath of God? No. A million times no! America will not escape God's wrath. She has sinned, and she must reap exactly what she has sown.

This conviction of the inevitability of God's wrath makes many political issues of little consequence. For example, nuclear

[24] David Danzig, *op. cit.* This article presents an unfortunate lumping-together of diverse religious and political groups as part of an allegedly homogeneous fundamentalist right.

[25] Daniel Bell, *op. cit.* Bell, Hofstadter, Riesman, and Lipset all mention the role of fundamentalists in right-wing politics. While Riesman notes that not all fundamentalists are rightists, he emphasizes similarities between various fundamentalists. None of the contributors to the book troubles to differentiate between fundamentalist groups on theological, organizational, or class grounds. Our contention is that the middle class supports secularist political groups that are theologically vague and generally lacking in religious concern in addition to the traditional churches (see esp. pp. 80–86, 116–17, 123–24, 203–18, 263, and 296).

[26] The statement, made in passing and not as part of an anti-Semitic attack, appeared to be unreflective folk knowledge rather than part of any "hate doctrine."

testing and disarmament are not very important because

> Even if the A-bomb and the H-bomb should be outlawed, so to speak, there will remain other secret weapons such as biological (germ) warfare.

Allen's criticism of the United Nations is quite different from right-wing political criticism. It is not grounded in political considerations such as the Soviet veto or excessive influence of the left-leaning Afro-Asian block. It is an essentially theological criticism.

> It [the UN] is man's supreme effort to gain peace without the help of God.
>
> As I look upon these buildings [UN headquarters], I am reminded of the words of Christ, "When they say peace and safety, then sudden destruction cometh upon them."

One of the problems in understanding the relationship between fundamentalist theology and right-wing politics is a confusion over the role of Manichaean thought. Hofstadter writes, in his most recent essay on the far right,

> The little we know from the press about the John Birch Society, the Christian Crusade of Dr. Fred Schwartz, and Reverend Billy Hargis has served to remind us how much alive fundamentalism is in the United States, and how firmly it has now fixed its attention on the fight against Communism. . . . To understand the Manichean style of thought, the apocalyptic tendencies, the love of mystification, the intolerance of compromise that are observable in the right-wing mind, we need to understand the history of fundamentalism as well as the contribution of depth psychology.[27]

This statement, the article by Danzig referred to above, and the comments by Riesman and Lipset indicate a confusion of two distinctly different relationships between rightist politics and Manichaean theology. The groups and individuals mentioned by Hofstadter and the others, such as Billy James Hargis, the Schwartz Christian Anti-Communist Crusade, and the Birch Society, are primarily political. They represent a secularization—a metaphor—of the fundamentalist Manichaean theology. The focus is not on Communists as devils but on Communists *instead* of devils; true, the style of thought is Manichaean, but the content is not. There is very little theology in Schwartz's political discussions. The Christianity of his Crusade is a broad cultural embellishment. This is even more true for the Birch Society. Welch boasts of Jewish members and states the Society's commitment as a non-denominational approval of religion over atheism.[28] Further, despite the Christian label, Protestant, Catholic, and Jewish laymen have been prominently featured as sponsors of Christian Anti-Communist Crusades.

In this judgment we concur with Ralph Lord Roy who has recently insisted,

> contrary to the contentions of several eminent scholars the current upsurge of the Right Wing is not to be seen as a resurgence of Protestant fundamentalism. The error in this assertion is in the failure to understand the nature of fundamentalism. Piety has been confused with theology, for many important and affluent figures on the Far Right are exceedingly liberal theologically. Biblical literalism, for example, and such doctrines as the vicarious atonement of Christ play no part in their thinking. It is important to remember, too, that many fundamentalist leaders, among them Billy Graham, refused to join the Radical Right.[29]

The Pentecostal faith retains a literal and deeply religious Manichaean theology. Communists are not substituted for demons because demons exist and are real. If

[27] Richard Hofstadter, "Pseudo-Conservatism Revisited: A Postcript—1962," in Bell, *op. cit.*, p. 86.

[28] "Far from founding a religion, we are merely urging Protestants, Catholics, Jews or Moslems to be better Christians, Jews, or better Moslems" (Robert Welch, *The Bluebook of the John Birch Society*, 1961, p. 168).

[29] Ralph Lord Roy, "Conflict from the Communist Left and the Radical Right," in Robert Lee and Martin E. Marty (eds.), *op. cit.*, p. 62.

Communists are to be considered, they must take their place alongside a host of other agents of the devil. While some fundamentalist groups may mix right-wing politics with a fully religious Manichaeanism, the Allen movement does not appear to do so. Many of the groups linked together under the label "fundamentalist right" are regarded as anathema by Allen. Allen and other Pentecostals would undoubtedly regard the theologically liberal, though politically very conservative, Congregationalist James Fifield as a modernist and a skeptic, an agent of the devil. A large number of the churches Danzig regards as part of the fundamentalist right would be eschewed by Allen as "prisons with stained glass windows."

Allen never mentions Communists in the State Department or the evil influence of Keynes at Harvard. His response to national and world problems is a thoroughly religious one.

> Judgment is on the way. And there is only one hiding place from the wrath of God. That hiding place is in the cleft of the Rock, Christ Jesus, protected by His mighty hand.
>
> My friend, have you found the cleft in the rock? Have you been shut in by the mighty protecting hand of God? Do you have on the wedding garments? Are you ready for the wonderful coming of Jesus Christ?
>
> Now is the time to flee to the rock. Tomorrow may be too late. . . . Seek the Lord while he may be found. Call upon Him while He is near, Remember, TOMORROW MAY BE TOO LATE!

While Allen may take a stand on a political issue with direct religious implications, such as the Supreme Court decision on school prayer, the movement strikes one as deeply apolitical. That this is not peculiar to Allen, but rather part of the Pentecostal tradition is evidenced by Pope's memorable report of his efforts to ascertain the political and social beliefs of a Pentecostal minister in Gastonia.

> When asked about the "social views" of his denomination, a Pentecostal Holiness minister replied that his denomination engages in, and enjoys, Sunday School picnics and young peoples' "sociables," but does not permit oyster suppers to raise money for the church.[30]

When we recall the history of the Pentecostal churches and the probable social composition of the movement, it is not at all surprising that Allen's teachings are essentially apolitical. Lower-class Negroes from states which still have or have only recently abolished severe restrictions on Negro suffrage are probably heavily represented among Allen's followers. The South and Southwest have a conservative political image. However, for the rural poor and the urban working class of both races, the outstanding fact is the history and tradition of exclusion from even minimal political participation. The poll tax, the literacy laws, the single-party system, and the "courthouse crowd" resulted in very low political participation, especially for the lower class.[31] Further, even in other areas of the country, the groups most heavily represented in the Allen movement are precisely those least likely to vote.[32] They are also people least likely to participate in voluntary associations.[33] It seems highly unlikely that many lower-class Negro women of rural southern origin will, as a result of their participation in the Allen movement or through any other cause, join a political party, write frequently to government officials, conduct

[30] Pope, *op. cit.*, p. 164.

[31] For relevant material see W. J. Cash, *The Mind of the South* (New York: Vintage, 1941), esp. p. 251; V. O. Key, Jr., *Southern Politics* (New York: Vintage, 1963), esp. pp. 553–664; D. J. McGovney, *The American Suffrage Medley* (Chicago: University of Chicago Press, 1949), esp. pp. 50–51, 59–79, 81, and 110–11.

[32] Seymour Martin Lipset, *Political Man* (Garden City, N.Y.: Doubleday, 1959), pp. 184–85; Robert E. Lane, *Political Life* (Glencoe, Ill.: Free Press, 1959), pp. 220–34.

[33] Charles R. Wright and Herbert H. Hyman, "Voluntary Association Membership of American Adults: Evidence from National Sample Surveys," *American Sociological Review*, XXIII (1959), 284–94.

phone campaigns in school board elections, or engage in any of the other characteristic activities of the radical right. The minimal political act, voting, is probably much less common among Allen's followers than in the population as a whole.

Pentecostals are embroiled in battles but not of a political sort. They are locked in combat with "little old two-by-four infidels, agnostics, skeptics, and modernists." Allen encourages them and counsels them in their disagreements with ministers who "don't heal the sick and cast out devils" and friends and family who despise and mock them because of their full Gospel practices or deride them as "Holy Rollers."

The politics of the Allen movement are conservative in the literal sense of that word. Allen's people contribute to the preservation of the status quo by refraining from worldly efforts to change it. While the religious beliefs undoubtedly have a dynamic effect on individual members' personal lives, the movement has a minimal effect on public life. The implications of the belief that all man's woes are caused by sin and cured by salvation are that political solutions should be ignored or opposed since they are futile. The apolitical background of the movement appears to have resulted in a pattern of ignoring political solutions. If political awareness were to increase without a concomitant weakening in commitment to a theological view of the social world, the movement and Pentecostalism in general might become a source of political negativism and obstructionism.

RACE RELATIONS

The Allen movement is racially integrated. As indicated above, a large proportion of the revival congregations is Negro, and a Negro singer plays a prominent role in the organization. Despite these facts, race relations are mentioned in very few of the publications, and we have never heard the topic discussed in the radio broadcasts.

Although Allen's position on race relations is not a major feature of the movement, he has taken a position—and a strong one at that. The May, 1963, issue of *Miracle Magazine*, the official publication of the movement, has as the lead article, "How God Feels about Segregation." The cover of the magazine is also concerned with race relations. It shows Negro singer Gene Martin singing with Allen and another white member of the revival "team." Above the picture the scriptural support for Allen's position is printed:

> There is neither Jew nor Greek, . . . neither bond nor free, . . . neither male nor female: for ye are all one in Christ Jesus. Galatians 3:28.

The article itself is a passionate and unequivocal but strictly religious condemnation of segregation.[34] It is presented not as Allen's opinion but rather as a divinely revealed "message of prophecy" which "came forth with mighty anointing and fervor." Segregation is denounced as a "sin in the sight of the Lord" and an "evil in the house of God."

> I say unto thee, divide not thyselves into flocks that are segregated unto thine own call. For I say unto thee that segregation is not the call of God. Yea, whether it be in My Church or yea, whether it be on the street.
>
> I say unto thee, this is a boil, yea, and a sore evil in my sight. Yea and as I did send the curse upon the land of Egypt, I say unto thee this land is under a curse because of this very thing.
>
> I say unto thee, segregation shall not stand, for I the Lord thy God shall come upon the scene and ride speedily. I shall deliver My Church! I shall deliver My People!

The prophetic pronouncement is bordered with photographs which emphasize the integrated nature of the Allen movement, showing Allen ministering "impartially to all regardless of race or creed" or

[34] We should not ignore the possibility that the integrated character of the movement results from organizational considerations aimed at drawing the maximum of potential adherents rather than from religious convictions.

"integrated baptismal service," and pictures of a revival meeting are captioned, "People thronged to this well integrated Auditorium Campaign." Another page of pictures is headlined: "He Blesses All Alike." Again the interracial theme is heavily emphasized. "God places His sanction upon integrated services by pouring out his blessings on all alike."

While it is true that many of Allen's followers are Negro, it is also true that many are poor southern whites who might be expected to balk at Allen's integrationist sentiments. We do not know how whites reacted to the plea for integration. However, we suggest that the intense religious fellowship of the Pentecostal groups may be capable of eradicating, at least in the religious context, concern with race. This seems the most plausible explanation of such things as the success of integrated baptismal services in cities such as Atlanta.

An enhanced sense of personal worth and dignity is probably gained from success as a religious virtuoso in the fantastic healing miracles and outbursts of speaking in tongues. Perhaps the white's status security as one of God's saints makes denigration of Negroes, as a status defense mechanism, superfluous.

It should be pointed out here that Allen's concern with integration is not contrary to our early observations about the apolitical tenor of the movement. The pronouncements on race relations are purely religious. They make no mention of secular struggles for civil rights. There is no hint of either support or disapproval of civil rights organizations, government activity in the area of race relations, or political action to advance integration. On race relations as on other issues Allen proves to be unconcerned with worldly solutions.

CONCLUSION

The characteristics of Allen's teachings, especially in the area of politics and race relations, appear to call for some conceptual distinctions between fundamentalist groups. First of all, lower-class groups probably do not have the tradition of political participation and awareness which makes for a convenient blending of political and religious views. The analysis of class differences among fundamentalist groups is therefore essential.

Second, not all fundamentalist groups are equally elaborate theologically and in religious practices. The many unique beliefs and practices of the Pentecostal groups seem to "crowd out" most secular concerns. This commitment to an elaborate religion appears to be associated with otherworldliness. The scope of pure religiosity is enhanced at the expense of the usual concerns of the world.

Further, the presence of the word "Christian" in the name of an organization does not qualify it for the fundamentalist label or even guarantee that the group is seriously concerned with religion. We suggest that careful investigation of the religious beliefs and affiliations of participants in the various Christian anti-Communist crusades and schools will demonstrate considerable heterogeneity on strictly religious questions.

Secular, political, or social activity requires some sort of group consciousness. This prerequisite is not to be found in Allen's teachings. People are not appealed to in group terms—neither class nor racial awareness is encouraged. Rather, following the Protestant evangelical pattern, people are appealed to as individual sinners. Sociologists may be convinced that position in a social group is at the bottom of the troubles for which Allen's followers seek relief. The Pentecostal teachings, however, emphasize solutions to which the individual, the charismatic leader, and God are the only parties.

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Protestant Religion and Social Status

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ABSTRACT

Several studies show that members of Protestant denominations have higher status than persons who are not members of any denomination. Several studies also show that mean denominational status is *negatively* related to mean denominational church attendance rates. At the same time—within each denomination—status is *positively* related to attendance rates. These opposed relationships tend to cancel, producing a net relationship that approaches zero. Several sources of variance in social status and church attendance rates are reported.

The literature now contains an appreciable number of contributions relating social status to church attendance. Generally, the large sample studies have reanalyzed religious data that were gathered incidentally to serve as exploratory or control data for other problems not directly concerned with religion. When the relationship between social status and church attendance was measured, it was usually done without controlling for many of the variables that might be known (or expected) to be related to the variables under study, for example, age, sex, marital status, denominational affiliation, spouse's denominational affiliation, nativity, etc. The conclusions drawn from these studies have been various; the reported relationships have been negative, positive, curvilinear, or random. My own research, and limited reanalysis of data from some of these studies, suggest two factors that account for most of the inconsistency: (1) the absence, or non-utilization, of relevant controls, and (2) variations in the operational definitions of religious "affiliation."[1]

The present article will show that: (1) there is agreement among all studies that social status is *positively* related to *membership* in a Protestant denomination; (2) among those who are members, there are two opposite relationships between social status and religious behavior: (*a*) a *negative* relationship *between* denominations and (*b*) a *positive* relationship *within* denominations; and (3) these opposed relationships tend to cancel one another out so that the net relationship approaches zero.[2]

It should be emphasized that I am comparing Protestants with Protestants or with persons not affiliated with any denomination, Protestant or otherwise. While there is adequate reason for the exclusion of Catholic and Jewish respondents on the grounds of inadequate sampling, my more compelling reason has been the belief that their position as minority and marginal groups might interact with the relationship between social status and religion so as to require a complex analysis beyond the scope of the present paper.

[1] Depending upon the wording of the questions, the investigator may obtain the distribution of "religious preferences" in his sample (Protestant, Catholic, Jewish, no preference), specific "denominational preferences," and/or specific "denominational memberships." Each distribution will differ from the others. I shall use the term "affiliation" to refer to positive responses in any of these areas. In nearly all cases, the affiliation is ascertained by asking the respondent to assign himself. Such self-assignments are of doubtful validity in many cases, but they will be accepted as provisionally valid for the purposes of this paper.

[2] Persons who are members of no denomination have such low rates of church attendance that it would be difficult to detect any relationship between attendance rates and social status for them; in the samples available to me, no firm relationship could be detected.

SOCIAL STATUS AND RELIGIOUS AFFILIATION

Being affiliated with a Protestant denomination means—on the average—that one is of higher social status than the non-affiliated.[3] This relationship has been found in Allinsmith,[4] Bultena,[5] Burchinal,[6] Cantril,[7] and Hollingshead.[8]

The studies of Allinsmith and Bultena are of particular interest because their respondents were questioned about their denominational "preferences" as well as their actual memberships. From Allinsmith's data I have computed that 15 per cent of the sample have a denominational "preference" but *lack* a "church membership"; the comparable figure from Bultena is 18 per cent.[9]

It is regrettable that the social status of those who have only a denominational "preference" was not reported. It seems probable, however, that their status would be intermediate between the non-members and the members.

The proportion of the population who are not members of any denomination is variously reported by the different authors:

Author	Per Cent
Allinsmith	23
Bultena	30
Burchinal	23
Cantril	26
Hollingshead	16

These figures tend to underestimate nonmembership as it is reported by the denominations themselves. The data that they supply suggest that 30–40 per cent of the

[3] Social status has been measured by one (or two) of the following: (1) income, (2) education, (3) occupational prestige, (4) Hollingshead's measure of social class, and (5) the Office of Public Opinion Research's interviewer ratings. The variety of index is a result, largely, of utilizing different sources of data.

[4] Wesley Allinsmith, "Social-Economic Status and Outlook of Religious Groups in America," *Information Service,* XXVII (May 15, 1948), No. 20, Part 2. The *N* from several national samples combined was over 17,000. In his Table II, "no preference," "Protestant, undesignated," and "no answer" have larger percentages in the lower class than any Protestant denomination except "Baptists" and "smaller bodies." In this sample it is not possible to compare "members" with "non-members" on social status. One can only compare those with, and those without, a "preference."

[5] Louis Bultena, "Church Membership and Church Attendance in Madison, Wisconsin," *American Sociological Review,* XIV (1949), 384–89. A sample of every other household; *N* was over 24,000. Bultena did not compare the social status of Protestant members and of non-members. Comparing the occupational distributions of Protestants and of non-members, as presented in his table, $\chi^2 = 60$, d.f. $= 3$, and $p = < .001$. Non-members have the larger percentages of low-prestige occupations.

[6] Lee G. Burchinal, "Some Social Status Criteria and Church Membership and Church Attendance," *Journal of Social Psychology,* XLIX (1959), 53–64. The sample contained 256 husband-and-wife pairs from Iowa, Ohio, Kansas, and Wisconsin. See p. 61 for the comparison of members with non-members. This sample contained some twenty non-Protestant families, whom Burchinal included among his members. A separate tabulation of his raw data, excluding non-Protestant members, shows that the Protestant members, alone, have significantly higher status than non-church members.

[7] Hadley Cantril, "Educational and Economic Composition of Religious Groups: An Analysis of Poll Data," *American Journal of Sociology,* XLVIII (1943), 574–79. In several national "social" samples combined, Protestant members have higher educational and "economic" status than non-members.

[8] August Hollingshead, *Elmtown's Youth* (New York: John Wiley & Sons, 1949), p. 459, Table VI, and p. 460, Table VII. In Table VI the non-members are shown to have a lower social-class position than any denominational category, including Catholics.

[9] Allinsmith reports data for four national "voting" samples and two national "social" samples. Since the latter two are more representative of the United States population, I have relied on these, where possible.

It should be noted that among those who express a "denominational preference" many lack a "denominational membership," because two studies cited below measured the social-status–church-attendance relationship among persons selected by the criterion of having a Protestant "denominational preference." Such a category includes over 50 per cent of all those who are not members of any denomination.

population of the last decade are (or were) not members.[10]

SOCIAL STATUS AND CHURCH ATTENDANCE

As noted in the introductory remarks, there is little agreement among those measuring the relationship between social status and church attendance. For instance: (1) Bultena reports no relationship;[11] (2) Burchinal reports no relationship;[12] (3) Hollingshead reports a curvilinear relationship;[13] (4) Lenski reports a curvilinear relationship with one index of status, a negative relationship with another, and with a third index the direction of the relationship is not reported, but the relationship is said to be insignificant;[14] (5) Lazerwitz reports a positive relationship.[15]

I have been able to reanalyze the work of Bultena, Burchinal, and Lazerwitz. In each instance there is a negative relationship between the ranked mean frequencies of church attendance and the ranked mean social status of the Protestant denominations.

For Bultena's sample the relationship is —.60 (ρ, $p = < .02$) among seventeen denominations.[16] The relationship is also —.60 (ρ, $p = < .20$) for the eight denom-

[10] Michael Argyle, *Religious Behavior* (London: Routledge & Kegan Paul, 1958), p. 28.

Bultena's figure of 30 per cent could be considerably enlarged by adding the 15 per cent of his sample whose claimed membership lay in an "out-of-city" church. They would appear to be organizationally identical with the non-members.

[11] Bultena, *op. cit.*, p. 388. "A study of Protestant church attendance indicated there was no statistically significant difference between the attendance of *members* of different occupational, economic and educational levels." (Italics mine.)

[12] Burchinal, *op. cit.*, p. 63. Burchinal controlled for sex and measured status by education and occupation, separately. He found a significant positive relationship between social status and attendance in his total sample, but for *members* only, he concluded that there was only "a suggestion that perhaps even among church members, attendance was related to social status." In my reanalysis of his data on males with more controls employed (including the elimination of Catholics), the Pearsonian $r = .07$. Among Protestant members, then, this relationship approaches zero.

[13] Hollingshead, *op. cit.*, p. 98, reports that Class III (the middle class) attends most frequently. A footnote to this statement refers to Table VIII in the Appendix, which, however, is an obvious error. The footnote describes an index of "church activity" based on ministers' ratings of husbands and wives, to which Table VII in the Appendix seems to be the closest approximation. This table shows a *linear positive* relationship between social class and church attendance. The apparent contradiction between the textual statement and the table described makes it difficult to reach any firm conclusion, but I am inclined to give preference to the evidence in the table itself (see discussion below).

[14] Gerhard E. Lenski, "Social Correlates of Religious Interest," *American Sociological Review*, XVIII (1953), 533–44. While the dependent variable here was "religious interest" rather than church attendance, the high correlation between the two, as well as the general sophistication of the analysis, warrants its citation here.

[15] Bernard Lazerwitz, "Variations in Church Attendance," *Social Forces*, XXXIX (1961), 301-9.

[16] While it may be a puristic whim, I excluded the Christian Science denomination, feeling that it departs too radically from the mainstream of Protestantism. Bultena presents a table showing the mean frequency of church attendance, the mean educational attainment, the mean rent value of dwellings, and the percentage distribution in four occupational-type categories. The denominations may be readily ranked by their mean frequency of church attendance. As a measure of social status, I ranked their mean educational attainment and the percentages *not* in the laborer category. For each denomination, these two rank-order values were then summed, and the resulting distribution was re-ranked. The ρ was computed between the rank order on this social status measure and the church-attendance ranking.

Computed separately, the Spearman ρ's between church attendance and (1) percentage non-laborer, (2) education, and (3) rental value are, respectively, —.72, —.57, and —.36. The last figure is not significant at the .05 level.

One denomination, containing only nineteen sample members, contributed 60 per cent of the D_i^2 in the computation of $\rho = -.60$. Excluding this single denomination, the United Brethren, would raise the ρ to about —.90. I strongly suspect an unusual sampling of the members or some unusual circumstances affecting this congregation at the time the data were gathered. The total Protestant sample contained over 10,000 members.

inations used in Burchinal's analysis.[17] Among Lazerwitz' sample of seven denominations there is a ρ of -1.00 ($p = .002$).[18]

The ubiquity of these results is underscored by the fact that Bultena studied an urban sample (Madison, Wisconsin); Burchinal had a rural sample from the Midwest; and Lazerwitz used a national sample.[19]

The conclusions these three original analysts presented were based on a particular method—lumping all the respondents together as individuals. The negative relationships that were found in reanalysis were contingent upon separating the respondents into denominational categories. The different results are not so much contradictory as complementary.

Putting together the results obtained by Bultena, Burchinal, Lazerwitz, and me, it is deducible that there must be intradenominational "positive" relationships between social status and church attendance; but deducible or not, there are. Lazerwitz analyzed his seven denominations separately and found that *within* each denomination there was a positive relationship between status and attendance.[20]

Burchinal's own analysis did not include an examination of each denomination separately; however, he kindly made his raw data available to me, and I can report that *within* the denominations of his rural sample there are positive relationships also.[21]

In the case of Bultena's data, it is still necessary to rely solely on a deduction for the conclusion that there is a positive relationship within denominations; but this deduction is necessary to account for his report of "no" relationship, treating all Protestant respondents individually, and the discovery of a significant negative relationship *between* denominations.

It is not difficult to understand how two opposed relationships might cancel one another out to produce an over-all relationship approaching zero, and this was the case with Bultena's and Burchinal's data. However, a difference occurs in the case of Lazerwitz' data; for here the *intra*denominational positive relationship and the *inter*denominational negative relationship produced an over-all, or net, positive relationship.

Of the several possible reasons for this discrepancy in results one very probable reason should be described. Both Burchinal and Bultena found the lack of association in a sample of Protestant "members" of denominations. Lazerwitz found the moderate positive relationship among a sample

[17] The ρ was computed from the rank orders of the mean family income for each denomination and the mean frequency of church attendance for the males.

[18] The ρ was computed from rank orders of mean educational achievement for each denomination and the percentage who attended church regularly or often. This and all other p values are two-tailed.

[19] I have refrained from presenting supporting tables since a representative example is already in print (see Bultena's published table to verify the existence of a negative relationship). Lazerwitz has published tables for Methodists and Baptists, and has offered to furnish data on other denominations on request.

[20] His published tables for the Methodists and Baptists clearly show the positive relationships. Using these and his other tables, one can compute the following ρ's between the five, ranked educational levels and the ranked percentages from each level who attend church "regularly" or "often."

Episcopalian	0.40
Presbyterian	.80
Other non-Fundamentalist	.875
Methodist	.90
Lutheran	.50
Other Fundamentalist	.625
Baptist	0.60

[21] The small size of the sample, the very small size of most of the denominations, and the very small number of coded categories of church-attendance frequencies (three) make it impractical to compute the strength of positive relationships in each denomination. However, the direction of the relationship is positive in seven of the eight denominations. The probability that seven or eight of these denominations would show the same direction of relationships (either positive or negative) is .07.

of persons who had expressed a "preference" for Protestantism.[22]

Allinsmith's data reveal that 17–20 per cent of those who express "denominational" or "religious" preferences would not be "members" of any denomination. Since all of the research measuring the association between social status and membership status has found the non-members to have a low average social status, and since they are also known to have very low rates of church attendance, the inclusion of this large a percentage (17–20) of non-members would tend to result in a positive relationship in the larger sample.[23]

At this point we are in a position to bring some of Hollingshead's results into congruence with those of Bultena, Burchinal, and Lazerwitz. It was noted in the first paragraph of this section that Hollingshead reported a *curvilinear* relationship between social status and attendance—specifically, that Class III persons attended more frequently than any other class. He describes the basis of this relationship in more detail in a footnote, which further cites a table in the Appendix. Contrary to the textual description, this table shows *unilinear, positive* relationships between "social class" and church attendance for both males and females separately. The C values are .55 for males and .66 for females; the p values are less than .01.

These results (positive relationships) parallel those obtained by Lazerwitz and my reanalyses of Bultena and Burchinal, because they actually measure the relationship *intra*denominationally. Hollingshead says, "Each minister rated the church activity of each family in his church from September, 1941, through April, 1942."[24]

The categories by which the church members were rated were (1) minister does not know, (2) does not attend church, (3) attends church rarely, (4) attends church irregularly, and (5) a church worker. The relevant characteristic of these categories is that the last three do not impose the same standard of measurement on all ministers, for example, what constitutes "irregular attendance" or "a church worker" may be, and probably is, different for different ministers. This kind of scale approaches being an ordinal scale; but insofar as it is ordinal, then each minister has *ranked* his own congregational members; and Hollingshead has superimposed all of these ranks on one another.

The empirical evidence from Burchinal and Lazerwitz shows, however, that when each denomination is treated separately and the members' frequencies of church attendance are compared with others in the same denomination, then the more frequently attending members have the higher social status. By logical deduction, Bultena's data point to the same conclusion. From the evidence that Hollingshead presents, he has measured the same intradenominational relationship; but use of an ordinal scale (or at least one that approaches ordinality) permits him to superimpose the distributions received from each minister upon one another. Thus, the tabulation *cannot* reflect the effect of any *inter*denominational negative relationship.[25]

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[22] When measuring the Protestant *intra*denominational relationships, Lazerwitz used two combined national samples that had been asked to express *denominational* preferences. When measuring the over-all relationship among Protestants he combined these with an additional sample that had been asked to express *religious preferences* only (Protestant, Catholic, Jew).

[23] Bultena, *op. cit.*, p. 385. The index figure for Protestant member church attendance is 1.85; for non-members, .49. In Burchinal's sample, 4 per cent of the male Protestant members never attend church. For male non-members the percentage is 67. Only one in fifty-eight non-members attended "regularly." The inclusion of even a very small percentage of non-members can contribute to a positive relationship between social status and attendance. See nn. 9 and 12 above.

[24] Hollingshead, *op. cit.*, p. 98.

[25] In addition, the first two categories of his "church-activity" index must include a large proportion of persons who are not church members and who would contribute heavily to a positive relationship.

lished account that suggests the existence of a negative *inter*denominational relationship, even by deduction. It is very possible that his data actually could show the curvilinear relationship that he reports—with Class III attending most often. If the data can support such a conclusion, however, then the scale of church attendance must be cardinal. Possibly an oversight resulted in the omission of such tabular evidence, but it seems warranted to withhold further judgment concerning the curvilinear relationship until more evidence is available.

In brief, this section has reported the generally diverse results of four studies that measured the relationship between social status and church attendance, plus one study that measured social status and "religious interest." Evidence obtained in reanalyzing three of these studies showed that a moderate *negative* relationship existed when the units of analysis were the denominational averages of social status and church attendance. Further evidence from four studies showed that within each denomination a moderate *positive* relationship existed, or was deducible. These two opposed relationships tended to cancel one another out, producing a net or over-all relationship approaching zero. However, when the analysis sample contained a significant proportion of non-members, the over-all relationship tended to be positive.

OTHER VARIABLES TO CONTROL

Several variables have been shown in the literature to relate to social status or church attendance, yet the research that has followed the initial publication of such findings has generally been replicatory only.[26] After the findings had shown that non-church members had lower social status than church members, Bultena and Burchinal were the only ones to control membership status when measuring the relationship between social status and church attendance. Burchinal, measuring the relationship with and without this control, demonstrated its power.

Similarly, despite a consistent finding (and common knowledge) that church attendance is significantly related to sex, Burchinal and Hollingshead were the only analysts reviewed here who controlled for sex when relating social status to attendance.

The relationship of age to church attendance has not been consistently established, but enough evidence has been gathered for it to seem quite promising to use age as a control variable when relating social status to attendance. Irrespective of the relationship between age and attendance, it is known that age is related to social status; and, therefore, it should be controlled on that account alone.

My own analysis of Burchinal's data shows age to be so positively related to church attendance among members that it should always be controlled when relating social status to church attendance.[27]

Lenski has shown that among Protestants, when spouses do not "prefer" the *same* denomination, there is less "interest in religion" than when the spouses are identically affiliated. The percentage of Protestants who are cross-affiliated with their spouses seems to be sizable.[28] And Burchinal's data showed the cross-affiliated

[26] That is, the authors of successive studies have been content to run the same variables against church attendance and stress the similarities or differences between their results. The infrequently practiced alternative was to utilize a variable of demonstrated predictive power, *as a control,* when relating social status to church attendance.

[27] Among males, age groups were divided into three categories (< 40, 40–49, > 49) and attendance into two categories; $\chi^2 = 9.23$, d.f. $= 2$, $p = < .01$, and $T = .20$. This relationship held up under controls for occupation as well as income. A separate analysis of non-members showed age to be *negatively* related to church attendance. This result suggests again the necessity of controlling both membership status *and* age.

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have less social status than the identically affiliated, though they have more than the non-affiliated. They have lower rates of church attendance than the identically affiliated, but higher rates than the non-affiliated.

There is a sizable proportion of Protestant members whose spouses are not members of any church. In Burchinal's sample these amounted to 14 per cent of the 202 families in which one *or* both spouses were Protestant members. Like the cross-affiliated, the respondents in these families have relatively low social status and lower rates of church attendance, whether one studies the Protestant church member or his non-member spouse.[29]

If the cross-affiliated families and the families containing one non-member were very small percentages of the total, or if they were only a little different in terms of social status or church attendance from the identically affiliated, these categories could be ignored. Actually, they are sizable percentages, and they differ greatly enough to produce very misleading results when not controlled.

It is safe to say that until the time of marriage most respondents' rates of church attendance are affected by the religious atmosphere in their family of orientation. How large the effect is after marriage is conjectural, but it seems probable that it is measurable. Control for this factor should reduce variance when measuring the relationship between social status and church attendance.

[29] If the husband was a non-member, 45 per cent of the wives attended regularly, and none of the husbands did. If the wife was a non-member, no husband attended regularly, and only 20 per cent of the wives did.

SUMMARY AND SUGGESTIONS

A more refined analysis of published data permits the assertion that there are two opposed relationships between social status and church attendance among Protestants. In three studies there is evidence of negative *inter*denominational relationships. In four studies there is evidence of a positive *intra*denominational relationship.

Many studies have shown or suggested the relevance of several variables which, if controlled, would lead to a more accurate assessment of the social status—church-attendance relationship(s). Some of these variables have been described here; and while they are not exhaustive, it would seem that a large amount of variance could be removed by taking them into account. Individually, they may not account for very much variance; but taken together, they probably are a source of far too much variance to be ignored.

I believe that the major results obtained by this reanalysis will stand up to more intensive investigation; but I am all too aware of the difficulties that were involved in using different samples, and with only partial access to the original data, to expect these new results to do more than paint a broad picture. Much more exact results should be obtained from large samples, well investigated and analyzed with more sophisticated statistical techniques.

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Overt Conformity to Church Teaching as a Function of Religious Belief and Group Participation[1]

John D. Photiadis

ABSTRACT

Data collected from members of a small-town Mormon church have been used to test hypotheses dealing with possible functions of the church in modern society. This testing suggested that overt conformity to church teaching is influenced cumulatively by church participation and religious belief, and independently by church participation. It is speculated that this independent function of church participation is determined by the mutual dependence between such participation and attachment to the social system, and operates independent of the known mutual dependence between belief and ritual.

Current theories regarding the place of religion in society have been drawn primarily from studies of non-literate societies and, in large part, from studies of a generation or two ago. More recently, several writers questioned these concepts because they do not fully account for the place of religion in modern, complex, and changing societies.[2] These early theories tended to ignore the functional alternatives to religion in meeting societal needs and, primarily, the function of the church as a social system.

In order to study the social functions of religion, Bellah suggested "the study of norms, where concrete action derives its meaning, and of the superordinate meaning system which gives meaning and coherence to the norms."[3] The present analysis uses the study of norms, whose derivation it considers in the light of two such systems—the doctrinal belief system, and the social system. Its purpose is to compare these two systems as to their function in determining overt conformity to church teaching, and in turn use this comparison to pinpoint some of the possible social functions of the church in modern society.

More specifically, it examines (*a*) overt conformity to church teaching as determined independently by religious belief and by church participation; (*b*) overt conformity of weak believers participating in social subsystems which include different proportions of strong believers (who as a consequence are strong overt conformists); and (*c*) whether besides the known mutual dependence between ritual and belief which leads to overt conformity to church teaching, there is also a mutual

[1] This paper is based on data obtained in a study of a Mormon trade area center of approximately 7,000 population and seven small villages which surround it and use it for many of their services. The community included 15 wards (local church units) of the Church of Jesus Christ of Latter Day Saints (Mormon) and 86 per cent of the population were of the Mormon faith at the time the study was made. The larger study was focused on leadership development in a Mormon community and was conducted by Dr. William W. Reeder of Cornell University, who was the chairman of the author's graduate committee. The author was given permission to use some of the data for his Ph.D. thesis and to publish this article based on part of the data in the thesis. The analysis and interpretation are the author's.

[2] Allan Eister, "Religious Institutions in Complex Societies," *American Sociological Review*, XXII (1957), 387–91; Charles Glock, in *Sociology Today*, ed. R. Merton, L. Broom, and L. Cottrell (New York: Basic Books, 1959), pp. 154–57; Elizabeth Nottingham, *Religion and Society* (New York: Random House, 1954).

[3] Robert N. Bellah, "Some Suggestions for the Systematic Study of Religion" (McGill University, Montreal, Canada, 1956) (mimeographed)

dependence between participation and attachment to the social system which leads to the same conformity.

PROCEDURE

The data were collected in a Mormon trade area community of about 10,000 population. A sample of 553 male adult members, drawn from the entire congregation of the fifteen wards in the community, filled out an anonymous questionnaire.[4] All persons in the sample were invited to their ward chapel by letter on a weekday evening. Fifty-six per cent responded to this invitation and completed questionnaires in a group interview, placing their completed questionnaires in a box to maintain anonymity. The remaining 44 per cent were contacted personally, and completed questionnaires were placed in a specially constructed mail-type bag to preserve the anonymity of all respondents.

Overt conformity to church teaching is the dependent variable, and it is measured by behavior prescribed by norms dealing with that kind of conformity, such as abstinence from smoking and from drinking alcoholic and certain other beverages. Nearly all such behavior refers to individual behavior, with the exception of the offering of grace before meals.

Besides being prescribed by norms, these aspects of behavior are also rituals. According to Kingsley Davis, "Religious ritual is the active side of religion. It can include any kind of behavior known, such as the wearing of special clothing, starving, feasting, reading, etc. The religious character of the behavior does not come from the behavior itself, but from the attitude taken toward it."[5] Abstinence from drinking, for instance, or offering grace, besides being behavior prescribed by a norm, are rituals, expected to have a sacred meaning for members of this denomination. The dependent variable, overt conformity to church teaching, then is used in this study as a measurement both of behavior dealing with conformity to group norms and of ritual.

Church participation is measured with a modified Chapin Formal Participation Scale.[6] Our scale did not include items on membership, because the sample included only church members, or items on contributions, because tithing was practiced. It included participation in intra-organizational activities such as dances, dinners, and discussion meetings sponsored by the church where church and non-church matters were discussed. The latter was included so that all possible interaction and involvement in the church group would be measured.

Religious belief is measured with eight doctrinal items dealing with the individual's personal belief about the existence of God and the nature of God.[7] The term weak believer does not refer to non-believer but to acceptance of fewer standard elaborations of theology.

A test of unidimensionality for both the

[4] The sample was limited to members of the Mormon faith and was a stratified sample of male church officers and teachers, active members, and inactive members or low attenders between 20 and 65 years of age. The educational characteristics of the sample were as follows: 8 grades or less—12 per cent; some high school—23 per cent; high-school graduate—23 per cent; some college—29 per cent; college graduate—13 per cent; total—100 per cent.

Occupational characteristics were as follows: professionals—11 per cent; business managers—12 per cent; farmers—28 per cent; sales and clerical—9 per cent; laborers—28 per cent; other—12 per cent; total—100 per cent.

[5] Kingsley Davis, *Human Society* (New York: Macmillan Co., 1949), p. 534.

[6] Stuart Chapin, *Experimental Designs in Sociological Research* (New York: Harper & Bros, 1944). The initial Chapin Scale included five attributes: (*a*) membership, (*b*) dues, (*c*) attendance, (*d*) committee membership, and (*e*) offices held.

[7] Doctrinal orthodoxy refers to that orientation which stresses intellectual assent to prescribed doctrines. Besides the doctrinal aspect, Gerhard Lenski, in a similar study, used devotionalism which refers to orientation emphasizing the importance of private or personal communion with God (*The Religious Factor* [Garden City, N.Y.: Doubleday & Co., 1961], p. 25).

belief and overt conformity items showed a coefficient of reproducibility close to .96.

In order to treat data dealing with interaction patterns between strong believer conformists and weak or non-believer conformists, the following four group categories which include unequal proportions of believer conformists are used: (*a*) high participants in all church functions, (*b*) high participants in business meetings, (*c*) high participants in intrachurch activities, and (*d*) officers. These are more quasi-social systems than actual social systems. However, members of these group categories, which in certain cases overlap, interact more among themselves than with outsiders—which implies the social system[8] —and have some common social characteristics—which implies consciousness of kind.

FINDINGS

Religious belief is a predisposition to action. Thus, it is expected to influence overt conformity advocated by the church. These data show that the relationship between belief and overt conformity to church teaching is positive and significant with $\chi^2 = 40.10$ and $p < .01$. The association between these two variables measured with the ϕ coefficient[9] is .272.

This association is not exceptionally high, and it could indicate either that belief is a weak motive, or that a number of respondents who are weak believers are also strong overt conformists. This second alternative appears justifiable when one considers that congregations, being autonomous social systems, could influence overt conformity to church teaching independent of belief. If this speculation is correct, then church participation which implies involvement in the church social system should be more related than belief to overt conformity. This should be the case because in the relationship with belief, high overt conformists will include not only strong believers, but also, as speculated above, non-believer social conformists. Thus, this relationship will not be as high as that with participation where high overt conformists will be mainly high participants, but not believers who are non-participants. Belief usually implies participation, but as speculated above the opposite is not necessarily true. The data support the speculation by indicating that the relationship between church participation and overt conformity, with $\chi^2 = 245.70$ and $\phi = .689$, is much higher than the relationship between belief and overt conformity (same dichotomy for overt conformity in both tables) where ϕ is only —.272.

Further, if our initial speculation is correct and the social system determines overt conformity independent of belief, then the relationship between belief and overt conformity should be lower among the high than the low participants. Since the latter are not as involved in the social system, when they conform they should do so primarily because of their beliefs rather than because of social pressures.

Table 1 shows the relationship between overt conformity and belief when church participation is used as a control variable. The initial relationship ($\chi^2 = 40.10$; and $\phi = -.272$) disappears in the group of high church participants where $\chi^2 = .42$, $p > .05$, and $\phi = .050$. Even those who are weak believers are high overt conformists when they participate extensively in the group. Most probably, the more they participate in the group, the more they like the members, use it as a reference

[8] Interaction which is repeated tends to develop certain uniformities over time, some of which tend to persist. As they are orderly and systematic, they can be recognized as social systems (Charles Loomis, *Social Systems* [Princeton, N.J.: D. Van Nostrand Co., 1960], p. 3).

[9] The ϕ coefficient (mean square contingency) is a measurement of association appropriate for four-cell tables (see James E. Wert, Charles O. Neidt, and J. Stanley Ahman, *Statistical Methods* [New York: Appleton-Century-Crofts, 1954], p. 153). Like any other χ^2-type statistical measurement, ϕ has certain shortcomings (see Leo A. Goodman and William H. Kruskal, "Measurements of Association," *American Statistical Association*, Vol. XLIX [December, 1954]).

group, and overtly conform to its norms. On the other hand, in the group of low participants, the initial relationship between overt conformity and belief has been retained with $\chi^2 = 16.76$, $p < .01$, and $\phi = .188$.

The above data then indicate that church participation is related to overt conformity independent of the degree of belief. But the reverse is not necessarily true, because belief does not usually exist without church participation.[10] This then allows us to speculate that, at least in complex societies, participation in the church group is necessary in order for belief to be retained or grow. If this is the case, what then is the function of the social activities which are adopted by modern churches (considering that such activities increase or initiate participation of weak believers and non-believers among others)? This will be discussed later.

TABLE 1

RELATIONSHIP BETWEEN OVERT CONFORMITY TO CHURCH TEACHINGS AND RELIGIOUS BELIEF WHEN TOTAL CHURCH PARTICIPATION IS THE CONTROL VARIABLE

Overt Conformity	Total Church Participation (Per Cent)			
	High Belief		Low Belief	
	High	Low	High	Low
High	87	83	27	10
Low	13	17	73	90
Total	100	100	100	100
Total N	(99)	(64)	(94)	(272)
	$\chi^2 = .42; p > .05; \phi = .050$		$\chi^2 = 16.76; p < .01; \phi = .188$	

[10] This does not indicate that belief cannot exist without church participation, because we know how strong the beliefs of some hermits were. We do not know, however, if this is true today, because modern society's mass communication and transportation systems are so effective that they reach almost everybody.

Now let us consider further the independent influence of the social system in determining overt conformity, examining whether the proportion of believer conformists in a group influences the overt conformity of weak or non-believers.

If we assume that the social system forces weak believers to become strong overt conformists, we could further hypothesize that overt conformity would be enforced more in social subsystems with higher proportions of strong believers who, as a consequence, are strong overt conformists. This is expected to be true primarily among high participants because they are more active in the social system. The social systems selected to test this hypothesis are the four group categories mentioned previously.

Table 2 shows the relationship between overt conformity and belief in these four groups. Each group includes only high participants. The table indicates that, as the number of strong believers (who because of their belief are also high overt conformists) increases from 67 to 87 per cent, the difference between the proportion of high overt conformists who are high believers and of high overt conformists who are low believers decreases—from 21 per cent (67 minus 46) to 4 per cent (87 minus 83). Correspondingly, the difference between the two sets of proportions expressed in terms of association between the two variables is 0.189, 0.168, 0.093, and 0.050. The latter two are derived from relationships which are not statistically significant.

These data, then, besides supporting the initial proposition that the social system can influence overt conformity independent of belief, further suggest that the intensity of this conformity is in turn determined by belief and more specifically by the number of believer conformists in the social system.

Having established that church participation and overt conformity (which could both be treated as rituals) can exist independent of belief, let us see how compatible this is with the known mutual dependence between belief and ritual.

Theorists such as Durkheim, Malinowski,[11] and Chapple and Coon have discussed the mutual dependence of belief and ritual. They refer to ritual as a conditioning factor reinforcing religious beliefs, which in turn reinforce ritualistic behavior. From Durkheim's point of view, "These two elements of the religious life are too closely connected with each other to allow any radical separation. In principle, the cult is derived from the beliefs, yet it reacts upon them."[12] Chapple and Coon present a comprehensive analysis of this mutual dependence which they explain using the conditioned response theory of learning.[13] According to these theories, a church member who engages in the ritual of either attending church or abstaining from drinking is immediately reminded of his beliefs. This conditioning strengthens his beliefs; the strengthened beliefs in turn help him continue his participation and abstinence. Thus, the belief-ritual system builds up.

The present data actually support this theory because belief was found to be related both to church participation and to overt conformity to church teaching (which is a form of ritual). The same data, however, show that ritual in either the form of church participation or overt conformity can exist without belief. Ritual and belief, then, are not always mutually dependent. Ritual, in this case, does not always condition belief, but perhaps it conditions a different sentiment. As previously suggested, it could be that in modern society church participation (a part of which is secular) initiates sentiments of liking toward the members of the group or a desire to be accepted and have higher status in the group. Similarly, certain persons who participate or abstain from drinking do not necessarily think of themselves as participating or abstaining from drinking because they have these beliefs. But they could think of participation or abstinence from the standpoint of good group membership,

TABLE 2

SOCIAL CONFORMITY IN SUBGROUP CATEGORIES WITH DIFFERENT PROPORTIONS OF BELIEVER CONFORMISTS*

Group Category	High Overt Conformity (Per Cent)		Significance of Difference between Two Proportions	φ Coefficient
	Strong Belief	Weak Belief		
High participants in all church functions	87 (86)	83 (53)	N.S.	.050‡
High participants in formal meetings	77 (75)	69 (51)	N.S.	.093‡
High participants in intra-church activities	72 (71)	56 (45)	5 per cent	.168
Officers†	67 (70)	46 (59)	1 per cent	.189

* This table includes the parts of four three-way tables that show the proportion of high participants who are high overt conformists. Figures in parentheses indicate the actual cases corresponding to the proportion above.

† Among those who hold a large number of offices, the smaller proportion of high overt conformists who are also strong believers (as compared with the other forms of participation) is probably due to different dichotomies applied in each of the four control variables.

‡ Derived from relationship not significant at .05 level.

[11] Bronislaw Malinowski, *Magic, Science and Religion and Other Essays* (Glencoe, Ill.: Free Press, 1948).

[12] Émile Durkheim, *The Elementary Forms of Religious Life* (New York: Macmillan Co., 1915), p. 101.

[13] Eliot D. Chapple and Carleton Coon, *Principles of Anthropology* (New York: Henry Holt & Co., 1942), chaps. iii and xxi; Ernest Hilgard, *Theories of Learning* (New York: Appleton-Century-Crofts, Inc., 1948).

and because they obey the group norms they should be given group acceptance and raised status.

The present data thus support a hypothesis that today's churches, besides initiating the system of ritual-belief, initiate an additional system—that of interaction or participation and attachment to the church group. This would, in turn, suggest that the introduction of secular activities into the American churches could have contributed considerably to their success and survival. This contribution could refer not only to the attachment of individuals to the social system[14] but also to the doctrine.

CONCLUSION

With respect to the particular group, instrument of observation, and methods of analysis discussed here, the data indicate that the interaction patterns established during participation in various church activities (which comprise the social system) are more instrumental than religious belief in determining overt conformity to church teaching. This is the case because, although belief and church participation, which often coexist, influence overt conformity to church teaching cumulatively, church participation influences overt conformity independently. The effectiveness of the social system in securing such conformity independently depends on the number of individuals in the system who are strong believers and, as a consequence, high overt conformists. The higher the proportion of such strong believers, the more likely it is that weak believers will become high overt conformists.

Finally, because the relationship between church participation and attachment to the social system can influence overt conformity to church teaching independent of belief, we can speculate, if not assume, that modern churches secure the overt conformity of their members because of (1) the established mutual dependence of belief and ritual, and (2) the dependence between church participation and attachment to the social system discussed here.[15] Further, it has been speculated that this second mutual dependence could lead to the first.

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[14] Such attachment could refer to (*a*) attraction of new members or increased participation by present members, (*b*) maintenance or strengthening of already held religious beliefs through increased interaction and identification with the group.

[15] The two systems function independently in the case where church participation is not related to belief. They also function independently in cases where belief exists, but certain aspects of behavior are determined only by social forces and do not condition belief.

The Changing Character of Negro Migration[1]

Karl E. Taeuber and Alma F. Taeuber

ABSTRACT

Recently published data on migration during the 1955–60 period reveal that, contrary to the popular stereotype, Negro in-migrants to a number of large cities, despite the presence of a socioeconomically depressed group of non-metropolitan origin, were *not* of lower average socioeconomic status than the resident Negro population. Indeed, in educational attainment Negro in-migrants to northern cities were equal to or slightly higher than the resident *white* population. Comparisons with limited data for earlier periods suggest that, as the Negro population has changed from a disadvantaged rural population to a metropolitan one of increasing socioeconomic levels, its patterns of migration have changed to become very much like those of the white population.

An extensive literature portrays a situation in which whites are abandoning cities to a Negro population increasing daily through the addition of low-status migrants who produce increasing burdens upon schools, welfare agencies, the police, and other public agencies. General demographic research on migration, however, has usually shown that it is the higher status segments of a population which are the most residentially mobile. Knowledge of the profound changes in the distribution of the Negro population during the last fifty years, highlighted by the 1960 Census figures showing Negroes to be more urbanized than whites, also prompts caution in accepting easy generalizations about Negro migration.

The fragmentary sources upon which we base most of what is known about the early period of large-scale Negro migration are in general agreement that Negro in-migrants to cities were of lower social and economic status than both the resident Negro and white populations. Increased racial tensions were blamed upon the heavy influx of Negroes of low socioeconomic status. Even the old Negro residents are reported to have shared in resentment of the new in-migrants:

> Inevitably many of them were inclined to hold the migrants responsible for these increasing social restrictions and tended to resent the influx of other blacks, many of whom were inferior in education and culture to the northern Negroes and many of whom were unaccustomed to northern standards of living and modes of conduct.[2]

Contemporary evidence usually indicated that the newcomers were drawn from the higher status segment of the population of origin.[3]

Analysis of characteristics of migrants is a complicated task. Migrants have both a place of origin and a place of destination, and their characteristics may be compared with non-migrants at either place. Furthermore, migrations are not all in one direction, but flow in complex and interlocking channels. Precise determination of the differing character and relative importance of each migration stream requires a large body of systematic data. Unfortunately, much of the literature has been based on

[1] Paper No. 22 in the series "Comparative Urban Research," issuing from the Population Research and Training Center, University of Chicago, under a grant from the Ford Foundation. An earlier version of this paper was read at the 1964 Annual Meeting of the Population Association of America.

[2] Louise V. Kennedy, *The Negro Peasant Turns Cityward* (New York: Columbia University Press, 1930), p. 222.

[3] Carter G. Woodson, *A Century of Negro Migration* (Washington, D.C.: Association for the Study of Negro Life and History, 1918).

data that are not only unsystematic but empirically untrustworthy.

Although previous census data have permitted some studies of the characteristics of urban migrants, recently published data from the 1960 Census are by far the most detailed.[4] These data are based upon a comparison between place of residence reported for April 1, 1955, and place of residence at the time of the census in April, 1960. A difference between the two places of residence indicates that at least one move was made during the five years, although some persons may have made several moves during the period and what appears to be a "move" from one place to another may have involved several intervening residences. Necessarily excluded from the migration tabulations are children under five years of age, those who moved between 1955 and 1960 but died before enumeration, and those who moved abroad.[5] Some limitations of these data for the study of migrant characteristics are discussed subsequently.

From the published data, it is possible to examine several characteristics of non-white in-migrants to selected metropolitan areas (SMSA's) by place of residence in 1955—whether another SMSA or non-metropolitan area. Similar characteristics are also available for out-migrants from these SMSA's. Although most of the data we utilize refer to metropolitan areas rather than cities, the overwhelming majority of the Negro population in each of the SMSA's considered resides in the central city rather than the suburban ring. Data are presented for thirteen SMSA's. The central cities of these SMSA's comprise thirteen of the fifteen leading cities in Negro population in 1960. Los Angeles is omitted because the migration data for non-whites cannot be assumed to r sent Negroes, and Newark is omitted cause of its peculiar metropolitan statu part of the New York Standard Con dated Area.

NUMBER AND SOURCES OF MIGRANTS

The number of non-white (predominai Negro) in-migrants to each of the thirt SMSA's and their distribution by place origin (residence in 1955) are presen in Table 1. Negro in-migrants are m numerous in the northern and border are and least numerous in the southern area but in each SMSA the in-migrants duri the five-year period account for less th 10 per cent of the total 1960 Negro popu lation. In the northern and border SMSA' roughly half of the in-migrants are fro other metropolitan areas, as opposed t one-third or less in the southern SMSA's The predominant movement in a metro politan society is intermetropolitan rathe than rural-to-urban. Most Negro migrants to northern metropolitan areas have had considerable previous experience with urban living. Variation among SMSA's in the relative importance of in-migrants of metropolitan origin reflects the size and character of the Negro population in the immediate hinterland of the SMSA, the special location and functional base of the SMSA, and the extent to which the SMSA participates in a national intermetropolitan stream of migration.

Region of residence in 1955 is shown for non-white in-migrants in Table 2. About two-thirds of the in-migrants to northern and border SMSA's resided in the South in 1955. If we assume that all in-migrants from the North and West were residents of metropolitan areas in 1955, then it is possible to estimate the percentage of in-migrants from the South of

[4] U.S. Bureau of the Census, *U.S. Census of Population: 1960, Subject Reports, Mobility for Metropolitan Areas*, Final Report PC(2)-2C (Washington, D.C.: Government Printing Office, 1963), Introduction.

[5] For a more complete discussion of census procedures, see *ibid.*

[6] Following census usage, "northern areas" include, reading down the stub of Table 1, New York through St. Louis; "southern areas" include Washington through Houston. Philadelphia, St. Louis, Washington, and Baltimore are sometimes referred to as "border areas."

TABLE 1

NON-WHITE POPULATION BY MIGRATION STATUS, 1955–60
(IN THOUSANDS)

SMSA	Total Population Age 5+	Total In-Migrants	In-Migrants from: Other SMSA	In-Migrants from: Non-metropolitan Area	In-Migrants from: Abroad	Total Out-Migrants	Moved, 1955, Residence Not Reported
New York........	1,129.7	101.7	37.5	39.4	24.8	50.0	48.6
Chicago..........	777.8	68.1	28.2	35.9	4.1	39.7	59.4
Detroit...........	486.6	25.5	13.6	10.4	1.4	30.1	16.3
Cleveland.........	222.8	19.9	10.7	8.3	0.8	12.0	7.1
Philadelphia.......	589.9	37.4	18.3	15.8	3.4	22.6	18.5
St. Louis..........	252.7	14.1	5.5	7.9	0.6	14.7	10.9
Washington.......	429.5	44.5	18.4	20.6	5.5	22.2	21.6
Baltimore.........	328.3	21.6	8.8	11.3	1.5	13.2	12.8
New Orleans......	228.1	11.2	3.0	7.6	0.6	14.3	5.2
Atlanta...........	199.1	10.8	3.1	7.4	0.4	11.5	4.5
Birmingham......	190.1	8.9	3.0	5.7	0.3	15.3	1.8
Memphis.........	193.2	12.7	2.4	10.1	0.2	15.8	3.0
Houston..........	210.6	20.5	6.8	13.1	0.6	13.1	6.5

Source: U.S. Bureau of the Census, *U.S. Census of Population: 1960. Subject Reports. Mobility for Metropolitan Areas*, Final Report PC(2)-2C (Washington, D.C.: Government Printing Office, 1963), Tables 1 and 5.

TABLE 2

DISTRIBUTION OF NON-WHITE IN-MIGRANTS 1955–60 BY REGION OF ORIGIN
(PER CENT)

SMSA	Total	Same State: Other SMSA	Same State: Non-metropolitan Area	South*: Total	South*: Other SMSA†	South*: Non-metropolitan Area†	North*	West
New York........	100.0	1.2	1.4	76.6	26.8	49.8	17.9	2.9
Chicago..........	100.0	2.3	3.0	68.8	15.7	53.0	21.6	4.4
Detroit..........	100.0	6.4	1.8	64.4	22.8	41.6	23.4	3.9
Cleveland........	100.0	8.5	2.5	60.9	19.6	41.2	25.4	2.6
Philadelphia......	100.0	6.0	1.8	66.3	21.7	44 6	23 3	2.7
St. Louis.........	100.0	2.9	7.3	60.3	8.7	51.6	24.5	5 0
Washington.....	100.0	2.4	4.3	68.4	19.9	48.5	21.3	3.5
Baltimore........	100.0	1.7	7.0	71.3	22.1	49.2	17.3	2.6
New Orleans......	100.0	5.7	34.0	48.3	10.8	37.5	7.4	4.5
Atlanta..........	100.0	5.2	57.2	23.6	10.2	13.4	12.2	1.8
Birmingham......	100.0	8.5	57.2	13.9	5.5	8.4	18 6	1.8
Memphis.........	100.0	1.5	16.9	66.5	2.8	63.6	13.4	1.7
Houston.........	100.0	15.4	43.3	31.2	8.7	22.5	4.3	5.8

* Exclusive of "same state."

† Estimate based on the assumption that all in-migrants from the North and West were from metropolitan areas in these regions. The per cent of southern metropolitan origin is an underestimate while the per cent of southern non-metropolitan origin is an overestimate.

Source: U.S. Bureau of the Census, *Mobility for Metropolitan Areas, op. cit.*, Tables 1 and 3; U.S. Bureau of the Census, *U.S. Census of Population: 1960. Subject Reports Mobility for States and State Economic Areas*, Final Report PC(2)-2B (Washington, D.C.: Government Printing Office, 1963), Table 34.

metropolitan and non-metropolitan origin. The figures, shown in Table 2, underestimate the percentage of southern metropolitan origin and overestimate the percentage of southern non-metropolitan origin. The estimates show that about half or less of non-white in-migrants to northern and border SMSA's are of southern non-metropolitan origin, a much smaller proportion than might have been anticipated. Available data on state of origin of migrants are too detailed for presentation here, but they show the same general pattern found in all previous studies: lines of migration tend to flow in a straight south-north direction. Metropolitan areas in the northeastern states draw most of their non-white in-migrants from the Atlantic Coast states; metropolitan areas around the Great Lakes draw mainly from the South Central states, and so forth.

The southern metropolitan areas draw 80 per cent or more of their non-white in-migrants from elsewhere in the South (same state plus rest of South). In general, about two-thirds of the in-migrants are from non-metropolitan areas, and most of these are from the same or neighboring states.

Corresponding information on the destinations of non-white out-migrants is presented in Table 3. The great majority of out-migrants from northern and border SMSA's moved to the North and West, although many went to the South. Assuming that all out-migrants to the North and West went to metropolitan areas, we can estimate roughly that among out-migrants to the South, those going to non-metropolitan areas outnumber those going to metropolitan areas.

Of non-white out-migrants from southern SMSA's, most of those remaining in the South moved to non-metropolitan areas in the same or contiguous states. Of those moving to metropolitan areas, generally more than two-thirds went north.

The general picture which emerges from

TABLE 3

DISTRIBUTION OF NON-WHITE OUT-MIGRANTS, 1955–60 BY REGION OF DESTINATION (PER CENT)

SMSA	Total	Same or Contiguous States		South*			North*	West
		Other SMSA	Non-metropolitan Area	Total	Other SMSA†	Non-metropolitan Area†		
New York	100.0	26.5	14.9	37.6	18.1	19.5	9.0	12.0
Chicago	100.0	14.7	11.7	28.0	6.0	22.0	21.6	24.0
Detroit	100.0	21.6	8.1	30.1	11.8	18.3	22.7	17.6
Cleveland	100.0	23.7	15.8	24.8	11.1	13.7	19.4	16.2
Philadelphia	100.0	35.9	10.3	34.7	14.8	19.8	9.1	10.0
St. Louis	100.0	23.6	15.9	15.0	2.6	12.5	20.2	25.2
Washington	100.0	14.9	13.6	24.1	4.9	19.2	35.3	12.2
Baltimore	100.0	26.9	17.2	21.6	‡	‡	27.2	7.1
New Orleans	100.0	13.5	28.9	8.1	2.5	5.6	18.2	31.2
Atlanta	100.0	15.1	36.5	9.5	3.7	5.8	31.7	7.3
Birmingham	100.0	14.9	20.6	7.8	3.8	4.0	44.3	12.5
Memphis	100.0	8.5	22.9	6.9	1.7	5.2	44.2	17.5
Houston	100.0	24.4	33.0	5.7	0.6	5.1	7.4	29.4

* Exclusive of "same or contiguous states."

† Estimate based on the assumption that all out-migrants from the North and West were to metropolitan areas in these regions. As such, the per cent of southern metropolitan destination is an underestimate while the per cent of southern non-metropolitan destination is an overestimate.

‡ Not computed because of an inconsistency between the two source publications.

Source: U.S. Bureau of the Census, *Mobility for Metropolitan Areas*, *op. cit.*, Table 5; and U.S. Bureau of the Census, *Mobility for States . . .*, *op. cit.*, Table 36.

consideration of both in-and out-migration is one of a sizable intermetropolitan flow of population, both from southern metropolitan areas to northern metropolitan areas, and between northern metropolitan areas. The southern metropolitan areas have a large local exchange of Negro population—the majority of their in-migrants are from non-metropolitan areas of the same or nearby states while a moderate share of their out-migrants return to such areas. There is a substantial movement of Negroes from southern non-metropolitan places to the metropolitan areas of northern and border states, but such migrants are a minority of all in-migrants to these SMSA's. Of smaller magnitudes are the "return" movement from the North back to the South (to both metropolitan and non-metropolitan areas) and the movement between metropolitan areas of the South. The latter stream seems particularly underdeveloped.

LIMITATIONS OF MIGRATION DATA

Serious inadequacies in the census enumeration plague analysis of volume of migration and places of origin and destination. Data on characteristics of migrants are affected not only by these qualitative deficiencies but also by the nature of available tabulations. A brief discussion of the most important of these limitations may indicate the need for caution in interpreting the data.[7]

The Negro population has been underenumerated in recent censuses, and apparently it is young, mobile adults in big cities who are most likely to escape enumeration. In the nation as a whole, one estimate places the net underenumeration of Negro males in their early twenties at about 20 per cent.[8] If enumerated, many of these persons would probably be classified as migrants. Since these persons undoubtedly differ in socioeconomic composition from the enumerated population, characteristics of non-migrants as well as each migrant group are distorted by their omission, but the amount of bias cannot be determined.

A similar defect affecting both the numbers and characteristics of migrants and non-migrants is the presence in the final census tabulations of a large number of persons who are known to have changed residence during the 1955–60 period, but for whom place of residence in 1955 was not adequately reported. From published data on characteristics of migrants, it is possible to obtain as a residual characteristics for this "not-reported" group combined with migrants from abroad. Since the number of non-white migrants from abroad is small (Table 1), the residual group is mainly composed of "not-reported" cases. In age distribution, this group is more similar to non-migrants than to the youthful distribution of the migrants. The group has an overrepresentation of males, of residents of institutions and other group quarters, of individuals living alone, and of non-relatives of household heads. There is an extremely high incidence of "not reporting" on all of the socioeconomic characteristics for which such a category is tabulated. Apparently these are mainly persons for whom the "second stage" of the 1960 Census enumeration was inadequately carried out. We expect that more of these persons would be classified as "local movers" than as migrants if their migration status were known, but no precise determination can be made. In the absence of additional information, this group is excluded from further consideration and is omitted from our tabulations of both non-migrants and migrants. The numerically small group of in-migrants from abroad, composed in large part of returning military personnel, is also disregarded.

[7] For an official discussion of some of these limitations, see U.S. Bureau of the Census, *op. cit.*

[8] Reynolds Farley, "Negro Cohort Fertility" (unpublished Ph.D. dissertation, University of Chicago, 1964). A slightly lower figure is given by Donald S. Akers, "Estimating Net Census Undercount in 1960 Using Analytical Techniques" (unpublished paper presented at annual meetings of the Population Association of America, 1962).

Recorded answers to a query on place of residence five years before are bound to include many inaccuracies. Entirely aside from outright errors, there is a tendency to report the name of a nearby larger, better-known place instead of a smaller, lesser-known place. The problem of suburban residences being identified by reporting the name of the central city is so serious that efforts by the Census Bureau to tabulate 1955 metropolitan residences according to whether they were in the central city or suburban ring were abandoned.

Analysis of migrant characteristics is hampered because various types of migrants are confounded in the tabulations. The migration question was not restricted to the population in households.[9] Thus individual migration streams may be strongly affected by the location of a particular college, military installation, or other type of institution. One-third of non-white in-migrants from other metropolitan areas to the Atlanta area, for example, were residents of group quarters in 1960, reflecting the presence of both the Atlanta University complex and a large federal prison.

Migration connected with military service is an important type of "special-purpose" migration. Data showing the percentage of the male labor force which was in the armed forces at the time of the census are presented in Table 4. This means of identifying military-connected moves is incomplete, for it overlooks moves by men no longer in the services in 1960, as well as the associated moves of family members. Despite this undercounting, the data in Table 4 reveal the important role played by this type of "special-purpose" migration. Military bases in the Philadelphia, Washington, and Baltimore areas account for much Negro in-migration to those places. Out-migrants to military service

TABLE 4

NON-WHITE MALE LABOR FORCE IN ARMED FORCES, BY MIGRATION STATUS, 1955–60 (PER CENT)*

SMSA	Non-Migrants	In-Migrants			Out-Migrants			
		Total	From Other SMSA	From Non-metropolitan Area	Total	To Other SMSA: City	To Other SMSA: Ring	To Non-metropolitan Area
New York	0.4	3.5	5.0	2.0	28.0	12.5	40.9	41.5
Chicago	0.2	3.5	4.9	2.2	27.4	12.2	44.4	41.2
Detroit	0.2	4.7	5.7	3.4	26.0	9.3	47.7	48.0
Cleveland	0.1	1.1	1.6	0.5	20.1	5.7	40 2†	33.4†
Philadelphia	0.5	21.9	28.3	14.4	37.0	13.2	57.0	55.5
St. Louis	0.2	6.4	9.4	4.0	27.8	10.7	58.7†	38.5
Washington	1.2	16.5	22.9	10.8	32.2	16.4	49.8	43.1
Baltimore	0.5	16.3	25.9	9.0	31.3	17.2	46.9	37.7
New Orleans	0.3	4.6	9.2†	2.6	21.7	12.1	44.7†	21.9
Atlanta	0.1	3.3	8.5†	1.4	20.6	11.1	49.0†	20.7
Birmingham	0.1	0.7	0.9†	0.6	17.9	6.0	45.7†	30 0†
Memphis	0.0	3.7	10.0†	1.9	16.1	8.1	42.9†	17.8
Houston	0.1	0.5	1.2	0.1	23.9	13.0	56.2†	21.1

* In-migrants exclude persons who were abroad in 1955; out-migrants exclude persons who were abroad in 1960. Both groups exclude those "not reported" who may have been migrants.

† Based on fewer than 1,000 persons.

Source: U.S. Bureau of the Census, *Mobility for Metropolitan Areas*, *op. cit.*, Tables 4 and 6.

[9] "A household consists of all the persons who occupy a housing unit. . . . All persons who are not members of households are regarded as living in group quarters. Group quarters are living arrangements for institutional inmates or for other groups containing five or more persons unrelated to the person in charge" (U.S. Bureau of the Census, *op. cit.*, pp. xii–xiii).

are drawn from all metropolitan areas. Particularly striking is the role of the military in accounting for what might appear to be suburbanization: about half of the men in the labor force moving from our 13 SMSA's to the suburban ring of another metropolitan area were in the armed forces. Military-connected migration also accounts for much of the movement from metropolitan to non-metropolitan places.

"Special-purpose" migrants make up a substantial share of out-migrants from each SMSA, but are a large share of in-migrants only in those SMSA's containing a large college, military base, prison, or other institution. The presence of these migrants is reflected in varying degrees in measures of the characteristics of in- and out-migrants. Measures based on age and sex distribution, since they derive from classifications of the total population, are affected by all types of migrants. Our measure of educational attainment is based on the population over twenty-five years of age. Most students and members of the armed forces are probably too young to appear in the education statistics. Unfortunately, so are many migrants; in each SMSA about 40 per cent of in-migrants, 35 per cent of out-migrants, and 20 per cent of non-migrants age fifteen and over are between fifteen and twenty-four years of age and are omitted from tabulations of educational attainment. Our measure of occupational status is based upon employed civilian males age fourteen and over. Many college students are not in the labor force; military personnel are not part of the civilian labor force; and most institutional population is not in the labor force.

The various limitations of the 1955–60 migration data require the exercise of considerable caution in interpreting data showing the number or characteristics of migrants. Deficiencies in the count of migrants seem particularly serious. For instance, the data on in- and out-migration presented in Table 1 can be utilized to obtain estimates of net migration, but the resulting figures are suspiciously low. There are too many possible errors for such data to be used as evidence, say, that non-white migration to large cities during the last half of the 1950–60 decade was at a slower rate than during the first half. Data on migrant characteristics are biased by the deficiencies in enumeration and confounded by the presence of "special-purpose" migrants, but we believe they are still useful for the gross types of comparisons we shall make. Interpretation of the characteristics of individual migration streams, however, must take careful note of the full range of possible limitations in the data.

CHARACTERISTICS OF NEGRO MIGRANTS AND NON-MIGRANTS

As compared to non-migrants, the in-migrants are better educated and are more likely to be engaged in white-collar occupations (Tables 5 and 6). Two subgroups of Negro in-migrants may be distinguished, those moving from other metropolitan areas and those of non-metropolitan origin. In-migrants of non-metropolitan origin are much lower in educational and occupational status than migrants from other metropolitan areas, but are rather similar to Negro non-migrants. In-migrants from other SMSA's, on the other hand, are particularly distinctive, being of substantially higher educational and occupational status than either in-migrants of non-metropolitan origin or non-migrants. Thus, the relatively high status of the total in-migrant group is due to the combination of the high-status intermetropolitan stream with the lower status (partially rural-to-urban) stream of non-metropolitan origin.

Compared to in-migrants, out-migrants tend to be of higher educational and occupational status. In fact, out-migrants resemble in-migrants from other metropolitan areas in these characteristics more than those of non-metropolitan origin. This is not surprising since out-migrants from one SMSA generally become in-migrants to

TABLE 5

NON-WHITES COMPLETING ONE OR MORE YEARS OF HIGH SCHOOL BY MIGRATION STATUS, 1955–60 (PER CENT)*

SMSA	NON-MIGRANTS	TOTAL IN-MIGRANTS	IN-MIGRANTS FROM		TOTAL OUT-MIGRANTS
			Other SMSA	Non-metropolitan Area	
New York...	53.4	61.4	66.1	55.4	63.4
Chicago.....	51.6	55.6	64.5	46.0	63.5
Detroit.....	51.4	59.8	65.5	50.0	59.9
Cleveland....	54.2	63.5	66.6	58.2	62.3
Philadelphia...	48.5	59.9	66.0	51.2	64.3
St. Louis......	42.1	50.2	63.2	38.4	59.7
Washington..	53.6	66.5	75.1	57.3	66.4
Baltimore....	39.6	53.9	61.2	47.6	58.5
New Orleans..	30.5	39.6	56.0	30.6	50.4
Atlanta......	37.2	43.2	56.1	35.8	51.1
Birmingham..	34.4	40.0	55.8	29.5	54.5
Memphis........	30.7	31.5	52.9	24 9	46.2
Houston.........	47.1	54.3	63.3	47.9	57.3

* In-migrants exclude persons who were abroad in 1955; out-migrants exclude persons who were abroad in 1960. Both groups exclude those "not reported" who may have been migrants.

Source: U.S. Bureau of the Census, *Mobility for Metropolitan Areas, op. cit.*, Tables 4 and 5.

TABLE 6

NON-WHITE EMPLOYED MALES IN WHITE-COLLAR OCCUPATIONS BY MIGRATION STATUS, 1955–60 (PER CENT)*

SMSA	NON-MIGRANTS	TOTAL IN-MIGRANTS	IN-MIGRANTS FROM		TOTAL OUT-MIGRANTS
			Other SMSA	Non-metropolitan Area	
New York....	25.8	25.5	32.3	18.7	31.7
Chicago......	20.7	21.2	28.7	15.1	30.2
Detroit........	14.3	24.7	31.3	15.1	18.4
Cleveland....	15.9	17.9	22.0	13.2	24.2
Philadelphia...	18.1	21.3	28.6	14.0	26.6
St. Louis......	16.7	21.0	28.4	14.7	25.8
Washington..	27.4	29.3	40.0	21.1	37.6
Baltimore....	15.8	22.7	36.1	13.9	21.5
New Orleans..	12.8	10.9	20.7†	7.1	24.9
Atlanta.......	12.1	14.7	25.1†	11.1	25.7
Birmingham..	8.5	9.7	14.1†	7.4	17.6
Memphis......	11.1	14.0	24.3†	11.6	18.0
Houston.........	12.8	11.0	19.0	7.0	22.8

* In-migrants exclude persons who were abroad in 1955; out-migrants exclude persons who were abroad in 1960. Both groups exclude those "not reported" who may have been migrants.

† Based on fewer than 1,000 persons.

Source: U.S. Bureau of the Census, *Mobility for Metropolitan Areas, op. cit.*, Tables 4 and 6.

another SMSA. Once Negroes become metropolitan residents, the tendency is to remain in metropolitan areas and to move, if at all, within national streams of inter-metropolitan migration.

On the basis of these data on the volume and characteristics of in- and out-migration, it is possible to make some inferences concerning the net effect of migration upon the socioeconomic status of the resident Negro population. Both in-migration and out-migration must be considered. The net effect upon the resident population is a function both of the socioeconomic composition of each migration stream and the relative volume of in- and out-migration. Since net migration is small both absolutely and relative to the gross movement, the impact of migration, whether favorable or unfavorable, is slight.

The situation of the southern metropolitan areas is clear—migration has had a deleterious effect upon the educational and occupational advancement of the resident Negro population. The depressing effect of an out-migration which is high-status is not counteracted by in-migration because: (1) it is of lesser volume; and (2) its status is lower, being only slightly better than non-migrants, owing to the predominance of in-migrants of non-metropolitan origin.

The situation is more ambiguous in the northern and border SMSA's. Although each area is characterized by a high-status out-migration, the countering effect of in-migration varies both in volume and status. Hence, the net impact of migration on the socioeconomic status of the resident Negro population is sometimes favorable and sometimes unfavorable.

The various migration streams differ in age composition, and we may inquire whether these differences account for the observed differences among streams in educational and occupational status. Since young people are generally characterized by higher average levels of educational attainment than older people, it is possible that the younger average age of the in-migrants is responsible for their higher educational level relative to non-migrants. Measures of educational attainment controlled for age, presented in Table 7, are very similar for the total in-migrant group and non-migrants, indicating that the favored position of in-migrants is largely accounted for by their favorable age composition. The age-standardized measures, however, still reveal a large difference in educational level between migrants from metropolitan areas and those from non-metropolitan areas. In-migrants from other SMSA's are of uniformly higher status than those of non-metropolitan origin or the non-migrant population, while migrants from non-metropolitan areas are of lower status than non-migrants. In sum, in-migrants of metropolitan origin are clearly of higher educational status than non-migrants, whether the comparison is with all non-migrants or with non-migrants of the same age. Migrants of non-metropolitan origin, though similar in educational status to all non-migrants, are of lower status than non-migrants if the comparison is restricted to non-migrants at the same ages.

NEGRO MIGRANTS AND WHITE RESIDENTS

We have considered the socioeconomic status of in-migrant non-whites relative to the non-white population, but have not yet indicated their status relative to the resident white population. The percentages of high-school graduates among the white populations of the thirteen *cities* are presented in Table 8, where they can be compared with the percentages among non-white in-migrants to cities. In their levels of educational attainment, non-white in-migrants to northern and border cities resemble the resident white population, while that segment of in-migrants coming from other metropolitan areas is of higher status than the whites. Controlling for differences in age composition tends to lower the percentages for non-whites and raise those for whites. On an age-standardized basis, the non-white in-migrants to northern and border cities are slightly below the resident white popula-

TABLE 7

AGE-STANDARDIZED PER CENT COMPLETING ONE OR MORE YEARS OF HIGH SCHOOL, BY MIGRATION STATUS, 1955–60 FOR NON-WHITES*

SMSA	NON-MIGRANTS	TOTAL IN-MIGRANTS	IN-MIGRANTS FROM	
			Other SMSA	Non-metropolitan Area
New York	54.3	53.2	57.7	47.4
Chicago	52.5	48.4	55.8	40.3
Detroit	51.9	51.9	55.9	44.7
Cleveland	55.0	55.3	58.4	50.3
Philadelphia	49.3	49.5	54.9	42.0
St. Louis	42.7	41.3	49.8	32.9
Washington	54.6	56.8	64.0	49.2
Baltimore	40.2	45.2	51.5	39.7
New Orleans	31.0	31.7	42.4	25.2
Atlanta	37.8	35.8	45.8	29.9
Birmingham	34.9	32.4	42.8	24.8
Memphis	31.1	26.9	40.9	21.9
Houston	47.9	47.3	53.2	42.8

* Standardized by the indirect method, using the age-sex-specific percentages for the non-white population of each SMSA as the standard set of rates.

TABLE 8

ACTUAL AND AGE-STANDARDIZED PER CENT HIGH SCHOOL GRADUATES FOR NON-WHITE IN-MIGRANTS TO CITIES, 1955–60, AND TOTAL WHITE POPULATION OF CITIES, 1960*

CITY	ACTUAL			AGE-STANDARDIZED		
	White, City Total	Non-white In-Migrants		White, City Total	Non-white In-Migrants	
		Total	From Other SMSA		Total	From Other SMSA
New York	38.3	39.0	44.3	39.1	30.4	34.6
Chicago	36.8	32.3	39.7	38.2	25.8	31.2
Detroit	37.1	33.8	41.8	40.5	28.9	35.0
Cleveland	30.9	36.9	40.4	32.0	30.1	32.9
Philadelphia	32.9	35.1	38.8	34.6	28.4	31.3
St. Louis	27.1	30.7	41.9	29.9	25.1	32.6
Washington	61.1	45.1	54.5	71.4	45.1	54.3
Baltimore	31.8	29.4	37.6	33.7	24.6	31.3
New Orleans	42.1	21.9	34.6	44.0	19.0	28.4
Atlanta	50.7	26.0	34.7	52.9	24.0	31.2
Birmingham	46.4	21.9	31.0	47.2	20.3	28.1
Memphis	53.1	16 0	31.7	53.7	15.3	28.7
Houston	50.4	32.3	40.6	50.6	29.3	35.9

* Standardized by the indirect method, using the age-sex-specific percentages for the white population for each SMSA as the standard set of rates to be applied to the age-sex composition of non-white in-migrants and the total white population of each city.

tion in educational status, and in-migrants of metropolitan origin are very much like the whites.

In the southern cities, the status comparisons have a somewhat different pattern. Both the total non-white in-migrants and those of metropolitan origin are substantially below the white population in educational attainment levels, and age-standardization tends to widen this gap. Educational levels among the non-white in-migrants from metropolitan areas, however, are similar to those among the same segment of in-migrants to northern and border cities. The total in-migrant group remains of lower educational status, whatever the comparison, because of the preponderant influence in the southern cities of the low-status in-migrants of non-metropolitan origin.

If these comparisons between non-white in-migrants and resident whites were made for the total metropolitan population rather than the city population, the status levels among whites would generally be raised a bit, while the figures for non-whites would be little affected. If these comparisons were made for occupational levels rather than educational levels, all of the non-white groups would clearly be of lower status. Nevertheless, the figures we have presented demonstrate that the in-migrant non-whites, at least in northern and border cities, resemble in educational levels the whites among whom they live.

CHANGING CHARACTER OF NEGRO MIGRATION

The data for 1955–60 show Negro in-migrants to be of substantially higher socioeconomic status, on the average, than the resident Negro population in a number of large metropolitan areas. These findings are at variance with most previous discussions of Negro migration. Has the character of Negro migration been changing? Empirical data for earlier years are scanty, but limited material for the 1935–40 period and the 1940–50 decade shed some additional light on the matter.

From the 1940 Census, information is available on migrants during the 1935–40 period, based on comparison of place of residence in 1935 and 1940. These data are very similar to the migration data for 1955–60, but the published data are not in a form to permit a similar analysis. Special tabulations, however, were used by Freedman in an analysis of in-migrants to Chicago. Whereas

> at least with reference to the period 1935–40 the [white] migrants tended to be relatively well-educated young adults on the threshold of their productive careers, ready and willing to work and able to find employment, . . . the Negro migrants of the present period and the earlier foreign migrants are predominantly persons of low educational status, without capital or financial reserves, unskilled and ready to enter the labor market at the bottom of the occupational ladder.[10]

Rough comparisons clearly indicate an upgrading of Negro in-migrants relative to non-migrants in Chicago between 1935–40 and 1955–60.

Some studies have utilized census data for 1940 and 1950 to develop estimates of net migration by educational status.[11] These data have shown that the net impact of migration during tthe 1940's was to retard improvement in the educational status of metropolitan Negro populations. However, no inferences can be made from such data regarding the educational level of in- or out-migrants. Although such findings are not inconsistent with an assumption that in-migrants were of somewhat lower status than non-migrants, any pattern of net migration can be produced

[10] Ronald Freedman, *Recent Migration to Chicago* (Chicago: University of Chicago Press, 1950), p. 210.

[11] Otis Dudley Duncan and Beverly Duncan, *The Negro Population of Chicago: A Study of Residential Succession* (Chicago: University of Chicago Press, 1957), chap. iv; C. Horace Hamilton, "Educational Selectivity of Net Migration from the South," *Social Forces*, Vol. XXXVIII (October, 1959); Karl E. Taeuber and Alma F. Taeuber, *Negroes in Cities: Residential Segregation and Neighborhood Change* (Chicago: Aldine Press [in press]), chap. vi.

by a wide variety of numbers of in- and out-migrants at each educational level.

Other relevant data for earlier periods exist, but they are not comparable in nature or generality to those for 1955–60.[12] Indirect evidence that the character of interregional migration has been changing is available from a 1960 Census tabulation showing educational attainment, by age and sex, cross-classified by region of birth and region of residence. A comparison between median years of school completed for non-whites born and living in the North and non-whites born in the South, living in the North, is presented in Table 9. If we assume that most interregional migrants move before the age of thirty, we can regard the classification of persons by age as a classification also by time of move. Non-whites born in the South and living in the North who are in the younger age brackets must have moved during the past decade or two, whereas those in the older age groups are probably persons who migrated before the depression. The recent migrants have median educational levels very similar to the northern-born non-whites, whereas the older migrants are less well educated than the non-migrants.

Despite these pieces of evidence, any discussion of past patterns of Negro migration must be largely speculative. Very likely a high-status intermetropolitan stream of Negro migrants always existed, but its relative importance has increased substantially in recent years owing to the rapid urbanization of the Negro population. We have found that it is in-migrants of non-metropolitan origin who most nearly resemble the stereotype of the socioeconomically depressed migrant. It seems reasonable that as this component declines and the intermetropolitan component increases in relative importance, the status of the total in-migrant group would rise.

As the character of the Negro population has changed from that of a disadvantaged rural population to a largely metropolitan population of rising social and economic status, Negro migration should increasingly manifest patterns similar to those found among the white population. This, indeed, is what is revealed by analysis of census

[12] Extensive bibliographies are given in Woodson, *op. cit.*, and in Kennedy, *op. cit.* For additional discussion and analysis of the 1935–40 data, see Donald J. Bogue and Margaret Jarman Hagood, *Subregional Migration in the United States, 1935–40*, Volume II: *Differential Migration in the Corn and Cotton Belts* (Oxford, Ohio: Scripps Foundation, Miami University, 1954).

TABLE 9

MEDIAN YEARS OF SCHOOL COMPLETED, BY REGION OF BIRTH, FOR NON-WHITES RESIDING IN THE NORTH AND WEST, 1960

Age and Sex	Median Years of School Completed		Born in South as Per Cent of Those Born in North
	Born in North*	Born in South	
Male:			
14–15	8.3	8.1	97.6
16–17	10.0	10.0	100.0
18–19	10.8	10.9	100.9
20–21	11.7	11.6	99.1
22–24	11.8	11.4	96.6
25–29	11.8	10.8	91.5
30–34	11.3	9.8	86.7
35–44	11.1	8.9	80.2
45–54	9.2	8.0	87.0
55–64	8.4	6.8	81.0
65–74	7.8	5.6	71.8
75+	6.9	4.5	65.2
Female:			
14–15	8.6	8.4	97.7
16–17	10.3	10.2	99.0
18–19	11.5	11.3	98.3
20–21	12.1	11.9	98.3
22–24	12.1	11.8	97.5
25–29	12.1	11.3	93.4
30–34	12.0	10.7	89.2
35–44	11.5	9.8	85.2
45–54	9.9	8.5	85.9
55–64	8.6	7.4	86.0
65–74	8.2	6.5	79.3
75+	7.7	5.6	72.7

* "North" includes census regions Northeast and North Central. Data for non-whites born in the West and foreign-born are omitted because of the presence of large numbers of other races in these groups.

Source: U.S. Bureau of the Census, *U.S. Census of Population: 1960. Subjects Reports. Educational Attainment*, Final Report PC(2)-5B (Washington, D.C.: Government Printing Office, 1963), Table 3.

data on migration, 1955–60.[18] Among both whites and Negroes, there is a substantial intermetropolitan movement of persons of relatively high socioeconomic status. Both white and Negro in-migrants to metropolitan areas, despite the presence of some groups of depressed status among them, are, on the average, of higher socioeconomic status than the non-migrant population.

[18] An analysis of white migration similar to that reported here for Negroes is given in Karl E. Taeuber and Alma F. Taeuber, "White Migration and Socio-economic Differences between Cities and Suburbs," *American Sociological Review*, Vol. XXIX (October, 1964).

We may tentatively conclude that Negro in-migrants in the past were predominantly of lower status than the Negro population already resident in metropolitan areas. Older descriptions of migration patterns, however, are no longer applicable. There is a large and increasingly important high-status intermetropolitan migration in the total movement of Negro population.

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Some Current Conceptualizations in the Field of Conflict

Jessie Bernard

ABSTRACT

There has been a remarkable revival of interest in the field of conflict in the last decade, due in part to the work of Von Neumann and Morgenstern on the theory of games and the use of this work by the Department of Defense. Much of this work has a psychological orientation. There are differences in emphasis between the rationalistic (strategic) and the non-rationalistic approaches. Schelling has transformed classic game theory into a theory of interaction. Rapoport rejects the game approach as leading to absurd results. Richardson's collective-behavior model is espoused by both Rapoport and Boulding, but it is used differently by them. The bureaucratization of violence has been commented on by Hannah Arendt. The theory of non-violence has not been cultivated; but Schelling's concept of the co-ordination game may prove useful. Sociological research tends to be empirical rather than theoretical, but it now applies more sophisticated techniques than in the past. Experimental studies, based on laboratory "games" and simulation studies, combine theoretical and empirical approaches. Some of these games are as useful as projective tests as they are as techniques for studying conflict.

I. RENAISSANCE OF CONFLICT THEORY

Conflict theory, after burgeoning in the nineteenth and early twentieth centuries, had fallen into desuetude by the middle of the twentieth century. Very little new thinking was being devoted to it. Then, all of a sudden in the 1950's it began to attract the best brains in the country, and since then it has leaped ahead at an accelerated pace. There is nothing occult or mysterious about this great spurt. It has resulted in part from the stimulation generated by the Von Neumann–Morgenstern volume on *Theory of Games and Economic Behavior* in 1944. But this stimulation alone would not have been enough. In addition, the Department of Defense discovered the value of civilian brains. Men whose thinking would have been just as brilliant, but more neglected, on their own campuses were now assigned life-or-death problems at Rand or at academic centers for international studies. Understandably, therefore, much of the thinking has been focused on policy problems. But much has also spilled over into ordinary everyday problems. It would not be an exaggeration to say that today much of the best thinking in all the social sciences is directed to conflict theory, with serendipitous profit to related areas.

Current thinking in the field of conflict is psychologically oriented rather than, as in the nineteenth century, sociologically oriented.[1] The major emphasis in this paper, therefore, will be on the new psychologically oriented models, especially on strategic ones.

II. RATIONALISTIC AND NON-RATIONALISTIC MODELS

The intellectual climate of the West seems to show oscillations between strong emphasis on the rationalistic aspects of human nature and strong emphasis on the non-rationalistic aspects. The late eighteenth century has come to be known as the

[1] The contributions of sociologists are, in fact, conspicuously less than those of other social and behavioral scientists. Kathleen Archibald, commenting that "a sociology of conflict is being constructed without benefit of sociologists" ("Social Science Approaches to Peace: Problems and Issues," *Social Problems*, XI [Summer, 1963], 98), explains this anomaly as due, among other reasons, to fear of estrangement from favored variables and concepts; to reluctance to commit oneself to an interdisciplinary approach; and to awareness of the immensity, complexity, and "messiness" of the variables involved.

Age of Reason because man was viewed as essentially rational. Both economics and political theory reflected this view. The rational man maximized his gains, minimized his losses. When or if all men behaved this way, the result was an efficient economy. In the nineteenth century there was a swing away from the emphasis on reason toward emphasis on emotions. The Romantic Movement in literature played up feeling rather than reason. In psychology, instinct came to overshadow reason in the interpretation of human behavior. In sociology, Sumner, who emphasized the non-rational, overshadowed Ward, who emphasized the rational. The apotheosis of the non-rationalistic came with Freud who revealed such—from the rationalistic point of view—irrational behavior as masochism and the death instinct.

If such swings in thought were merely academic fashions, they would still be interesting, but not critical. Actually they have important ramifications. For the way one conceptualizes the world is itself a powerful force in shaping the world. In one sense all the researcher does is examine what goes on; actually what he finds is influenced by what he looks for and what he looks for is influenced by the theory—implicit or explicit—with which he begins. This is true of conflict. It is therefore important to know how conflict is conceptualized and to see some of the "conflicts" now taking place among these conceptualizations.

In the 1940's the non-rationalistic approach to conflict took the form of an emphasis on "tensions." Hostile stereotypes, aggression, misunderstanding were viewed as basic variables. UNESCO sponsored some of the best work, especially by social psychologists, in this area; much of it still classic.[2]

The modern form of the rationalistic approach is the so-called game-theory approach referred to above.

CHALLENGE: THE THEORY OF GAMES OF STRATEGY

The original version of this theory was strictly, indeed formidably, rationalistic. It presupposed players who not only knew what they wanted, but who had an order of preference for the things they wanted. These players were trying to maximize something and to minimize the costs. But, unlike the early nineteenth century decision-makers, they made their decisions in situations in which they had only some, but not complete, control; they had opponents as rational as they who also had some control. The outcome was a result of the way both behaved. Therefore each had to act in such a way as to take the other's behavior into account and, more to the point, knowing that his own behavior was being taken into account by the other also. In classic form, the gains of one player were exactly matched by the losses of the other.

In some situations there was no advantage in secrecy; knowing what your opponent was going to do was of no help.[3] In others, however, it was important to keep your own plans secret, so secret, in fact, that you did not know yourself what you were going to do until some device rigged to turn up certain odds (which the theory itself told one how to compute) told you. The genius involved in conceptualizing how to arrive at these odds was so dazzling that it literally enthralled all who came in contact with it. The most brilliant minds in mathematics, in political science, and in psychology, as well as in economics, got into the act, looking for ways to use this exciting new model.

It soon became evident, however, that there were very few situations in real life that the model fit or, perhaps, that fit the

[2] For a review of the so-called Tension Project and the research that grew out of it, see the International Sociological Association, *The Nature of Conflict* (Paris: UNESCO, 1957), pp. 9–32.

[3] Such situations occur when the maximum gain for one player is the minimum loss for the other.

model.[4] The theater of the absurd may have its devotees, but scientists cannot bear a theory that leads to absurdities. Certain games did precisely that. They led only to disaster if the players acted rationally. One such game is called "The Prisoner's Dilemma."[5]

In 1957, Luce and Raiffa, in a book itself a tremendous tour de force, "tried . . . to indicate the major intuitive and empirical objections that social scientists could or have raised against the theory—objections not to the mathematics, but to the applicability of this mathematics to empirical problems."[6] Their aim was "to warn and to challenge the reader at just those points where the theory is conceptually weak." They devoted eight chapters to "the conceptually difficult and not fully satisfactory theory of general games—those which either have more than two players or are not zero-sum or both."

They saw the failure of non-zero-sum games to behave like zero-sum games as "pathological,"[7] a term obviously figurative in intent, but one that reveals their point of view. They sought ways of dealing with co-operative games that involved manipulating the games until they were transformed into non-co-operative games.[8] They were, in effect, licked. They gave up.

There have been, however, several responses to the challenge. One may be called the neo-strategic approach; one the non-strategic approach; and one the anti-strategic approach. The first approach is a modification of the rationalistic model; the second and third approaches are, in a way, a swing back to non-rationalistic models.

RESPONSES: SCHELLING'S NEO-STRATEGIC MODEL

The biggest "news" in the theory of conflict in the last few years is, perhaps, the revolutionary breakthrough of Thomas C. Schelling in the conceptualization of game theory and its transformation into a theory of social interaction.[9] Two of his major contributions are his emphasis on communication rather than secrecy and his conceptualization of the co-ordination game. In his hands game theory, which began as, in effect, a theory of protection against interaction,[10] has converged with the social-interactionist school. Schelling and Goffman represent the meeting point.[11]

The whole theory has been recast by his work, which transforms it into something quite different from the archetypical zero-sum, non-co-operative model. Schelling no longer viewed co-operative games—or, as he calls them, mixed-motive games—as deviations from the classic game of strategy, as conceptually "pathological." The intrusion of psychological factors was no longer a bothersome fact that interfered with elegant and definitive mathematical formulations; they were of the essence.[12] The psychologi-

[4] John C. Harsanyi is of the opinion that war is the only social situation that fits the two-person, constant-sum game ("Some Social Science Implications of a New Approach to Game Theory" [paper submitted to the Conference on Conceptual and Experimental Analyses of Strategic Interaction and Conflict, Berkeley, Calif., February, 1964], p. 9).

[5] This game appears in almost all discussions of game theory. A good statement may be found in R. Duncan Luce and Howard Raiffa, *Games and Decisions, Introduction and Critical Survey* (New York: John Wiley & Sons, 1957), pp. 95, 97.

[6] *Ibid.*, p. viii.

[7] *Ibid.*, p. 111.

[8] *Ibid.*, pp. 153–54.

[9] *The Strategy of Conflict* (Cambridge, Mass.: Harvard University Press, 1960).

[10] It is probably not accidental that game theory and computer theory came from the same brain. In a game with a so-called saddle-point, there was no need for interaction; in a game without a saddle-point, one could program the mixed strategy and let a machine play the game out.

[11] Schelling found Goffman's work representative of the kind of behavior his own model tried to illuminate.

[12] One is reminded of the way Karl Marx turned classical economic theory inside out, viewing the business cycle not as an occasional interference with a normal system, but as an intrinsic aspect of the functioning of the system.

cal factors were not frustrating evidence that the players were not robots but challenging evidence that they were human beings. Not secrecy but communication was emphasized; not uncertainty, but certainty; not freedom but commitment. Schelling's model was almost a mirror image of the classic game.

But most important of all was the effect his model had in transforming game theory from a static to a dynamic model. The players no longer accepted the payoff matrix as fixed. They tried to change the game; that is, they tried to change the opponent's payoffs and, short of that, his strategy. The object of strategy was no longer to make the best of a difficult situation but to change the situation by manipulating the opponent. Schelling analyzed how strategic moves could do this.

Schelling himself summarized the difference between his point of view and the classic viewpoint as follows:

> There is probably no contrast more striking, in the comparison of the mixed-motive and the pure-conflict (zero-sum) game, than the significance of having one's own strategy found out and appreciated by the opponent. Hardly anything captures the spirit of the zero-sum game quite so much as the importance of "not being found out" and of employing a mode of decision that is proof against deductive anticipation by the other player.* Hardly anything epitomizes strategic behavior in the mixed-motive game so much as the advantage of being able to adapt a mode of behavior that the other party will take for granted. . . .
>
> Another paradox of mixed-motive games is that genuine ignorance can be an advantage to a player if it is recognized and taken into account by an opponent. This paradox . . . has no counterpart in zero-sum games. And, similarly, in a zero-sum game between rational players with full information it can never be an advantage to move first. . . . In the mixed-motive game it certainly can.[13]

* Concerning this point, Von Neumann and Morgenstern say: "We have placed considerations concerning the danger of having one's strategy found out by the opponent into an absolutely central position."

It is difficult to convey the importance of Schelling's reorientation of game theory. Many of the things that were viewed as bothersome interferences with classic game theory became central. In Schelling's hands, game theory—if, indeed, it can still be so called[14]—became a theory of interaction par excellence. The players are recognizable human beings who act like people we know who have other things to do than play poker or chess. They are not paranoid. They are still trying to do the best they can; but they have a little confidence in other people's common sense, at least to the extent that they are not all Patrick Henrys with only the alternatives of liberty or death.[15]

Equally, if indeed not more important is Schelling's concept of the co-ordination game, based on tacit communication or expectations, and the salient solution. Neither has been widely exploited.[16] They seem to have special relevance for sociology.

RESPONSE: NON-STRATEGIC MODELS OF CONFLICT

The work of Lewis F. Richardson on arms races has been especially stimulating

[13] Schelling, *op. cit.*, pp. 160–61.

[14] Schelling had worked out his models before adopting the game-theory form of presentation; they are not dependent on game theory.

[15] A game in which the alternatives are such as to result in defeat for one or death for both players fits the "chicken" or "paper tiger" or "brinkman" model (see Schelling, "Uncertainty, Brinkmanship, and the Game of 'Chicken'" [paper for the Conference on Conceptual and Experimental Analyses of Strategic Interaction and Conflict, Berkeley, Calif., February, 1964]).

[16] Mary Wilkov, a graduate student at Pennsylvania State University, used the co-ordination model in an experiment on husbands and wives. Choosing gifts without communication, they could win if their choices coincided (common interest), but the choices were between gifts for themselves and gifts for their spouses (conflicting interest). The salient solution was one in which more than half of the gifts were for the wives.

to Anatol Rapoport and Kenneth Boulding. But they do different things with it. In Rapoport's hands it is a model for the behavior of great social systems; it is a theory of escalation and de-escalation, a theory of collective behavior based on "organismic general system theory" in which "certain aggregates of organisms are treated as living systems of higher order."[17] Its ideological basis is the belief that "freedom or indeterminacy of the particles composing a large aggregate is irrelevant to the fate of the aggregate."[18] It is a model in which individuals find themselves caught up in forces which, once started, operate according to their own laws.[19]

Two equations with six parameters basically define the situation. The six parameters (two for each player) are: the degree of mutual dependence of the parties; grievances or good will, according to whether the parameters are negative or positive; and brakes on action and interaction. As applied to an arms race, the equations state that "the rates of armament expenditure increases will depend positively on the expenditure level of the rival . . . , negatively on one's own expenditures . . . , and positively on the 'permanent grievances.'" Rapoport is fully aware of the inadequacies of his model; but he notes also that it gives results conformable to common sense.[20] Because the decisive variables include attitudes of friendliness or hostility, this model may be viewed as a return to the non-rationalistic conceptualization of conflict; it involves emotional interaction.

In Boulding's hands this conceptualization remains; but in addition he sees nations as rational and self-conscious decision-makers, with scales of preference,[21] rather than as living systems of smaller particles. He may therefore be said to accept both a non-rationalistic and a rationalistic approach. He may even be said to combine the two in a semirationalistic approach, illustrating some possible forms of a particular equilibrium function of the Richardson process model in his amusing paradigm of the "yogi," "saint," "publican," "regenerate sinner," and "devil."[22]

In his rationalistic stance, Boulding's

[17] Anatol Rapoport, *Fights, Games, and Debates* (Ann Arbor: University of Michigan, 1960), p. xi (see Lewis F. Richardson, *Arms and Insecurity: A Mathematical Study of the Causes and Origins of War*, ed. by Nicolas Rashevsky and Ernesto Trucco [Chicago: Quadrangle Books, 1960], and his *Statistics of Deadly Quarrels*, ed. Quincy Wright and C. C. Lienau [Chicago: Quadrangle Books, 1960]).

[18] Rapoport, *op. cit.*, p. xii.

[19] This model was found useful in analyzing husband-wife relationships also (see Jessie Bernard, "Marital Adjustments," *Handbook on Marriage and Family*, ed. H. T. Christensen [Chicago: Rand McNally & Co., 1964]).

[20] Rapoport, *op. cit.*, p. 29.

[21] Kenneth Boulding, *Conflict and Defense* (New York: Harper & Row, 1962).

[22] In this paradigm, each party has a set of strategies which varies along a continuum from "friendly" to "hostile," represented geometrically by a pair of co-ordinates, one for each party, which intersect at indifference. The "yogi" pattern avoids interaction by invariably selecting a "friendly" strategy, no matter what the other party offers. This pattern is a straight line—at whatever distance—on the "friendly" side of the indifference point and parallels the ordinates. The "saint" pattern also remains on the "friendly" side and returns "friendliness" for either "friendliness" or "hostility," but in varying degrees. Such a strategy means that the "saint" can impose a friendly reaction on the "publican," whose strategy is one of returning "friendliness" for "friendliness" and "hostility" for "hostility." The "regenerate sinner" does not have this strategic advantage because, unlike the "saint," he begins with some "hostility" and therefore evokes it in the "publican." The "devil" pattern returns "hostility" for both "friendliness" and "hostility." Unlike the "publican," the "devil" is not vulnerable to the "saint's" strategy. When the "devil" and the "publican" confront one another, they may never reach an equilibrium but become increasingly hostile to one another (*ibid.*, pp. 31–32). This paradigm illustrates how much less powerful the Richardson model is than the game-theory model, especially Schelling's version. It assumes rather robot-like players who react to one another with very limited strategies and little resourcefulness, who are not rational in the strategic sense, and who seem more like the kind of people the tension approach had in mind. Boulding is not too insistent on his paradigm.

model, borrowed from economists' analyses, takes on the characteristics of a bargaining model, removing it from the Richardson conceptualization—in terms of national mood or collective attitudes—and taking it in the direction of the strategic model.[23]

RESPONSE: ANTI-RATIONALISTIC MODELS

One model is not only non-strategic in orientation but actually anti-rationalistic. It is one which Anatol Rapoport calls the "debate" model.[24] Here the players try to get rid of the difference which is at the base of the conflict. In this connection Rapoport discusses (1) brainwashing, based on conditioned-response theory, (2) explaining away the hostile image, based on Freudian theory, and (3) removing threats associated with alternative images, based on Carl R. Rogers' permissiveness theory. In and of itself this model would not be necessarily anti-rationalistic; it might even fall into the rationalistic category if producing these changes was viewed as an effective way of enhancing one's own goals.[25] But what makes this model in the Rogerian form so different from all the others is that it deals with the goals themselves. The players in this conflict are willing to change themselves as well as the others. They are willing to examine their own goals and values, and even to change them if necessary. They are people of rare security, understanding, and tolerance—or shallowness and lack of conviction. In its ideal form, this model is the kind that can be settled or solved by an appeal to scientific findings.[26]

SOME CONTROVERSIES

The strategic approach to conflict has been the focus of serious attack. If the theory of conflict had remained primarily an academic preoccupation, it is doubtful if it would ever have become an issue. But because so much of it developed in connection with international relations, it did become an issue; and not only social scientists but also the public became involved. Two "schools" emerged, one viewed as "hard" and one as "soft," and theorists themselves became unashamed proponents of one or the other. By and large the "soft" school has been the aggressor. It cannot be said that the "hard" school has been put on the defensive; they do not deign to defend themselves.

There seems to be something about strategic theorists—especially game theorists—which annoys people. As whiz kids they appear arrogant to the more conventional military advisers.[27] There seems to

[23] For a penetrating discussion of the contributions and limitations of Boulding's conceptualizations, see Thomas C. Schelling, "War without Pain, and Other Models," *World Politics*, XV (April, 1963), 465–87. Another Boulding concept, which Schelling views as a bench mark, is that of viability. A party is unconditionally viable if it cannot be absorbed or destroyed as an independent decision-maker; it is conditionally viable if it can be destroyed or absorbed by another party but is not. If one party can absorb or destroy the other, it is the dominant party. Conditional viability may be either secure or insecure. In this model the stakes are, of course, enormous and "the dynamics of a game of survival or extermination is very different from that of the game of maximizing profit or minimizing loss" (p. 66). Boulding uses the economist's competition model as related to geographic markets; but it can be extended. It is useful to explain the influence of psychological or social as well as of physical or geographic distance on conflict (chap. iv). In race relations, for example, it helps to see why things have to get worse before they get better during desegregation.

[24] Rapoport, *op. cit.*, Part III.

[25] Schelling's model also includes changing an opponent's values, at least his payoffs, by one's own behavior. This, however, is done by coercion or deterrence rather than on the basis of "empathy" as in the Rapoport model.

[26] Rapoport began his career with the conviction that science itself could mitigate conflict and change not only means but goals as well (see his *Science and the Goals of Man* [New York: Harper & Bros., 1950]). The work of Richardson and the theory of games of strategy persuaded him that there were other models than that of debate.

[27] Colonel Robert N. Ginsburgh, e.g., in "The Challenge to Military Professionalism," *Foreign Affairs*, Winter, 1963, complains: "Since the end of

be something about the strategy school which gives them a witty, amused and amusing even semihumorous approach to conflict.[28] They seem to enjoy the traps rational men build for themselves. They really have fun playing their games.

The criticisms of the strategic approach are technical, practical, and ethical. The long struggle with the non-zero-sum game and the *n*-person game, especially by Luce and Raiffa, illustrates the technical line of attack. Here Schelling's work on mixed-motive games and the work of coalition theorists on *n*-person games[29] are relevant. But even if or when the strictly theoretical criticisms have been met, there remain practical objections. Here Anatol Rapoport has been the outstanding critic.

Rapoport argues[30] that the strategic approach "lacks conceptual tools with which to deal with important problems." As related to war and peace, it asks the wrong questions. He reminds us that in certain games—"Prisoner's Dilemma" or "Chicken" (or "Paper Tiger" or "Brinkman")—"strategic answers frequently turn out to be at least counter-intuitive and often absurd." The strategic model is impractical, further, because it assumes that the players have enough information so that they can make strategic calculations. It assumes, finally, that their decisions determine the course of events. He denies the validity of these assumptions.

The practical line of criticism fades into the ethical. Rapoport unabashedly assumes the role of advocate. He condemns "the strategic mentality as a whole, and exhorts conflict theorists to embrace what he calls a "system" approach (Richardson model)[31] and a debate model.[32]

World War II, there has gradually developed an increasing number of civilian experts on military policy . . . used more and more to challenge the views of the professional military man. . . . The growing influence of the lay strategist has been accentuated and accelerated by the so-called 'whiz kids' who were brought to the Pentagon by the present Administration and who personify the new civilian experts." And—horror of horrors—broad defense policies at the very top level are now considered matters for civilian judgment.

[28] Williams' popularization of game theory in *The Compleat Strategyst* is a good example; but Luce and Raiffa and even Schelling, to a lesser degree, share a witty and amusing style. The use of clever cartoons is also characteristic. The dramatic nature of the confrontations lends itself to such presentations.

[29] Very often a game of three or more persons can be reduced to a two-person game through coalitions. William A. Gamson has reviewed the literature on coalitions, distinguishing three streams: (1) the sociological work, especially on triads; (2) the mathematical work based on game theory; and (3) the descriptive work (historical and journalistic) "A Theory of Coalition Formation," *American Sociological Review*, XXVI [June, 1961], 373–82; see also William Riker's application of game theory to international coalitions, *The Theory of Political Coalitions* [New Haven, Conn.: Yale University Press, 1962]).

[30] "Strategic and Non-strategic Approaches to Problems of Security and Peace (paper for the Conference on Conceptual and Experimental Analyses of Strategic Interaction and Conflict, Berkeley, Calif., 1964, pp. 18–20).

[31] "In contradistinction to the strategic approach the system approach places system dynamics rather than rational decisions at the center of interest. It seeks not the discovery of ploys to confer advantages on seekers of power but the discovery of laws which govern large scale historical events. The awareness of these laws confers not power but understanding. It is not so likely to discover new means to attain old goals as to emancipate people from preoccupation with old obsolete goals. Even from the strategic point of view rational decisions depend . . . on the value systems of the players, which in turn depend on the way the players view the world and human history" (*ibid.*, pp. 21–22). This "system" approach is reminiscent of the "social forces" theories, the Marxist form of which was that of an inevitable class struggle in which "history" operates to change the relationships among classes, one succeeding another to power until a classless society is arrived at; the geophysics form is in terms of land masses, heartlands, or national interests which propel policy makers toward inevitable decisions no matter who or what they are as individuals. People, in brief, are the pawns of forces that operate according to their own inertia; policy makers are interchangeable parts.

[32] Since games like the "Prisoner's Dilemma" and "Chicken" can be solved only if one or both

Irving Louis Horowitz has mounted an attack on a new kind of power elite, the men he calls the New Civilian Militarists (NCM), among whom he counts Herman Kahn, Thomas C. Schelling, Henry A. Kissinger, and Albert Wohlstetter.[33] Unlike Morris Janowitz, who discounts the impact of game theory on the military,[34] Horowitz finds that the NCM have outflanked the military. He accuses them of having rehabilitated the romantic defense of violence or war as an instrument of policy.[35] Aside from the fact that not all the men he mentions belong to the same school, it certainly cannot be said that as a group they espouse war. Indeed, for some, strategy is an alternative to violence. It is true, however, that it is often part of their assignment to tackle problems to which the military wants answers.

In Rapoport's eyes, there is no intrinsic incompatibility between the practical and the ethical critique of the strategic, especially the game, approach to conflict. He argues that the major contribution of classic game theory, in fact, was the convincing proof it offered that "the 'hardheaded' analysis of conflicts (with which game theory starts) comes to an impasse," and that "paradoxical conclusions cannot be avoided unless the situation is reformulated in another context and unless other, extra-game-theory concepts are invoked,"[36] and he concludes that "the cognitive assumptions of the strategists are neither revealed truths nor self-evident facts. They are rather derivatives of a power-oriented value system, which sharply delimits the cognitive horizon of its adherents. It is high time we stopped identifying narrowness of vision with 'realism.' It is high time to stop calculating long enough to think awhile, perhaps even to listen to the voice of our conscience."[37]

III. THE THEORY OF VIOLENCE

One important gain in recent thinking about conflict is the clear-cut conceptual distinction between violence and conflict. There was a time when the two were either confused with one another or actually identified with one another. A good deal of discussion of conflict was reduced to arguments for or against the use of violence. Violence is important enough to warrant a great deal of research; but it is not important enough to pre-empt all the thinking in the field. Still there is room for more study of violence per se.

Violence may be viewed non-rationalistically or rationalistically.[38] It may conform to the Rapoport "fight" or to the Richardson model or to the strategic model. In view of the enormous significance of violence as a sociological phenomenon, it is somewhat surprising that it has not attracted more theoretical—as distinguished from empirical or descriptive—attention.[39]

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players can be persuaded to behave in a way not mutually defeating, only a debate model can handle the situation (*ibid.*, p. 17).

[33] Irving Louis Horowitz, *The War Game, Studies of the New Civilian Militarists* (New York: Ballantine Books, 1963).

[34] A social science theory of international relations based upon game theory appears to be an unfulfilled promise, for it has not produced hypotheses and understanding beyond common sense. The point has been reached where the drive for a general theory of international relations and a general theory of conflict resolution has become a rigid act of faith and ideology rather than a problem-solving effort. . . . The purpose of social science is to enhance the rationality of political leadership, but it is clear that this purpose is not necessarily achieved by developing theories which assume that international relations are guided by rationality" (Morris Janowitz, *The Professional Soldier* [New York: Free Press of Glencoe, 1960], pp. 434, 442).

[35] Horowitz, *op. cit.*, p. 12.

[36] "The Use and Misuse of Game Theory," *Scientific American*, CCVII (December, 1962), 118.

[37] "Strategic and Non-strategic Approaches to Problems of Security and Peace," *op. cit.*, p. 23.

[38] Jessie Bernard, *American Community Behavior* (New York: Holt, Rinehart & Winston, 1962), chap. xxvi. This discussion of violence is about the only one in current sociological literature.

[39] It is interesting to note that Schelling finds Boulding's conceptual model of war defective be-

Quite aside from military violence or war, the norms regulating violence seem to be undergoing change in our society. In selecting jurors for the Medgar Evers trial, it was considered appropriate to ask the veniremen if they believed it was wrong for a white man to kill a Negro. Violence itself in our day has taken two turns. On one hand, warfare has returned to personal hand-to-hand combat; not, of course, in the romantic form of rule-disciplined chivalry but rather in the form of guerilla warfare and commando-type raids, training for which involves learning how to flick out an opponent's eyeballs or otherwise maim and destroy him. On the other hand, it has reached the ultimate in impersonality; one push of a button can destroy whole areas.

Even more interesting is the high degree of bureaucratization of violence in our day. It is no longer true that "the military professional is unique because he is an expert in war making and in the organized use of violence."[40] The organized use of violence was assigned also to others under the Nazis. One aspect of bureaucratization of violence was the high degree of specialization involved which assigned only one task to any particular bureaucrat. Together, as parts of an efficient organization, they succeeded in killing millions of people. Many, like Eichmann, were doubtless good sons, husbands, and fathers. They did what they were ordered to do, shipping "millions of men, women, and children to their death with great zeal and most meticulous care."[41] Who, then, were the violent ones? Obviously psychological models in terms of personal aggression, frustration, or pugnacity are inadequate. But there is hardly any other model—strategic or non-strategic—which suits violence on this level.[42]

Although there is actually no true distinction between violence and strategy, since violence may be strategic in some cases, Schelling does conceptualize strategy as an alternative to force, and the distinction is a useful one. Neither strategy nor force may be said to be more fundamental than the other in terms of historical precedents. But interesting changes are occurring in our day. In the past, for example, strategy was more likely than violence to be used by the underdog; the topdog had a choice between strategy and force. One of the paradoxes of modern conflict, especially among nations, is that the pattern tends to reverse itself. Today the powerful nations cannot use force as freely as in the past; they have to use strategy. The weak nations, on the other hand, can use either violence or strategy. Gunboat diplomacy no longer works. It would be very easy from the military point of view to send in the marines and take Cuba. But that alternative is not open to us. Rioting students in poor, weak nations around the world can attack our embassies at will; we cannot retaliate in kind by storming their embassies or taking over their governments. "The Street" can be called on by the weaker nations; they can apparently turn mobs off and on at will. There isn't much we can do in return. Force, in brief, is not what it used to be.

IV. THE THEORY OF NON-VIOLENCE

Non-violence combines both rationalistic and non-rationalistic elements. It is strategic, but it is also dependent on non-strategic

cause it lacks "a theory of the role of violence" ("War without Pain, and Other Models," *op. cit.*, p. 483). Schelling himself sees war as "violent, coercive bargaining—bargaining with demonstrations and threats of further pain, privation, destruction, and risk. And it is often inefficient, hotheaded, undisciplined bargaining—in danger of losing contact with policy."

[40] Janowitz, *op. cit.*, p. 15.

[41] Hannah Arendt, "Eichmann in Jerusalem," *New Yorker* (February 16, 1963), p. 67.

[42] The destruction of whole cities was common in Old Testament times (e.g., Josh. 6 : 21; 8 : 26; 12 : 11–14) and strictly according to Yahweh's orders (Deut. 7 : 2; 16; 23). But it was in the heat of battle, not coldly and scientifically carried out by civilians.

factors. It is a special form of strategic deterrence and/or coercion. Although it is non-violent it may, nevertheless, be as damaging to an opponent as violence. For this reason, theorists of non-violence as a movement have insisted that it must be based on love and that its practitioners be prepared for suffering.

The sociological aspects of non-violence have not been examined by sociologists in any detail since Clarence Case's book *Non-violent Coercion* in 1923. In fact, Boulding complains that it "has never been adequately studied by professional scientists."[43] This neglect is especially interesting in view of the large part non-violent techniques have played in the current Negro revolution.

Much of the discussion of non-violence is ethically motivated; but the arguments often include evidence that it is more effective than violence as a way of dealing with conflict.[44] Still not much has been done yet to determine when, under what conditions, against whom, and how non-violence succeeds or fails. Schelling's concept of the co-ordination game, as well as his analyses of strategic moves, would be useful here.

Non-violence is a highly sophisticated strategy. Mulford Q. Sibley's *The Quiet Battle* includes an account of how non-violence would operate in case of invasion. The amount of discipline demanded would be far greater than that required by the use of military force. The heart of non-violence is the absolute confidence people have in one another; they base everything on expectations, on being able to count on what the others will do; often a vast co-ordination game emerges, as in an underground movement. There is tacit communication; everyone knows what to expect of everyone else even in an emergency or unexpected situation. It would not be impossible to achieve such expectations; but it would require an effort of monumental proportions to train people and to motivate them for the practice of non-violence. It is definitely not for the weak, the cowardly, or the crafty.

The work discussed so far is primarily psychological in orientation and deductive in approach. There also has been sociologically oriented work which is empirical in approach. It is important not because it offers new theoretical conceptualization but because it has applied interesting techniques to the analysis of its data.

V. THE SOCIOLOGICAL APPROACH: EMPIRICAL STUDIES

Conflict continues to be grist for the historian's mill; and the historical method, or material produced by it, continues to be used by other students of conflict. In some cases theory is implicit in the way data are presented, as in Barbara Tuchman's *The Guns of August* (1963) describing the beginning of World War I or in Cecil B. Woodham-Smith's *The Great Hunger* (1963).

Of twofold interest for theory and method is Stanley M. Elkins' *Slavery, a Problem in American Institutional and Intellectual Life* (1959). First, it is itself controversial and hence party to a conflict—debate model—over the correct interpretation to be put on the nature of slavery, with an almost incalculable train of implications for race relations today.[45]

[43] *Op. cit.*, p. 337.

[44] "The advocates of non-violent resistance as a means would maintain . . . that . . . it is . . . far more efficacious . . . than the means of violence" (Mulford Q. Sibley, *The Quiet Battle* [Chicago: Quadrangle Books, 1963], p. 363). "There is rapidly developing a demand . . . for ways of conducting conflict . . . which are constructive and not destructive" (Joan Valerie Bondurant, *The Conquest of Violence* [Princeton, N.J.: Princeton University Press, 1958]). Note the symbolism of "fighting" violence in the title of this book.

[45] Elkins' first chapter is a masterly presentation of the history of the "conflict" or controversy regarding the interpretation of the nature of slavery. Ulrich Phillips' interpretation of it as a benign institution had superseded the older viewpoint. Then it came under "attack." "Phillips' moral biases were challenged, but not head-on; nor was there any basic impeachment of Phillips' scholarly

Also of interest in an overview of conceptualizations of conflict is Elkins' use of the experience of inmates of Nazi extermination camps to interpret and explain the character structure of Negroes. The use of this analogy was the subject of hostile comments. Interpreting conflict in terms of personality is an old story; interpreting personality in terms of conflict is a fairly new one. Despite Elkins' sophisticated defense of his method,[46] one cannot escape the feeling that this analogy between concentration camp victims and slaves was a direct thrust at the jugular vein. The racist foundation in both situations was inescapable.

Conflict is, of course, also a major preoccupation of political scientists. Marxists class-struggle theory, which once loomed so large as to be identified in the minds of many as sociology itself, has long since ceased to invite the dedicated attention of conflict theorists. As Lipset notes, "in every modern democracy conflict among different groups is expressed through political parties which basically represent a 'democratic translation of the class struggle.' "[47] Political conflict is therefore currently studied through voting behavior.[48] In this area, Lipset's own work has been salient, especially his documentation of the fact that the working classes are not uniformly liberal in their positions and that they are definitely not so in matters of civil rights.

In addition to this and to the traditional studies of diplomacy, colonialism, imperialism, and the nature of power, a new development has recently taken place which can be illustrated by the work of Rudolph J. Rummel.[49] In a statistically sophisticated study, he correlated nine domestic and thirteen foreign conflict-behavior measures (as of 1955–57) with variables on the structural and behavioral characteristics of sixty-six nations. By means of factor analysis he delineated three dimensions for domestic conflict which he labeled a turmoil dimension, a revolutionary dimension, and a subversive dimension. There were also three dimensions for foreign conflict: war, diplomacy, and belligerency. He was struck by the fact that domestic and foreign conflict constituted two quite different worlds so far as his indexes were concerned.[50]

Wagley and Harris studied longitudinally six societies with minority-group populations and concluded that the most useful conceptualization was one of ongoing social conflict.[51] Allen D. Grimshaw has reported on the ecology of race riots[52] and James S. Coleman has analyzed a large number of community conflicts and generalized the

claims. Instead, Hofstadter challenged Phillips' technique of sampling. . . . The implication was that, by enlarging the sample, fundamental errors of interpretation might be corrected. Willy-nilly, the moral center of gravity was shifting once more to a strong antislavery position" (pp. 18–19).

[46] "The basic variables were captivity, absolute power and their effects on personality. I was not comparing intentions, motives, or styles; even 'cruelty' was not indispensable as an item in my equation" (p. 227).

[47] Seymour Martin Lipset, *Political Man* (New York: Doubleday & Co., 1960), p. 220. Although Lipset discusses conflict, he also thinks in terms of "cleavages," a less dynamic concept. Like Elkins' work, Lipset's has also been controversial. It has been interpreted—appreciatively—as one more attack on the myth of the Noble Poor and hence, indirectly, on all the leftist ideologies based on it (see William Petersen and David Matza [eds.], *Social Controversy* [Berkeley, Calif.: Wadsworth Publishing Co., 1963]).

[48] Eugene Burdick and Arthur J. Brodbeck have assembled many such voting studies in *American Voting Behavior* (Glencoe, Ill.: Free Press, 1959).

[49] "Dimensions of Conflict Behavior within and between Nations" (Evanston, Ill.: Northwestern University, 1963 [mimeographed]).

[50] *Ibid.*, p. 61.

[51] Charles Wagley and Marvin Harris, *Minorities in the New World* (New York: Columbia University Press, 1958).

[52] "Lawlessness and Violence in America," *Journal of Negro History*, XLIV (January, 1959), 52–72, and "Urban Racial Violence in the United States," *American Journal of Sociology*, LXVI (September, 1960), 109–19.

natural history of such conflicts.[53] In addition there is doubtless a considerable amount of empirical and inductive research which is still classified and therefore not available.[54]

Interesting as these empirical studies are, they have not introduced any essentially new conceptualizations. They have, nevertheless, perfected classic techniques and produced valuable substantive findings.

VI. EXPERIMENTAL STUDIES

Of special interest is the fast-growing experimental approach to the study of conflict. In one of their earliest works the Sherifs found that they could create conflict among boy campers by setting up certain conditions and could mitigate it by changing the conditions.[55]

But the salient experimental approach is by way of laboratory games which, in effect, combine deductive theory with empirical research. Games are definitely "in." Everyone appears to be getting into the act: economists, psychologists, political scientists, and even sociologists. The essential idea is to set up some sort of controlled situation and then observe and record the way the subjects play it. The game may vary from zero-sum to non-zero-sum cooperative in structure; the rules may vary, stimulating deals, cheating, bluffing, or the reverse. In some cases the motivation is supplied by winning money; in one experiment half of the students' grades depended on the number of points they won. In James S. Coleman's game the situation simulates legislative logrolling; the players try to win votes for their bills by any means they can, including logrolling, and to defeat opponents' bills. Bargaining, negotiating, trading, and developing norms are among the processes studied in these games.

Schelling has summarized some of the contributions to be expected from such games:

> A game may be useful in revealing the *structure* of conflicts rather than the details. A game may be useful in the articulation of a theoretical model. . . . One kind of game may . . . elucidate a theoretical model. the other . . . show its limitations. One may be used to bring out the order, the rationality, and the coherence of the international structure; the other to illustrate the disorder, the irrationality, and the incoherence.[56]

Such games may be as illuminating as projective personality tests as they are as models of conflict. For one of the most interesting findings is how greatly people differ in their styles of playing. Some people enjoy game situations and play zestfully. Others reject them. Women tend to react somewhat differently from men.[57] These facts are useful. In the usual laboratory study, the situation is taken for granted and the emphasis is on individual differences in reacting to it. In game studies the reverse seems to be true. The people are taken for granted and the emphasis is on the game. Is it accidental that poker is a favorite game of politicians? That gambling which depends on strategy as well as on chance attracts a certain kind of person? That poker is so avidly played by

[53] *Community Conflict* (Glencoe, Ill.: Free Press, 1957).

[54] The Special Operations Research Organization, for example, has staff members working on revolution and there is almost certainly an equally large amount of work on conflict in the files of Rand and other research organizations. Not classified is Rex Hopper's "Cybernation, Marginality, and Revolution," in *The New Sociology*, ed. I. L. Horowitz (New York: Oxford University Press, 1964), pp. 313–30.

[55] Muzafer Sherif, O. J. Harvey, B. J. White, W. R. Hood, and Carolyn W. Sherif, *Intergroup Conflict and Cooperation* (Norman: University of Oklahoma Press, 1961).

[56] Thomas C. Schelling, "Experimental Games and Bargaining Theory," *World Politics*, XIV (October, 1961), 48.

[57] See, e.g., Harold H. Kelley, "A Classroom Study of the Dilemmas in Interpersonal Negotiations" (paper submitted to the Conference on Conceptual and Experimental Analyses of Strategic Interaction and Conflict, Berkeley, Calif., February, 1964), p. 2.

the "black bourgeoisie"? Some people love to pit their wits against worthy opponents; others loathe doing it. The strategic game may finally prove to be as useful for what it tells us about people as for what it tells us about interaction.[58]

It is difficult to evaluate the several approaches to the study of conflict. They are not, as a matter of fact, necessarily mutually exclusive or even always different. It can be stated almost unequivocally that the zero-sum game is not a suitable model. But beyond that there seem to be situations in which any one or some combination of several models is appropriate. It is not a matter of indiscriminate acceptance of one to the exclusion of all others; it is a matter, rather, of finding the model that best fits the situation. And the attraction of different models of conflict for different kinds of people may lie not so much in the validity of the model as in the predilections of the theorists themselves.

VII. THE SOCIOLOGICAL SIGNIFICANCE OF CONTROVERSY IN THE FIELD OF CONFLICT

The controversy among different schools of conflict is itself a distinctively sociological phenomenon. It is an interesting example of the way sciences grow, as analyzed by Kuhn, who showed that the growth of science is a process of natural selection among competing paradigms.[59] The game paradigm is now an important challenger; it has, despite its defects, attracted a substantial following, and the controversy it has engendered shows how our knowledge and insights grow.

The controversey is interesting sociologically also because it reveals that scientific thinking and researching about conflict are themselves sociological phenomena; like research about sex, they have sociological consequences. The models one selects have repercussions. It is not only that they perform the same function as that performed by, let us say, the muckrakers, the exposers, and the like, who sometimes force people to act—however reluctantly—in a way to implement their professed values. Scientific thinking and researching about conflict may actually change values themselves.[60]

The controversy is interesting, finally, because it shows how closely related science and ethics are. Many of those who reject strategic, especially game, theory do so not only on grounds that it is not efficacious—a rational objection—but also on grounds that it is not ethical.

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[58] Morris Janowitz rejects game theory as a theory of conflict, but reports interesting results from the use of games as personality tests. "The application of the game theory and mathematical studies of risk-taking have yet to demonstrate their practical utility for the conduct of international relations, although they may have some relevance in training potential policy makers and diplomats. Interestingly enough, these research efforts have produced some suggestive evidence that military personnel select more high payoff, low probability solutions than comparable civilian groups; in short, these experiments point to a lower tolerance for ambiguity in military thinking" (*op. cit.*, p. 276).

[59] Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962).

[60] Says Rapoport: "The most important result of a systematic and many-sided study of conflict would be the changes which such a study could effect in ourselves, the conscious and unconscious, the willing and unwilling participants in conflicts" (*Fights, Games, and Debates*, p. 360).

The Multivariate Analysis of Dichotomized Variables

Herbert L. Costner and L. Wesley Wager

ABSTRACT

This paper compares two alternative techniques for the multivariate analysis of three dichotomized variables—the test for the significance of the difference between differences between proportions and the χ^2 test for second-order interaction. A reinterpretation of the latter test is introduced, thus making available a test for the significance of difference between two Q's. It is also demonstrated that under certain conditions the two tests being compared produce contrasting results and, in the absence of other information, would lead the researcher to opposite conclusions. Some tentative criteria for deciding which of the two techniques is appropriate for a given set of data are suggested.

In recent years sociological analysis has increasingly shifted from the study of direct cause-effect relationships to the study of complex social situations in which a number of variables are embedded in intricate patterns. Traditional bivariate statistical techniques have been of limited use in these situations, for the intricate interrelationships between variables cannot always be represented in terms of the intercorrelations between variables taken two at a time.[1] In such cases, bivariate modes of analysis are likely to be unrewarding and perhaps misleading. To explore complex relationships without multivariate tools is analogous to the exploration of the connection between temperature change and precipitation without any concept of relative humidity. The interrelationships between these variables are, in fact, quite orderly: rain follows a drop in temperature, provided the relative humidity is already "high." But until all three variables are taken into account simultaneously, the interrelationships of these variables with one another appear weak or even chaotic.

Multivariate techniques which deal with this general type of situation for certain forms of data have been discussed but not widely utilized in the sociological literature. Thus analysis of variance and of covariance not only allows the exploration of several bivariate relationships on the same data but also includes a test for the constancy of these relationships across the various categories of the other variables included, that is, the tests for "interaction" effects.[2] Tests of the significance of the difference between correlation coefficients from independent samples serve the same general purpose.[3] But these techniques have not been widely utilized by sociologists—perhaps because they are unknown, believed unwieldy and difficult, or inapplicable to the data available for analysis. And when the data available for analysis take the form of three (or more) dichotomized variables, these familiar techniques can be used only by a gross disregard for their underlying assumptions.

In this paper we focus on the multivariate analysis of dichotomous variables and discuss (1) some problems associated with the procedures most commonly employed in the interpretation of $2 \times 2 \times 2$ tables; (2) two alternative techniques for examining the constancy of a relationship

[1] The type of multivariate relationship of special interest in this paper is what Hans L. Zetterberg has called a "contingent" relationship, e.g., "If *X* then *Y*, but only if *Z*" (see his *On Theory and Verification in Sociology* [Totowa, N.J.: Bedminster Press, 1963], p. 16).

[2] See Hubert M. Blalock, *Social Statistics* (New York: McGraw-Hill Book Co., 1960), chaps. xvi and xx.

[3] *Ibid.*, pp. 310–11.

under two different conditions—the Dorn-Stouffer-Tibbitts-Goodman method and the χ^2 test for second-order interaction—with their principal similarities and differences;[4] (3) the circumstances under which the two alternative sets of procedures lead to opposite conclusions for the same $2 \times 2 \times 2$ tables; and (4) differences in the conditions under which the two tests are appropriate. In view of the nearly total neglect of the χ^2 test of second-order interaction in the sociological literature, somewhat greater emphasis will be placed on the application and relative merit of this method of analysis. But it should be clear that both the Dorn-Stouffer-Tibbits-Goodman method and the relatively neglected χ^2 test for second-order interaction are tests for the persistence or constancy of a relationship between two variables under varying conditions of a third variable.

TRADITIONAL INTERPRETATIONS OF THE $2 \times 2 \times 2$ TABLE

The use of dichotomized variables is, of course, common in both experimental and survey research in sociology today. In view of this widespread use, Lazarsfeld, Kendall, and Hyman have elaborated on the techniques for analyzing relationships among three dichotomized variables originally suggested by G. Udny Yule and M. G. Kendall.[5] Essentially, the elaborations of Lazarsfeld, Kendall, and Hyman clarify the relationship between two variables or attributes through the introduction of a third "test" variable.[6] Through this controlling or partialing procedure, the investigator is able to interpret the theoretical linkages among the variables and confront more directly the problem of spuriousness. Of more direct relevance to the present discussion, the investigator who utilizes this partialing procedure is also able to specify the limits of generalizations, that is, to check the persistence of a relationship under varying conditions. Traditionally, sociologists have examined these multivariate tables by computing measures of association, χ^2's or percentage differentials for each 2×2 table resulting from the introduction of the test variable. Although such computations do allow for a preliminary comparison of the degrees of relationship in two such tables, no statistical test for the existence of a difference between degrees of relationship in the tables is ordinarily employed, even in studies otherwise manifesting considerable methodological sophistication. Appropriately, however, the absence of a significance test for the difference between the two tables has not prevented comparative conclusions from being drawn. But in the absence of such tests, the danger is increased that elaborate interpretations might be offered for differences that were nothing more than sampling fluctuations.

In introducing their test for the significance of difference between differences in proportions for use in comparing two 2×2

[4] Clark Tibbitts and Samuel Stouffer, "Testing the Significance of Comparisons in Sociological Data," *American Journal of Sociology*, XL (November, 1934), 357–63; Leo Goodman, "Modifications of the Dorn-Stouffer-Tibbitts Method for 'Testing the Significance of Comparisons in Sociological Data,'" *American Journal of Sociology*, LXVI (January, 1961), 355–63; M. S. Bartlett, "Contingency Table Interactions," *Supplement to the Journal of the Royal Statistical Society*, II (1935), 248–52; George W. Snedecor, *Statistical Methods* (Ames: Iowa State College Press, 1946), pp. 200–204; Leo A. Goodman, "On Methods for Comparing Contingency Tables," *Journal of the Royal Statistical Society*, Ser. A, CXXVI, Part I (1963), 94–108; Leo A. Goodman, "Simple Methods for Analyzing Three-Factor Interaction in Contingency Tables," *Journal of the American Statistical Association*, LIX (June, 1964), 319–52; and B. N. Lewis, "On the Analysis of Interaction in Multi-dimensional Contingency Tables," *Journal of the Royal Statistical Society*, Ser. A, CXXV, Part I (1962), 88–117.

[5] *An Introduction to the Theory of Statistics* (New York: Hafner Publishing Co., 1950), pp. 36 ff.; Patricia Kendall and Paul Lazarsfeld, "Problems of Survey Analysis," in R. K. Merton and P. Lazarsfeld (eds.), *Continuities in Social Research* (Glencoe, Ill.: Free Press, 1950), pp. 133–96; and Herbert Hyman, *Survey Design and Analysis* (Glencoe, Ill.: Free Press, 1955), pp. 242–329.

[6] Hyman, *op. cit.*, pp. 242–329.

tables, Tibbitts and Stouffer commented on the care to be taken in drawing conclusions from such comparisons.

> At first glance, a test of the comparison may seem to the reader to be superfluous. But it is entirely possible that one difference may be significant and the other not; and yet the difference between the two differences may not be significant. Conversely, it is possible, if differences have opposite algebraic signs, that neither may be significant when considered alone, but that the excess of one difference when over the other will be significantly large.[7]

While contemporary sociologists have neglected some available measures of independence for such 2 × 2 × 2 tables, classification problems of this order are not unusual but, indeed, are commonplace today.

TWO ALTERNATIVE TESTS

I. TESTING THE SIGNIFICANCE OF THE DIFFERENCE BETWEEN DIFFERENCES BETWEEN PROPORTIONS IN A 2 × 2 × 2 TABLE

The pioneering efforts of Dorn, Tibbitts, and Stouffer to provide a test for the significance of the difference between differences between proportions preceded by several years the growth of interest in elaborating dichotomous relationships through the introduction of a test variable. But their efforts have only recently been revived and brought to current attention. Goodman has not only revived these earlier efforts, but he has also corrected the earlier estimate of the sampling variability of the difference between differences between proportions.[8] Since Goodman's paper is readily available, it is sufficient to indicate here only the general nature of the test.

As in Figure 1, let the cell frequencies in the two 2 × 2 tables be represented by the letters a, b, c, d, and a', b', c', and d'. We define d_1 as the difference between proportions of row totals in the first table:

$$d_1 = \frac{a}{a+b} - \frac{c}{c+d} . \qquad (1)$$

The quantity d_2 is analogously defined for the second table. The test of significance of the difference between differences between proportions tests the null hypothesis that $d_1 - d_2 = 0$. Under the null hypothesis, $d_1 - d_2$ is normally distributed with mean zero and variance equal to the sum of the variances of d_1 and d_2. More specifically,

$$\sigma^2_{d_1-d_2} = \frac{p_1 q_1}{n_1} + \frac{p_2 q_2}{n_2} + \frac{p_3 q_3}{n_3} + \frac{p_4 q_4}{n_4} , \qquad (2)$$

where the p's are the proportions of row totals as indicated above, and the n's are the row totals themselves.

The degree of difference between proportions in a 2 × 2 table may be reasonably

a	b
c	d

a'	b'
c'	d'

FIG. 1

regarded as an index of the degree of relationship between the two variables represented in the table. The most commonly used measures of relationship for 2 × 2 tables other than the proportion or percentage difference are ϕ and Q.[9] It can be easily shown that when the difference between proportions in a 2 × 2 table is zero, ϕ and Q are also zero. Both ϕ and Q may be expressed as functions of the difference between proportions. As Blalock has noted,[10] ϕ is very similar in magnitude to d, and Goodman and Kruskal[11] have pointed out

[7] Tibbitts and Stouffer, *op. cit.*, p. 359.

[8] Goodman, *op. cit.* (1961), pp. 356–59.

[9] See John H. Mueller and Karl F. Schuessler, *Statistical Reasoning in Sociology* (Boston: Houghton Mifflin Co., 1961), pp. 242–58.

[10] H. M. Blalock, "A Double Standard in Measuring Degree of Association," *American Sociological Review*, XXVIII (December, 1963), 988–89.

[11] Leo A. Goodman and William H. Kruskal, "Measures of Association for Cross Classifications. II: Further Discussion and References," *Journal of the American Statistical Association*, LIV (March, 1959), 130.

that the earliest known occurrence of the "mean square contingency" (i.e., ϕ^2) in the statistical literature appears as the product of the two differences between proportions (of row totals and of column totals) for a 2×2 table. Hence, ϕ may be referred to as a symmetrical version of d. The similarities are evident in the following algebraic expressions. We let d_r and d_c represent the difference between proportions of row totals and the difference between proportions of column totals respectively. Thus

$$d_r = \frac{ad - bc}{(a+b)(c+d)};$$

$$d_c = \frac{ad - bc}{(a+c)(b+d)};$$

$$d_r d_c = \frac{(ad - bc)^2}{(a+b)(c+d)(a+c)(b+d)} = \phi^2 .$$

Clearly, if $d_r = d_c$, then d and ϕ are identical. And it may be noted also that

$$\phi = \sqrt{\frac{(ad - bc)^2}{(a+b)(c+d)(a+c)(b+d)}}$$

$$= \frac{ad - bc}{(a+b)(c+d)} \sqrt{\frac{(a+b)(c+d)}{(a+c)(b+d)}}$$

$$= d_r \sqrt{\frac{(a+b)(c+d)}{(a+c)(b+d)}} .$$

Thus if the marginals are skewed to a similar degree for both rows and columns, that is, if $(a+b)\,(c+d) = (a+c)\,(b+d)$, then $d_r = d_c = \phi$. The measure Q will be expressed as a function of d in a later section of this paper. Even though there are some very real similarities between d_1, ϕ and Q, the Dorn-Stouffer-Tibbitts-Goodman test is not a test of the null hypothesis of equal ϕ's or equal Q's although it more nearly approximates the former than the latter.[12]

[12] Goodman describes a test for the significance of difference between two ϕ coefficients ("Methods for Comparing Contingency Tables," *op. cit.*, p. 100).

II. TESTING FOR SECOND-ORDER INTERACTION IN A $2 \times 2 \times 2$ TABLE: A REINTERPRETATION

Bartlett, Snedecor, and others offer another test for examining pairs of 2×2 tables and for testing the null hypothesis of no difference in the degree of relationship between them.[13] The literature pertaining to this test has been recently reviewed and elaborated by Goodman.[14] Even though the Bartlett test has been almost wholly neglected by sociologists for the $2 \times 2 \times 2$ table, that test was developed at about the same time as the initial attempts to develop a test for the significance of the difference between differences between proportions.[15] In the more familiar χ^2 test of statistical independence in the 2×2 table, the expected frequencies are, of course, a function of the marginal frequencies or, more specifically, a function of the modifications necessary to make $ad = bc$, or $ad/bc = 1$. Expected frequencies necessary in the computation of χ^2 for the $2 \times 2 \times 2$ table, however, are a function of the modifications necessary to make the ratio of cross-product ratios equal for the two tables. It is important to recognize this difference in the calculation of the expected frequencies, since χ^2 with one degree of freedom serves as the test statistic for both the 2×2 table and the $2 \times 2 \times 2$ table.[16]

The chi-square test for the $2 \times 2 \times 2$ table has been described as a test for "second-order interaction." Second-order interaction is defined as the ratio of "first-order interactions" for the two tables. First-order interaction, in turn, is defined as the ratio of cross products.[17] Referring again

[13] Bartlett, *op. cit.*, p. 248; Snedecor, *op. cit.*, pp. 200–201, 203.

[14] Goodman, *op. cit.*, 1963 and 1964.

[15] Note that the Bartlett publication in the *Supplement to the Journal of the Royal Statistical Society* appeared in 1935 while the Tibbitts and Stouffer article in the *American Journal of Sociology* appeared in November, 1934.

[16] Bartlett, *op. cit.*, p. 249.

[17] Snedecor, *op. cit.*, p. 202.

to Figure 1, the null hypothesis tested by this test for second-order interaction is

$$\frac{ad}{bc}=\frac{a'd'}{b'c'}, \quad (3)$$

or

$$adb'c'=a'd'bc\,. \quad (4)$$

The statistical-inference problem is a problem of deciding whether the difference between the left and right member of equation (4) is too large to be attributed to sampling variability. If so, it may be concluded that there is second-order interaction. If not, the test would lead one to conclude that the data cast no doubt on the assumption that the first-order interactions are the same for the two tables. Alternative interpretations of the null hypothesis at issue in this test and its difference from the Dorn-Tibbitts-Stouffer-Goodman test will be discussed following a brief description of the procedures for finding the expected frequencies for the eight cells under the hypothesis of equation (4) and hence for finding the χ^2 value for testing that hypothesis.

The computations involve essentially two steps.[18] First, the expected frequencies under the hypothesis must be determined. This involves the calculation of the quantity x which represents the number to be subtracted from the cell frequencies in the left member of equation (4) and added to the cell frequencies in the right member of equation (4). Referring to the illustrative data in Figure 2, calculation of x yields the quantity by which the frequencies in that table must be altered in order for equation (4) to hold true. Fisher,[19] Bartlett,[20] Norton,[21] and Snedecor[22] have indicated that the value of this deviation, x, can be determined by solving the following cubic equation:

$$(a-x)(d-x)(c'-x)(b'-x) = (c+x)(b+x)(a'+x)(d'+x). \quad (5)$$

The second step involves the utilization of of quantity x in finding the expected num-

LOW DISCUSSION — Building in Present Route a Good Idea?

	No or Don't Know	Yes
Owners	a 79	b 46
Renters	c 30	d 26

HIGH DISCUSSION — Building in Present Route a Good Idea?

	No or Don't Know	Yes
Owners	a′ 26	b′ 41
Renters	c′ 8	d′ 6

$\chi^2_{2\times2\times2} = 2.91$, 1 degree of freedom, $p < .10$.

FIG. 2.—Relationship between residential status and attitude toward construction of tollway through immediate neighborhood: controlling amount of discussion in neighborhood.

[18] The computations are tedious and time-consuming when calculated with the aid of a desk calculator. In view of this prohibitive feature of the χ^2 test for second-order interaction (or for the significance of difference between Q's), a computer program has been written to speed this calculation. This program, which now makes feasible widespread use of the technique, is appropriate for the IBM 709. Preparations for its use are very simple. The program, which was written by Ross Kendall, is available upon written request from the present authors. As an alternative, see H. E. Salzer, C. H. Richards, and I. Arsham, *Table for the Solution of Cubic Equations* (New York: McGraw-Hill Book Co., 1958).

[19] Bartlett indicates that Professor R. A. Fisher pointed out to him that the x may be calculated by means of this cubic equation (Bartlett, *op. cit.*, p. 249).

[20] *Ibid.*, pp. 249–50.

[21] H. W. Norton, "Calculation of Chi-Square for Complex Contingency Tables," *Journal of the American Statistical Association*, XL (June, 1945), 251–58.

[22] Snedecor, *op. cit.*, p. 202.

ber for each of the cells in Figure 2 and, hence, the adjusted χ^2 statistic. Thus:

$$\chi^2 = (|X| - 0.5)^2 \left(\frac{1}{a-x} + \frac{1}{d-x} + \frac{1}{c'-x} + \frac{1}{b'-x} + \frac{1}{c+x} + \frac{1}{b+x} + \frac{1}{a'+x} + \frac{1}{d'+x} \right) . \quad (6)$$

The actual computation of χ^2 for Figure 2 proceeds from the substitution of values of Figure 2 in equation (5), yielding the following equation:

$$(79-x)(26-x)(8-x)(41-x) = (30+x)(46+x)(26+x)(6+x);$$

$$673{,}712 - 135{,}086x + 7{,}527x^2 - 154x^3 + x^4 = 215{,}280 + 56{,}016x + 3{,}968x^2 + 108x^3 + x^4;$$

$$-262x^3 + 3{,}559x^2 - 191{,}102x + 458{,}432 = 0 .$$

The method of solving for x involves the following sequences of procedures:

1. Elimination temporarily of the first two elements of the cubic equation and solution of the remaining portion of the equation, thus producing a first approximation of x and its powers. In this case:

$$-191{,}102x + 458{,}432 = 0$$
$$-191{,}102x = -458{,}432$$
$$x = 2.40 .$$

2. Substitution of the first approximating values for x, x^2, and x^3 in the cubic equation. If the approximation for x is correct, the positive and negative numbers should equal one another and the resulting difference should be zero. In this case the result of the first approximation is 478,932 — 462,266, or a positive difference of 16,666.
3. Arbitrary selection of successive approximations to x designed to make the resulting difference in step 2 disappear.
4. If the correct value of x is somewhere between the values of x which have been tried, interpolation should be utilized to reduce the number of approximations by estimating the proportion that x should be increased or decreased. Final approximation of x in this case is 2.4934.[23]

The correct value of x is then substituted in equation (6). In the case of Figure 2, the single χ^2 value is 2.91 for a $2 \times 2 \times 2$ classification with a single degree of freedom. This χ^2 value suggests that the evidence is not strong enough for us to assume that there is "second-order interaction" in Figure 2.[24]

Actually, the language of "second-order interaction" is probably somewhat confusing in this context. Although the mathematical definition of this concept is quite precise, that definition is not expressed in terms that allow any immediate interpretation in conventional statistical language. By constructing a straightforward algebraic argument, however, it is possible to transform this definition into more familiar statistical terms. Consider the hypothesis of equation (4), $adb'c' = a'd'bc$. Since by hypothesis these products are equal, we may subtract one from the left side and the other from the right side of equation (4). Thus

$$adb'c' - a'd'bc = a'd'bc - adb'c' . \quad (7)$$

If $ada'd'$ is then added to both sides and $b'c'bc$ subtracted from both sides, then

$$ada'd' + adb'c' - a'd'bc - b'c'bc = ada'd' + a'd'bc - adb'c' - b'c'bc . \quad (8)$$

[23] Under certain conditions which we are now exploring, the solution of the cubic equation yields more than one real root, and hence alternative χ^2 values are possible. It appears that this is infrequent and occurs only under peculiar circumstances.

[24] These are actual data from unpublished research of one of the authors.

Factoring each side, we obtain

$$(ad-bc)(a'd'+b'c') = (a'd'-b'c')(ad+bc). \quad (9)$$

After dividing through by the product $(a'd'+b'c')(ad+bc)$, it is clear that the equality hypothesized in equation (4) has now been transformed to an equality between familiar expressions

$$\frac{ad-bc}{ad+bc}=\frac{a'd'-b'c'}{a'd'+b'c'}.$$

Thus, the test for "second-order interaction" in a $2 \times 2 \times 2$ table is nothing other than a test of the null hypothesis of equal Q's for any pair of 2×2 tables included in the three-dimensional table. It can be shown readily by following these above steps in the reverse direction that equal Q's imply $adb'c' = a'd'bc$. Hence, what has been previously described in quite different language may be interpreted as a test for the significance of the difference between two Q's.[25] Such a test for the significance of difference between Q's has not been generally used in interpreting the relationships in $2 \times 2 \times 2$ tables even though Q has been one of several tools utilized in the analysis of separate 2×2 tables.

TWO TESTS—SIMILAR OR OPPOSING CONCLUSIONS?

In his modification of the Dorn-Tibbitts-Stouffer method, Goodman indicates that the difference of differences in proportions "can be viewed as a measure of a certain aspect of the association present in the $2 \times 2 \times 2$ cross-classification table," but adds that a number of different aspects of association may be of interest.[26] Furthermore, Goodman suggests that these differences are too important to be ignored by the serious research worker.[27] But the nature of the differences among techniques used for analysis of $2 \times 2 \times 2$ tables is not always evident. Such is the case for the difference of differences between proportions and the χ^2 test for second-order interaction in $2 \times 2 \times 2$ tables. Since the literature has been unclear regarding the differences between the two methods, it might appear that the two methods could be used interchangeably. It is now clear that under certain conditions the two techniques will lead the researcher to opposite conclusions. It is relevant, therefore, to know under what circumstances the two techniques will differ most, and given these circumstances, what other characteristics of a data-analysis problem are relevant to the choice between the two.

For very small samples, the two techniques will almost invariably yield an identical decision to fail to reject their respective null hypotheses. This does not, of course, indicate that their respective measures of relationship—difference between proportions and Q's—converge in some sense as samples become small. It simply indicates that with very small samples it is, in general, hazardous to conclude that the relationship of variables in the *pair* of 2×2 tables varies significantly, whatever the outcome. Furthermore, when there is no relationship in *either* of the two tables as measured by Q, there will be no relationship in either table as measured by the difference between proportions, and vice versa. Therefore, the two techniques will invariably be in agreement when the relationship is zero in both tables. However, the Q's in two tables may be identical even though there is a difference between differences between proportions, and vice versa. The degree of divergence between a comparison of Q's and a comparison of differences between proportions depend upon a characteristic of the tables to which we are ordinarily not attentive.

To understand the conditions under

[25] Utilizing a different line of reasoning, Goodman arrived at the same conclusion (see Goodman, *op. cit.*, 1963, p. 100).

[26] Goodman, *op. cit.* (1961), p. 363.

[27] *Ibid.*, p. 356.

which the two techniques lead the researcher to different conclusions, it is necessary to establish their relationships to one another. The two measures of relationship, difference between proportions (d_1) and Q, may quite readily be expressed as functions of one another. Beginning with the definition for the difference between proportions

$$d_1 = \frac{a}{a+b} - \frac{c}{c+d},$$

the simple operations of finding a common denominator, clearing fractions, and writing out the product of the original denominators yield

$$d_1(ad+bc+ac+bd) = ad - bc .$$

A few additional operations allow us to express Q as a function of the difference, d_1, between proportions

$$d_1(ad+bc) + d_1(ac+bd) = ad - bc ;$$

$$d_1 + d_1\left(\frac{ac+bd}{ad+bc}\right) = \frac{ad-bc}{ad+bc};$$

$$d_1\left(1+\frac{ac+bd}{ad+bc}\right) = Q .$$

Clearly, d_1 may also be expressed as a function of Q:

$$d_1 = \frac{Q}{1+[(ac+bd)/(ad+bc)]}.$$

Considering pairs of differences between proportions and pairs of Q's, it is evident from the above that if d_1 and d_2 are equal, the corresponding Q's will be equal if and only if the ratios $(ac + bd)/(ad + bc)$ for each table are identical. In general, the greater the discrepancy between this ratio for the two tables, the greater the discrepancy between the comparison of the two Q's and the comparison of the two differences between proportions as indicators of the constancy or persistence of the relationship. Hence, to the degree that these ratios are divergent for the two tables, the two tests for the constancy of a relationship under the two conditions will be sensitive not only to different but to divergent aspects of the same $2 \times 2 \times 2$ table.

Two hypothetical cases will illustrate the way in which certain concentrations of cell frequencies in $2 \times 2 \times 2$ tables can lead to different results when the two techniques are utilized and compared. These cases are presented in Figure 3.

The choice of the test for evaluating the constancy or persistence of a relationship between the variables under varying conditions of a third variable would be critical in both cases presented in Figure 3. In the first hypothetical case of Figure 3, the χ^2 value for the test between Q's with one degree of freedom is 31.24 with a probability that differences of this order would occur less than .00001 if in fact the true difference were zero. But for this same $2 \times 2 \times 2$ table, the test statistic for the significance of difference between differences between proportions is precisely zero.

An equally striking difference in the results of the two tests appears for Case B of Figure 3. In this illustration the χ^2 value is zero, but the difference between differences of proportions for the two 2×2 tables is 0.20. In probability terms we would conclude that a difference of this magnitude and for samples of this size would be likely to occur much less than .0001, if in fact the true difference were zero.

There are, of course, a number of other patterns of concentration which could present divergent results for the two tests under consideration.[28] Consider five such patterns of concentration as the following: concentration in diagonally opposite cells, in a single row, in a single column, in a single cell, or in three cells while the fourth is vacant or nearly so. As a general guide, a table with any one of these five basic patterns of concentration will form a $2 \times 2 \times 2$ table for which the two tests considered here will yield similar results when: (*a*) paired with another table of

[28] B. N. Lewis, *op. cit.*, pp. 106–10.

the same general pattern (but not necessarily showing the same degree of relationship) or (*b*) paired with another table showing no relationship. On the other hand, a table with any one of these five basic patterns of concentration will form a $2 \times 2 \times 2$ table for which the two tests *may* yield different results when paired with another table of any other basic pattern of concentration, depending, of course, on the degree of difference between the patterns of concentration in the two tables and upon *n*.

CASE A

FEMALES

Social Approval from Supervisor	Work Satisfaction: High	Low	Proportion of High to Row Total
High	950	50	0.95
Low	650	350	0 65
		Difference	0 30

MALES

Social Approval from Supervisor	Work Satisfaction: High	Low	Proportion of High to Row Total
High	550	450	0.55
Low	250	750	0 25
		Difference	0 30

$\chi^2_{2\times2\times2} = 31.24$, 1 degree of freedom, $p < .00001$.

$d_1 - d_2 = 0.0$.

CASE B

BLUE-COLLAR WORKERS

	Work Autonomy: High	Low	Proportion of High to Row Total
Females	200	750	0 21
Males	160	40	0.80
		Difference	−0 59

WHITE-COLLAR WORKERS

	Work Autonomy: High	Low	Proportion of High to Row Total
Females	200	160	0 56
Males	750	40	0 95
		Difference	−0.39

$\chi^2_{2\times2\times2} = 0.0$.

$d_1 - d_2 = 0.20$, $p < .0001$.

FIG. 3.—Two hypothetical cases which illustrate how alternative techniques lead to opposing conclusions

NOTES ON APPLICATION

Since the two techniques can lead to very different conclusions, the choice between them may be of crucial importance. Both techniques may be viewed as tests for the significance of difference between two measures of relationship—in the one case, between two *d*'s and in the other case between two *Q*'s. The choice between the two techniques should therefore depend on which of these two measures of relationship is most appropriate for the data being analyzed. Criteria for choice among various measures of relationship remain to be unambiguously and consensually defined. Goodman and Kruskal, however, have suggested as one relevant and cogent criterion that measures of relationship should have an "operational interpretation."[29] Adopting this criterion as a starting point, the choice of a measure of relationship is then largely a matter of choosing the measure that yields an "operational interpretation" most directly relevant to one's purposes.

[29] Leo A. Goodman and William H. Kruskal, "Measures of Association for Cross Classifications," *Journal of the American Statistical Association*, XLIX (December, 1954), 740–42.

The measure Q may be given a clear interpretation for ordinal data. Given ordinal data, Q, (or, more generally, γ) indicates the proportional reduction of error in the predictability of order as one shifts from ordering pairs of units on variable b by a random device to ordering pairs of units on variable b according to knowledge of their ordering on variable a. Referring to a case A of Figure 3, the Q for females is 0.82, while the Q for males is 0.57. Therefore, by taking into account the order of pairs of women on the variable "supervisory approval," the predictability of order of pairs of women on the variable "work satisfaction" can be improved over random ordering by 82 per cent. For men, the corresponding improvement in predictability is only 57 per cent.[80] The χ^2 test for second-order interaction indicates that these two degrees of improvement in predictability are so different from one another that it is unlikely that the difference between the two is simply a result of sampling variability.

Shifting attention to case B in Figure 3, the Q's for the two tables are both 0.875, indicating that the relative improvement in the predictability or order must be identical for both tables. For such an interpretation, in terms of the predictability of order, to be appropriate it is, of course, necessary to conceive of the categories of "female" and "male" as ordered in some sense. Dichotomies, however, may be readily conceived as ordered even if the categories are ordinarily conceived simply as distinct classes. Male and female may be conceived as ordered from high to low on "maleness" or general status, for example. But where it appears inappropriate for any reason to conceive of the dichotomies as ordered, it would probably be wise to consider measures other than those discussed in this paper, for example, Goodman and Kruskal's τ_b or their λ_b.[81]

Several debits appear in any attempt to give operational meaning to the measure d and hence to the use of a test for the significance of the difference between two d's. Although d may be given an "operational interpretation" in terms of "net profit,"[82] that interpretation is not wholly

[80] The interpretation of Q (or γ) in terms of the improvement in the predictability of order refers to all pairs of units that involve no tie on either variable, i.e., all pairs in which order on both variables is determinate. As the cutting point dividing a continuum into a dichotomy shifts—or, more descriptively, as the degree of skew for a dichotomized distribution shifts—the number of pairs involving no ties changes also. In a 2×2 table where the marginal distribution on one or both variables is highly skewed, tied pairs are numerous, and Q will be based on the improvement in predictability for a relatively small proportion of all possible pairs. Although this might be considered a "disadvantage," it should be noted that a test of significance for Q or a test of the significance for the difference between Q's (such as the χ^2 test for second-order interaction discussed here) takes this into account, i.e., sampling variability is greater where skew is greater, other things being equal (see Leo A Goodman and William H. Kruskal, "Measures of Association for Cross Classifications: III. Approximate Sampling Theory," *Journal of the American Statistical Association*, LVIII [June, 1963], 310–64, for estimates of the variance of γ [or Q]).

[81] To be useful in the context of the present discussion, i.e., in comparing the degree of relationship under two different conditions, it must be possible to test for the significance of the difference between two such measures. Although sampling theory for these measures has been developed (see Goodman and Kruskal, *op. cit.*, 1963, for some relevant estimates of sampling variability), some difficulties remain. For example, referring to case B of Table 3, λ_b (predicting autonomy) for white-collar workers is zero. With presently available estimates for sampling variability, it is not possible even to set up a confidence interval for a λ_b of zero.

[82] The difference between proportions may be operationally interpreted, without any reference to ordinal features of the data, in terms of the net gain or loss made possible by the predictability implied by the relationship. If the gain from a correct anticipation and the loss from an incorrect anticipation are both equal to some specified "cost unit," and if the number of cases in the two rows is identical, then an estimate of the "net profit" (i.e., gain minus loss) that may be realized

satisfactory for several reasons. For most sociological problems in which it is difficult to conceive of assigning "cost units" to the gain realized from a correct anticipation and the loss suffered from an incorrect anticipation, an operational interpretation in terms of "net profit" appears inappropriate and perhaps meaningless. Furthermore, even if these terms can be defined in ways appropriate to certain sociological problems, the measure *d* requires the assumption of equal numbers in the rows (given independent variable as the row variable) if *d* is to be useful in computing "net profit." And the measure *d* indicates the "net profit" that may be realized by following a particular kind of anticipation rule which is not, in general, the most accurate anticipation rule. The present authors regard these as serious limitations on the interpretability of *d* for sociological purposes, in spite of its apparent simplicity. Needless to say, the same considerations bear on the utility of a test for the significance of the difference between *d*'s.

With these assumptions, a criterion for the choice between the two tests discussed in this paper may be formulated. If the variables in the 2×2 tables are ordinal, *Q* appears to be appropriate. The measure *Q* is operationally interpretable for ordinal data and, therefore, the χ^2 test for the significance of differences between two *Q*'s has also an operational interpretation. However, this argument cannot be extended to nominal data. Hence the test for the significance of difference between differences between proportions appears to be preferable for nominal data or for problems where ordinal features of the data are not substantively important.

A further consideration in the choice of the two tests stems from the fact that the two tests are differentially sensitive to minor shifts in frequencies under certain conditions. Specifically, if one cell in one of the tables has a very small frequency while the other three have very large frequencies, a slight change in the small cell will have a relatively slight effect on the difference between proportions but will have a more marked effect on *Q*. Thus, under certain conditions, slight changes in the frequency of a single cell may change the decision based on the χ^2 test, while leaving the decision based on Goodman's test unaffected.

SUMMARY

This paper has described, illustrated, and compared two alternative techniques which may be employed in the multivariate analysis of three dichotomized variables. In the process of demonstrating the differences between the test for the significance of the difference between differ-

by utilizing knowledge of one variable in a differential anticipation of the other is indicated by the following function of *d*: "Net profit" $= |d|n$ (cost unit), where *d* is the difference between proportions of row totals if the predictor or independent variable is represented in rows, *n* is the number of cases for which anticipation is to be made, "cost unit" is the gain from a correct anticipation and/or the loss from an incorrect anticipation. Referring to case A of Figure 3, the *d* value is 0.30 for both males and females, indicating that for the 2,000 cases of each sex represented there, by anticipating high work satisfaction when social approval is high and low work satisfaction when social approval is low, a "net profit" of 600 cost units would be realized for females and another "net profit" of 600 cost units would be realized for males. In this case it is evident without a significance test that the difference between differences between proportions is not significant, since the difference between *d*'s is exactly zero and therefore the "net profit" for the two tables by the above anticipation rule is identical if the cost units are identical. It should be noted, however, that the anticipation rule used in realizing this "net profit" (i.e., the rule to anticipate high satisfaction when approval is high and low satisfaction when approval is low) does not yield the maximum possible "net profit" that may be realized. A greater "net profit" may be realized for females (but not for males) by anticipating high satisfaction for all, regardless of degree of supervisory approval. The measure *d* is not sensitive to such a possibility.

ences between proportions and the chi-square test for second-order interaction, a reinterpretation of the latter test was introduced, thus making available a test for the significance of difference between two Q's. Through the further introduction of a simple algebraic argument and the presentation of a set of illustrative tables, it has been shown that when the cell frequencies are concentrated in a particular way in a $2 \times 2 \times 2$ table, the two alternative techniques produce contrasting results and are likely, in the absence of other information, to lead the researcher to opposing conclusions. These findings suggest that, in order to select the appropriate technique, careful attention should be given to the nature of the variables and to the operational interpretations of the potential outcomes of the two tests.

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RESEARCH NOTES

A Test of Significance for the Homophily Index[1]

A technique for directly determining the statistical significance of the homophily index has been developed in the process of evaluating data on friendship choices among student dentists.[2] The technique is simple and yields reasonably accurate estimates of significance, even for as few as ten choices per attribute.

As described by James Coleman,[3] the computation of the index of homophily demands that the data on the round of choices within a given sample be conceived of as arranged in a 2 × 2 table. This is illustrated in Figure 1. The cells record the number of choices given by possessors of a particular attribute to those who share and those who do not share this attribute. For example, if a chooser with characteristic C_1 selects an individual with characteristic C_2, this is recorded as a component of x_8.

The index comprises a means of measuring the extent to which choosers with one attribute tend to overchoose or underchoose individuals with the same attribute as compared to their choices of individuals with the alternate attribute. Note that m_1 is the total number of choices made by individuals in the sample who have characteristic C_1, and not the number of *individuals* with such characteristics. The numbers of individuals are given as n_1 and n_2 for those who have C_1 type and C_2 type characteristics, respectively; thus $n_1 + n_2 = N$, the sample size.

A separate index is computed for two types of choice situations: where choosers *over*choose their attribute similars and where choosers *under*choose attribute similars. These two situations are defined by

$$h_i = \frac{x_i - e_i}{m_i - e_i}, \qquad (1)$$

when $x_i \geq e_i$ $(i = 1, 2)$, and

$$h_i = \frac{x_i - e_i}{e_i}, \qquad (2)$$

when $x_i < e_i$ $(i = 1, 2)$.

Chosens with Characteristic:

	C_1	C_2	
Choosers with Characteristic: C_1	x_1	x_3	$m_1 = x_1 + x_3$
C_2	x_4	x_2	$m_2 = x_2 + x_4$

FIG. 1.—The homophily matrix

The symbol e_i refers to the expected value of x_i, if it were presumed that there were no forces at work which would influence the overchoice or underchoice of like characteristics. This value is defined by

$$e_i = \frac{n_i - 1}{N - 1} m_i. \qquad (3)$$

Coleman also gives a short form for computing e_i when N is large:

$$e_i = \frac{n_i}{N} m_i. \qquad (4)$$

[1] The authors are grateful to Dr. James Coleman of Johns Hopkins University for his critical reading of the original manuscript of this paper.

[2] Robert M. O'Shea, "Friendship Relations in the Dental School" (unpublished Ph.D. dissertation, Department of Sociology, Catholic University of America).

[3] "Relational Analysis: The Study of Social Organizations with Survey Methods," *Human Organization*, XVII (Winter, 1958), 28–36. The reader should be warned that the notation used here does not conform to Coleman's symbols. This departure was prompted by the need to simplify the presentation of computing formulae.

It should be clear from the above formulae that the value of h_i is completely defined by a two-cell table in which m_i distinct things have a probability, $p_i = (n_i - 1)/(N - 1)$, of occupying cell C_iC_i, and a probability of $q_i = 1 - p_i$ of occupying the remaining cell.

Since our question of statistical significance asks: What is the probability that a given value of h_i, or a value larger, could be obtained, assuming no relationship between the characteristics of chooser and chosen in the selecting process, the problem can be translated into terms of values of x_i, $(i = 1, 2)$:[4]

$$P(h_i \geq v) = P[(k_1x_i - k_2) \geq v] = P(x_i \geq a), \quad 0 \leq a \leq m_i. \quad (5)$$

As a varies from 0 to m_i, $P(x_i = a)$ describes a binomial distribution of the form,[5]

$$\binom{m_i}{a} p^a q^{m_i - a}. \quad (6)$$

It is well known that the binomial distribution tends to approximate the normal distribution as the sample size increases.[6] This allows us to resort to tables of the normal distribution in determining the probabilities we seek.

The only remaining problem is to discover the parameters μ and σ with which to enter the tables of a normal distribution. David[7] shows that the mean and variance of the binomial distribution are homologous to the same parameters of the normal distribution. The mean of a binomial distribution is given by np, and the variance by npq, for a distribution of the form

$$\binom{n}{k}$$

with probabilities $p + q = 1$.

Applied to our distribution,

$$\binom{m_i}{a},$$

the mean is m_ip_i, and the variance is $m_ip_iq_i$. But $p_i = e_i/m_i$; therefore, the parameters are defined by

$$\mu_i = m_i \frac{e_i}{m_i} = e_i \quad (i = 1, 2); \quad (7)$$

$$\sigma_i = \sqrt{\left[e_i\left(1 - \frac{e_i}{m_i}\right)\right]} = \sqrt{\left(e_i - \frac{e_i^2}{m_i}\right)}. \quad (8)$$

The distance of a given value, a, from the mean in standard deviation units, z, can be computed as follows:

$$z_i = \frac{a - e_i}{\sqrt{\left(e_i - \frac{e_i^2}{m_i}\right)}}. \quad (9)$$

To obtain a better fit to the normal curve, David further suggests a "correction for continuity" in computing z:[8] (1) If computing $P(x_i \geq a)$, the correction is $-\frac{1}{2}$. (2) If computing $(x_1 < a)$, the correction is $+\frac{1}{2}$. For the typical test of significance applied to the homophily index, condition (1) prevails; therefore, a correction of $-\frac{1}{2}$ is called for. It is ap-

[4] In the case where

$$x_i \geq e_i, \; h_i = \left(\frac{1}{m_i - e_i}\right) x_i - \frac{e_i}{m_i - e_i}.$$

For any one computation of h_i, m_i and e_i are constant, since our problem is, given values of m_i and e_i, what is the probability of obtaining values of h_i greater than or equal to any *given* values of h_i? Therefore, for our purposes, $h_i = k_1x_i - k_2$, with k_1, k_2 constant, and h_i is seen to be a linear function of x_i. This same argument, *mutatis mutandis*, will hold for the case where $x_i < e_i$.

[5] F. N. David, *Probability Theory for Statistical Methods* (Cambridge: Cambridge University Press, 1949), pp. 24, 25.

[6] *Ibid.*, chap. v: "Replacement of the Binomial Series by the Normal Curve," pp. 47–57.

[7] *Ibid.*, pp. 31, 32.

[8] *Ibid.*, pp. 54, 55. It should be noted that z_i^2, without the correction for continuity, is the same as χ^2 for a two-celled distribution, one degree of freedom, providing p_i is estimated as e_i. It follows that a χ^2 table can be used by squaring z_i. Generally, the one-tailed test would prevail.

plied in the difference-from-the-mean computation, and results in

$$z_i = \frac{a - e_i - \frac{1}{2}}{\sqrt{\left(e_i - \frac{e_i^2}{m_i}\right)}} . \qquad (10)$$

If the short form of computing e_i (equation [4]) is used, $p_1 = n_1/N$, $q_1 = p_2 = n_2/N$. Therefore, $p_1 = n_1/N$, $q_1 = p_2 = n_2/N$, and equation (10) can be written

$$z_i = \frac{N(a - e_i - \frac{1}{2})}{\sqrt{(m_i n_1 n_2)}} . \qquad (11)$$

To determine $P(x_i \geq a)$ one need only enter the usual tables of areas under the normal curve with the computed z value.

Since the binomial distribution is symmetric only for $p = q = \frac{1}{2}$, other values of these probabilities will naturally fit the normal curve less well, because they will define a distribution which will be comparatively skewed to one side or the other of the normal mean. In general, as David observes, the lower the significance level chosen, the greater the chance of error in the normal approximation, while the true binomial probability areas would be less than those of the normal curve when $p < \frac{1}{2}$ and greater when $p > \frac{1}{2}$ (assuming, of course, that we are interested in the areas under the right-hand tail of the curve).[9]

At this point it would be well to insert a note of caution. First of all, the test of significance is really a test of how well actual data fit a given model, what one might call a "chance" model. Here we are using a model in which *all* the choices are assumed both independent and of equal probability in each class (characteristic), which is to say, all choices within a class are attracted with an equal force—or valence—whether they be choices of individuals or sequential choices (first, second, third, etc.) on given individuals. Now it might be argued that it is not reasonable to test multiple-choice data against a model which is based on the independent probabilities of all of an individual's choices. We feel justified in using the model for the following reasons.

We are aware of no a priori arguments which would make the dependence of sequential sociometric choices any more acceptable than their independence. Although individual effects may be dependent, which is problematic, the effects for a whole population may be random. Therefore, as sample size increases, there should be a tendency for dependence effects to disappear. Unfortunately, we are not able to offer a proof of this statement.

Perhaps our model can be defended more satisfactorily by considering the fact that it is already in use. When we solicit serial choices from a respondent we are, in effect, asking him to *rank* others in his field. Considered in this way, the model can be seen as similar in pertinent respects to the one employed by M. G. Kendall in developing a test of significance for the rank correlation coefficient τ. Kendall's model assumes both equal and independent probabilities for all possible rankings in the non-tie case.[10]

Nevertheless the problem of independence does complicate the use of the present test, since a significant difference between our data and our model could be due to the non-independence of ranked sociometric choices rather than to direct sociometric attractive-repulsive forces. This possibility would present some difficulty should a researcher wish to distinguish these two effects. In such a case, we would suggest either a separate testing of choices of each rank (for example, all first choices tested, then all second choices, etc.), or that the rank-dependence effects be ignored when the number of choices is small compared to the sample size. Although it would be quite a task to prove it, it does seem reasonable to assume that the potential dependence effects of ranked choice will

[9] For further discussion of this see David, *op. cit.*

[10] *Rank Correlation Methods* (New York: Hafner, 1962), chap. iv.

be minimal when the number of choices per chooser is small in proportion to the sample size. As a rule of thumb, we suggest that the smaller the N, the fewer the number of choices per chooser that can be safely used in a test of significance using our model *if* the researcher does not want to confuse rank effects with

	0–2	3–5	
0–2	53 ($e_1 = 38$)	13	$m_1 = 66$, $n_1 = 30$
3–5	16	38 ($e_2 = 23$)	$m_2 = 54$, $n_2 = 22$

$$h_1 = \frac{x_1 - e_1}{m_1 - e_1} = 0.54\,; \quad h_2 = \frac{x_2 - e_2}{m_2 - e_2} = 0.39$$

FIG. 2.—Distribution of 120 friendship choices among 52 students in a dental school, based on categories of alphabetic position.

choice effects. The stringency of this rule could be alleviated by an estimate of the degree to which dependence fails to introduce a *systematic* bias—perhaps a cogent guess based on one's experience with the data.

We supply two examples from a study done by one of the authors to illustrate the use of the technique. The distribution shown in Figure 2 yields an h_1 of 0.54 and an h_2 of 0.39.[11]

In order to determine the significance of these two indices we must compute the respective values of z. To do this, we must know the values of N, a, e_i, m_i, n_1, and n_2. These can be determined from the table as:

$N = 52$
$a = 53$ or 38 (for h_1 and h_2 computations respectively)
$e_1 = 38$, $e_2 = 23$
$m_1 = 66$, $m_2 = 54$
$n_1 = 30$, $n_2 = 22$

[11] Equations (4) and (11) were used in these computations.

Employing the formula for z, we write

$$z_1 = \frac{52(53 - 38 - \frac{1}{2})}{\sqrt{(66 \cdot 30 \cdot 22)}} = 3.60\,;$$

$$z_2 = \frac{52(38 - 23 - \frac{1}{2})}{\sqrt{(54 \cdot 30 \cdot 22)}} = 3.98\,.$$

Any table giving the area under the normal curve, to the right or the left of a point which is z standard deviation units from the mean, will reveal that these two computed z-values are well below the .001 "significance" level (for h_1, $p = .0002$; for h_2, $p < .0001$).

A contrasting case is shown by the data in Figure 3. Here, the z-values are $z_1 = 0.57$ and $z_2 = 1.64$. Consulting a table, we

	1–3	4–5	
1–3	58 ($e_1 = 55$)	26	$m_1 = 84$, $n_1 = 34$
4–5	27	23 ($e_2 = 17$)	$m_2 = 50$, $n_2 = 18$

$h_1 = 0.10$ $\qquad h_2 = 0.18$

FIG. 3.—Distribution of 134 friendship choices among 52 students in a dental school, based on categories of age.

learn that h_1 is not very "significant" ($p = .2843$), while h_2 is "significant" at about the 5 per cent level ($p = .0505$).[12]

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[12] It is interesting that the testing of h_1 and h_2 separately leaves one with the option of rejecting a statement concerning one channel of choices (for example, individuals with C_1 attribute choosing individuals with the same attribute), while accepting the other (those with C_2 attribute choosing their attribute-mates).

Race and Social Class as Separate Factors Related to Social Environment[1]

There have been numerous studies in which some measure of social class has been used to identify and classify the backgrounds of a sample of respondents. Myrdal,[2] however, has pointed out that race also plays a significant part in defining the social condition of individuals. Thus, regardless of his class level, a Negro may experience less access to such societal features as good housing, educational facilities, and job opportunities than does his white counterpart. It would seem relevant, therefore, to evaluate *both* race and social class as separate factors determining social environment. Unfortunately, many previous studies have not been able to carry out such an evaluation satisfactorily because their sample designs have often confounded race and class. This problem arises from the difficulties in finding Negro and white respondents distributed in similar proportions in each social class.

The present study made an effort to overcome this problem by means of a sample in which race and social class are empirically independent of each other. It is hoped that the data in this study will clarify the following issues: (1) the similarities and differences in the patterns of association which race and social class hold with a number of attitudinal and environmental variables; and (2) the extent to which race and social class jointly interact in defining and influencing environmental conditions and attitudes. For the latter problem, interest is not centered upon the simple associations between race or social class and the dependent variables. Rather, the concern is with the extent to which the associations between class and the dependent variables show differential patterns from race to race. Fourteen dependent variables were studied, but only those which showed statistically significant associations will be reported. Those variables not showing significant relationships dealt with the respondents' exposure to mass media.

RESEARCH PROCEDURES

The sample consisted of children and parents who were involved in an ongoing research project on cognitive processes. The group of children consisted of approximately equal numbers of Negro and white boys and girls in the first and fifth grades. The sampling procedures aimed at securing a study group in which the social-class level would be adequately represented within each race. Therefore, in selecting the sample, schools located within census tracts with known socioeconomic characteristics were used to identify likely areas containing families of certain social and racial groups. The school records then were examined to provide a list of students from which the sample was randomly selected.

A total of 340 pairs of parents and children was originally chosen. This N was reduced to 292 as a result of the inability to complete interviews with either the parent or the child. Chi-square analyses of this sample indicated that the lack of an association between race and social class was maintained.

The social-class standing of the sampled

[1] This article is a slightly expanded version of a paper read at the annual meeting of the American Psychological Association, 1963. These data were collected as part of grants given to the Institute for Developmental Studies, New York Medical College, by the Taconic Foundation and by the National Institute of Mental Health.

[2] G. Myrdal, *An American Dilemma* (New York: Harper & Bros., 1944).

households was measured by an index consisting of two equally weighted components: the educational level of the main support of the family and a prestige rating of his (her) occupation based on the North-Hatt study.[3] The distributions of the scores for these components were totaled for a combined index value. The distribution of these totaled scores was then trichotomized into the following levels.

Level I.—A typical family in this group has as a main wage-earner a person who is unemployed, or who has an unskilled or semiskilled job. The educational level is likely to range from elementary schooling to completion of the eighth grade.

Level II.—The typical family at this level is headed by a wage-earner with a semiskilled, clerical, or sales job whose education ranged from about nine grades to high-school graduation.

Level III.—The typical household is headed by a professional or managerial wage-earner whose education would be high-school graduation, college, or graduate work.

The data were based primarily on personal interviews with the children and responses by parents to a questionnaire which was mailed to them. An effort was made to interview as many as possible of the parents who did not return the forms. For these interviews, the same questions were used as those in the mailed questionnaire. No differences in the distributions of responses were noted between parents answering the questions by mail or in the home interview.

The format for both the parental and child questionnaires consisted primarily of questions requiring the respondent to supply specific information; there were also some open-ended questions. Most of the data were obtained from a parent. In a few instances, children supplied the data. Each table indicates which source was used. The adequacy of coding responses to the open-ended questions was determined by having independent coders recode a 20 per cent random sample of the questionnaires. The reliability check showed an agreement of at least 90 per cent between coders.

[3] A. J. Reiss, *Occupations and Social Status* (New York: Free Press of Glencoe, 1962).

RESULTS AND DISCUSSION[4]

Table 1 presents the percentages for each dependent variable found to be significantly associated with either or both social class and race. Table 2 lists the significance of these associations.

There are two characteristics in which Negroes (regardless of class) and the Level I group (regardless of race) show the same direction of association. Thus the housing of Negroes and Level I respondents is rated as more dilapidated than is the housing of others. (Table 1, B). Similarly, there is a higher incidence of the father being absent from the home in the Negro and Level I samples (Table 1, A). As race and class are empirically independent of each other in the sample, it is possible to suggest explanations for these results. The quality of housing may be determined by patterns of segregation in the case of the Negro, or by poverty for the Level I group. The absence of the father may reflect a historically based matriarchal tradition in the Negro family[5] or an unstable family organization resulting from the stress of adverse economic conditions in the lowest social group.

[4] The results are analyzed by a χ^2 model appropriate for complex contingency tables. In essence, this model allows for the partition of the total χ^2 and degress of freedom into components analogous to factorial analysis of variance designs. In this study, the total χ^2 is partiitioned into separate tests of association of race and social class with a given dependent variable. In addition, there is also a test of the combined association of race, social class, and the dependent variable. A thorough discussion of the use of χ^2 for multiple classification designs is provided by J. P. Suttcliffe, "A General Method of Analysis of Frequency Data for Multiple Classification Designs," *Psychological Bulletin*, LIV (March, 1957), 134–37.

[5] E. F. Frazier, *The Negro Family in the United States* (rev. ed.; New York: Dryden Press, 1942).

TABLE 1

Dependent Variables Found Significantly Associated with Race or Social Class (Per Cent)

A. FATHER PRESENT IN THE HOME*

	All %	Class Level I %	II %	III %
All	85 (281)	72 (106)	90 (102)	97 (73)
Negro	78 (143)	62 (55)	85 (55)	93 (33)
White	92 (138)	82 (51)	96 (47)	100 (40)

* Data source: parental response.

B. DILAPIDATED HOUSING†

	All %	Class Level I %	II %	III %
All	63 (292)	77 (109)	54 (106)	55 (77)
Negro	70 (153)	78 (58)	64 (58)	68 (37)
White	56 (139)	76 (51)	44 (48)	45 (40)

† The data were derived from the 1960 Census tracts' rating of housing adequacy. Inasmuch as the housing ratings were available for blocks within Census tracts but not for specific household units, a mean weighted index was computed from the distribution of housing units rated as falling into the categories of sound versus varying degrees of dilapidation. A household having an index of three or above was classified as dilapidated.

C. PARENTAL EDUCATIONAL ASPIRATIONS FOR CHILD; DESIRES SOME DEGREE OF COLLEGE TRAINING‡

	All %	Class Level I %	II %	III %
All	94 (288)	88 (108)	96 (106)	98 (74)
Negro	97 (150)	96 (57)	97 (58)	100 (35)
White	89 (138)	79 (51)	95 (48)	97 (39)

‡ Responses were dichotomized between parents who desire their children to have a high school education versus those who desire college training (including college graduation and post-graduate work). No one in the parental sample indicated aspirations below high school.

D. PARENTS DESIRING HIGH-STATUS OCCUPATIONS FOR THEIR CHILDREN§

	All %	Class Level I %	II %	III %
All	88 (244)	75 (95)	94 (88)	97 (61)
Negro	93 (129)	86 (51)	96 (50)	100 (28)
White	82 (115)	64 (44)	92 (38)	94 (33)

§ The occupations were rated on a ten-point scale as derived from the research of North and Hatt (see n. 3). Occupations rated seven and above were categorized as high-status positions.

E. CHILD'S OWN OCCUPATIONAL ASPIRATIONS; DESIRES A HIGH-STATUS OCCUPATION§

	All %	Class Level I %	II %	III %
All	70 (229)	55 (73)	72 (84)	81 (72)
Negro	78 (96)	65 (26)	84 (37)	82 (33)
White	64 (133)	49 (47)	66 (47)	80 (39)

F. PARENTAL PERCEPTION OF OWN STATUS AND CONDITIONS IN SOCIETY: THOSE PERCEIVING THEIR STATUS AS IMPROVING‖

	All %	Class Level I %	II %	III %
All	65 (283)	53 (107)	68 (103)	79 (73)
Negro	76 (149)	68 (57)	72 (57)	94 (35)
White	54 (134)	36 (50)	63 (46)	66 (38)

‖ Parental responses were dichotomized between those who perceived their status in society as improving versus those who perceived their status as remaining the same or going down.

TABLE 1—*Continued*

G. CROWDEDNESS IN THE HOUSEHOLD‡

	All %	Class Level I %	II %	III %
All	36 (292)	47 (109)	37 (106)	20 (77)
Negro	35 (153)	57 (58)	33 (58)	5 (37)
White	37 (139)	35 (51)	42 (48)	33 (40)

‡ Based on a person-to-room ratio as obtained from information provided by the parent. A household with a ratio of 1.5 or greater was classified as crowded.

H ADULT RESPONDENTS HAVING UPWARD OCCUPATIONAL MOBILITY**

	All %	Class Level I %	II %	III %
All	17 (243)	9 (82)	20 (97)	23 (64)
Negro	15 (130)	11 (44)	13 (54)	22 (32)
White	20 (113)	5 (38)	28 (43)	25 (32)

** The main breadwinner in a household was classified as upwardly mobile if his current job was rated as more prestigeful (by the North-Hatt scale) than his first permanent job. The table represents a dichotomy between respondents having upward occupational mobility versus those whose status has remained the same or has gone downward.

I. CHILDREN WHO HAVE A NUTRITIONALLY ADEQUATE BREAKFAST††

	All %	Class Level I %	II %	III %
All	25 (287)	17 (106)	27 (105)	34 (76)
Negro	25 (151)	19 (58)	30 (57)	28 (36)
White	25 (136)	15 (48)	23 (48)	40 (40)

†† Data were obtained from reports by the children of the kinds of foods they had for breakfast on the day they were interviewed A nutritionally adequate breakfast was defined as consisting of juice or fruit, milk, cocoa, or tea, and one or more of the following—cereal, eggs, cheese, meat, and bread.

J. MOTHER'S PRESENCE WITH THE CHILD AT BREAKFAST‡‡

	All %	Class Level I %	II %	III %
All	32 (287)	28 (107)	34 (104)	36 (76)
Negro	25 (150)	23 (56)	26 (58)	25 (36)
White	40 (137)	33 (51)	43 (46)	45 (40)

‡‡ Each child was asked to recall whether or not he had breakfast with his mother on the day preceding the interview.

K. PARENTS PERCEIVING THEIR CHILD AS READING ADEQUATELY FOR HIS AGE§§

	All %	Class Level I %	II %	III %
All	79 (256)	71 (94)	81 (90)	86 (72)
Negro	73 (131)	66 (50)	78 (46)	77 (35)
White	85 (125)	68 (44)	84 (44)	95 (37)

§§ Each parent was asked whether child was reading satisfactorily for his age.

A second group of findings concerns the similarity of patterns of association between Negroes and the Level III respondents. The results contradict the stereotype of the Negro parent being intrinsically less motivated for his child than his white counterpart. As shown in Table 1, C and D, Negro parents, regardless of class level, and Level III parents, regardless of race, show similar aspirational patterns. Thus Negro parents express significantly higher occupational and educational aspirations for their children than does the white group as a whole. This trend is also reflected by the Negro children themselves (Table 1, E) whose occupational aspirations are significantly higher

than those of the white children. Somewhat related to the above pattern, Table 1, F, indicates that, in comparison to the Level I respondents and to all white respondents, Negroes show a significantly higher tendency to perceive their condition in society as improving.

These findings may be one manifestation of the current push by Negroes for equal rights, self-improvement, and greater integration into American society. The higher aspirations may also reflect an understanding by the Negro that in a "white world" he must aim higher and be better in order to get as far as his white counterpart.

A third set of relationships points to aspects of family life which are class-linked rather than associated with race. G, H, and I of Table 1 show that there are more crowded housing conditions in Levels I and II than in Level III; that there is less actual occupational mobility in Level I than in Level III; and that children in Level I households are less likely to have nutritionally adequate breakfasts than are children in Levels II and III. Such characteristics as these seem to be a product of educational and economic disadvantage rather than anything related to either race. Negro-white differences in these characteristics are not significant. This conclusion would be qualified somewhat with respect to crowdedness of housing (Table 1, G). Here the picture is complicated by the significant interaction between class and race. The greater crowdedness in Level I is especially striking among the Negro respondents. Negroes in Level I have even less living space than would be predicted from knowledge of race and class taken as separate rather than as interacting attributes.[6]

A fourth group of findings (Table 1, J and K) refers to characteristics which are linked to race and not to class. One interesting finding is the significantly greater tendency for Negro than for white mothers not to be present in the home with their children at breakfast.[7] Such an absence is not clearly related to poverty, for the mother's absence is not prevalent in the lower levels. It may reflect a stronger tendency for Negro mothers than for white mothers to work and therefore to have less time for the child. This trend may also be symptomatic of family instability as reflected in the higher incidence of the Negro father not living in the

TABLE 2

SIGNIFICANCE LEVEL OF χ^2 TESTS OF ASSOCIATION

Dependent Variable	Negro-White	Class Level	Race and Class Level (Interaction)
Father present in the home	0.01	0.001	N.S.
Dilapidated housing	.02	.001	N.S.
Parental educational aspirations for the child	.01	.001	0.05
Parent's choice of child's future occupation	.01	.001	N.S.
Child's own occupational aspiration	.02	.005	N.S.
Parental perception of own status in society	.001	.01	N.S.
Crowdedness in the household	N.S.	.001	0.01
Occupational mobility	N.S.	.05	N.S.
Nutritional adequacy of child's breakfast	N.S.	0.01	N.S
Mother's presence with child at breakfast	.01	N.S.	N.S.
Parental estimation of child's reading ability	0.05	N.S.	N.S.

[6] Conversely, Negroes in Level III tend to live in less crowded conditions than their white counterparts.

[7] No significant associations were noted for the presence of the mother at dinner time. In addition, in those cases where the father was living in the home, no significant associations were found in relation to his presence at either breakfast or dinner.

home. A further finding is that proportionately fewer Negro than white parents perceive their children as reading adequately. One interpretation of this result is that because Negro parents have high aspirations for their children (as suggested by some results described above) they are likely to be especially critical of their children's achievement.

A fifth point refers to the paucity of statistically significant interactions between race and social class. Thus, the relationships between social class and various environmental conditions are generally similar in the white and Negro samples. However, the social effects of race and class may combine in defining and influencing social conditions. To use a more familiar example, intelligence and motivation probably contribute to much of the variance in scholastic achievement. Although intelligence and motivation do not necessarily interact, they might combine so that the lowest achievers are those with the least ability and lowest motivation. In a similar way, the cumulative social effects of race and class may be reflected in some of the observed Negro-white differences—for example, housing dilapidation. In this case, a Negro rated in a lower social class is likely to live in highly dilapidated areas not only because of his poverty, but also because of the patterns of housing segregation which exist for Negroes in general.

The relative effectiveness of race and social class in predicting environmental correlates comprises the sixth and final point. The results indicate that the association of environmental conditions with social class tends to be stronger than with race. Of the eleven dependent variables evaluated in this study, eight showed stronger relationships with class than with race. Although it is risky to generalize from magnitude of statistical *significance* to magnitude of statistical *association*, one may tentatively conclude that social class may be a more potent variable than race in predicting environmental and attitudinal factors.

CONCLUSION

The results of this study suggest the complexity of the interrelationship between race, social class, and the specific dependent variables. Reflecting this complexity is the fact that Negroes share some characteristics with Level I individuals, but overlap with Level III in others. In addition, some environmental conditions were found to be associated solely with either race *or* class, but not with both. Further research, using sample designs similar to the one used in this study, is needed to clarify these complexities. Thus, it would be useful to delineate clusters of variables which are unique to either race or social class, as well as specific areas in which these parameters interact, if indeed they do.

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COMMENTARY AND DEBATES

A Note on Negro Alienation

Alienation is a reasonable explanation for the plight of the urban Negro. However James S. Coleman's delineation of the roots of alienation[1] has two weaknesses: (1) it covers a very restricted population, that is, recent rural migrants, and (2) the evidence available does not support his conclusions for even this restricted population.

Southern "feudalism," as conceived by Coleman, implicitly applies to Negro sharecroppers who do not engage in free-market activity and are totally dependent upon their landlords. Tenant farmers, in contrast, are rent-paying entrepreneurs.[2] However, there are no data to indicate that recent migrants are predominately sharecroppers. Some proportion of the rural Negro migrants (albeit an unknown proportion) included tenant farmers, small-farm owners, or employees in occupations other than farming. The evidence generally indicates that recent Negro migrants from the South are young (the highest rate of migration is found among persons 18–25 years of age) and are better educated than their contemporaries who do not migrate.[3] These facts suggest that the most recent migrants should prove to be relatively adaptable to a new life. In addition, migration implies a belief in the manipulability of one's life chances. It would appear to require a feeling of control over one's destiny to sever deep-rooted social ties and move to a new and distant home.

"The white rural hillbilly, whose whole existence has depended upon self-sufficiency," has not made any better adjustment to city life than has the southern Negro migrant.[4] Bruno describes the plight of the southern white migrant, as follows:

> Lacking leadership, organization, or political power, these descendants of the pioneers are a lost people, exploited by landlords, employers, and merchants. . . . With bitterness, some eventually realize that they have landed at the bottom of the pecking order, in the spot occupied by the Negro back home.[5]

If the southern white migrant has the same problems as the southern Negro migrant, perhaps they share something that leads them to their alienated condition. They both appear to be rural people who are unable to adjust quickly and smoothly to the pressures of urban life.[6] It should also be recognized, however, that alienation is an attractive explanation for the plight of *all* urban lower-class Negroes. Three possibly relevant variables, in addition to the rural-urban transition, can be noted.

First, discrimination, which Coleman partly dismisses as an "easy explanation," surely plays an important part in the Negro's life. Discrimination exists in many crucial areas—housing, education, jobs—even in the northern "liberal" cities. The recent violence in New York City, Rochester, and Jersey City gives some indication of the extent of Negro frustration in the urban ghetto.

Second, feelings of dependence, which Coleman characterizes as important in pro-

[1] James Coleman, "Implications of the Findings on Alienation," *American Journal of Sociology*, LXX (1964), 76–78.

[2] Gunnar Myrdahl, *An American Dilemma* (New York: Harper & Row, 1962), 245–50.

[3] C. Horace Hamilton, "The Negro Leaves the South," *Demography*, I (1964), 293–94.

[4] Hal Bruno, "Chicago's Hillbilly Ghetto," *Reporter*, XXX (June 4, 1964), 28–31.

[5] *Ibid.*, p. 28.

[6] *Ibid.*, p. 29.

ducing alienation, are probably fostered more by the manner in which welfare is allocated in the metropolitan North than by the "feudal" system in the South. Welfare has become so pervasive in some urban segments that the phenomenon of second-generation welfare recipients is increasing in our cities.[7] Persons who are born, live, and die under the aegis of welfare, and who have no other life chances to exploit, presumably will have difficulty in developing a sense of independence. Ironically, welfare may be indirectly creating a form of northern-style "feudalism" which is debilitating to feelings of self-sufficiency.

Finally, mediating organizations play a part. The hypothesis that members of "work-based" mediating organizations are less alienated than non-members was largely supported by the findings of Neal and Seeman.[8] There is also abundant evidence that lower-class persons (and hence the Negro population with which we are concerned) are less often members of mediating organizations than persons in the middle and upper classes.[9] Negroes are more likely than whites *not* to belong to any organization.[10] Moreover, some Negro voluntary associations, such as the Black Muslims, preach the Negro's powerlessness against the white population, and hence the necessity for segregated living so that the Negro can control his destiny.

In short, we can view alienation as affecting a large proportion of lower-class urban Negroes, and the roots of alienation can be found primarily in the urban condition and not in the "feudal" background of the Negro urban dweller or his ancestors. The variables presented hardly exhaust the sources of Negro alienation. Research into the impact of these and other variables should be encouraged.

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[7] For a fascinating insight into the world of welfare see Julius Horowitz, *The Inhabitants* (New York: Signet Books, 1961).

[8] Arthur G. Neal and Melvin Seeman, "Organizations and Powerlessness: A Test of the Mediation Hypothesis," *American Sociological Review*, XXIX (1964), 216–26.

[9] Charles R. Wright and Herbert H. Hyman, "Voluntary Association Memberships of American Adults: Evidence from National Sample Surveys," *American Sociological Review*, XXIII (1958), 288.

[10] *Ibid.*, p. 287.

Some Comments on Comments

James S. Coleman's reactions to the report of Melvin Seeman (*American Journal of Sociology*, November, 1963), entitled "Alienation and Social Learning in a Reformatory," were indeed interesting. My own research with Negro and white lower-class youth tends to support Coleman's view that the adolescent who fails to see any relationship between what goes on in a particular learning situation and his own goal expectations will be alienated from that learning situation. In this case I was dealing with youths in high schools and found that the lower the social-class background of the adolescent the less likely he was to see some meaningful relationship between classroom activity and his own goals. In each case this was more true of Negroes than of whites. In Coleman's words, "a man is sensitive to the cues of his environment only when he believes he can have some effect upon it."

We would expect then that the more restrictive the social system is, in the perceptions of the individual, the less likely he is to depend on traditional means of goal attainment. It is quite possible, then, that people with such world views would turn to "outside" agencies (such as welfare programs) as a means for goal attainment or survival. Again, given the unique position of the Negro in our society, we would anticipate his holding the view of a relatively closed social system—one in which success

is not attained by the means available to those confronted with fewer social restrictions.

Given this interpretation, I am somewhat confused and disturbed by Coleman's final statement on policy implications. He takes the position that it is important to break the welfare or dependency syndrome of these people and teach them to survive in the urban setting. For one thing the ability for personal survival is an almost everyday feature in the lives of the disadvantaged. It is, in fact, a most remarkable characteristic of the deprived that they have managed to do as well as they have, given the consistent restrictions which they have had to face in every aspect of life. It would seem more reasonable to me to move to the source of their alienation in proposing action programs—in this case, removing the restrictions in the system that have forced them to move away from the more acceptable forms of goal attainment and to turn to others for assistance.

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Reply

I am pleased that my comments have provoked others to think about the general questions I raised. In particular one point made by both commentators is important: The existence of discrimination, apart from its objective barriers, can also produce a sense of powerlessness which (if Seeman's results can be extrapolated) reduces the sensitivity of the person discriminated against (e.g., a Negro) to his environment.

I am somewhat disappointed that the two comments that have been made neglect what seem to me the most interesting suggestions of Seeman's results. Seeman's finding is that a sense of powerlessness in a social structure leads to an insensitivity to the cues provided by the environment, and thus a reduction in learning about that environment. This result, if it is more generally true than in institutions of the sort he studied, suggests to me ideas about relations between social structure and behavior that I had not previously entertained. Specifically, I suggested that southern Negroes immigrating to cities from the feudal social structure that characterizes the rural delta South have been in a social structure in which they had no control over the decisions that governed their daily activities. In particular, in almost no areas were they exposed to a market economy in which they could make the consumer decisions, long term and every day, that are characteristic of an urban open society.

Whether my conjecture about the effects of this disjunction of social structures is true or not remains to be tested, and I hope some of the responses to my comments will be directed toward testing it. But I would like to suggest a generalization of this possible relationship that has even broader implications. It is this: Different social structures, and different positions within a social structure, can be characterized in terms of their *precariousness*. That is, in certain social structures such as a feudal hierarchy, few positions are precarious, while in others such as an open society with a market economy, many positions are precarious. And in any social structure, different positions have different degrees of precariousness. By precariousness, I mean the possibility of one's position becoming either worse or better by virtue of one's own everyday actions.

Further, different ethnic groups, which are the principal carriers of cultural streams, are differentially distributed in precarious positions. I would suggest that those ethnic groups which find themselves characteristically located in precarious positions develop cultures that include a high sensitivity to the cues of the social and economic environment. The best examples of ethnic groups that have been

in such a precarious position are merchant groups such as Jews in Europe and Chinese in various Eastern countries. In contrast, those ethnic groups which have found themselves characteristically located in non-precarious positions (whether high or low in the social scale) develop cultures lacking in sensitivity to these cues.

If such a relationship does exist, it is of fundamental importance, because such sensitivity to cues in the social and economic environment determines the ability to learn. Thus both its practical implications for education and its implications for the utopias we conceive (since many utopias eliminate precarious positions) are very great.

I should add one word of caution: In the history of ideas, it can be the case that an interesting and reasonable conjecture, untested, can gain wide currency simply through repetition. Thus it is important that, before much further speculation in this vein takes place, some systematic evidence be examined to test the conjecture.

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Some Comments on "The Professionalization of Everyone?"

In reading Professor Harold Wilensky's "The Professionalization of Everyone?"[1] we have been favorably impressed by his useful contribution to the sociology of work through his analysis of the sequential process of professionalization as it affects, in greater or lesser degree, eighteen occupations, and through his further analysis of different forms of professionalization and barriers to it. However, we feel impelled to comment upon the puzzling polemic implied in the title of this paper and in the tone of its conclusions. Wilensky concludes that "this notion of the professionalization of everyone is a bit of sociological romance" and that "if we call everything professionalization, we obscure the newer structural forms now emerging."

In a review of many hundreds of articles and reports on professionalization that we have conducted in preparing a book of readings on this subject,[2] we cannot recall any sophisticated authors claiming that everybody is becoming professionalized. What many social scientists have tried to do, however, is to apply the concept of professionalization to a wide variety of occupations—that is, in essence, to ask the question, *how professionalized* are auto workers in Detroit, technicians in atomic power plants, librarians in colleges, managers in company *x*, or particular kinds of thieves? Asking how professionalized any particular occupational group is, is like asking how bureaucratized any particular organization is. These are legitimate questions for empirical research; they do not assume that the members of any particular occupational group are very professionalized at all, any more than we can assume that all organizations are necessarily very bureaucratized. Asking such questions does assume, however, that the concept of professionalization can be defined in terms of specific elements like the concept of bureaucratization, that allow it to be applied to a wide variety of occupational (or organizational) types.

In order to ask meaningfully how professionalized any given occupation may be in some place and at some point in time, we must be specific in what we mean by the concept. To date, many authors have been rather loose in this regard, as has Wilensky in the paper under consideration. In his first four paragraphs he speaks almost interchangeably about "professionalization," "a profession," "professionalism," and "professionals," and carries

[1] *American Journal of Sociology*, LXX (1964), 137–58.

[2] Tentatively titled *Professionalization: Readings in Occupational Change*, to be published by Prentice-Hall, Inc.

this confusion throughout the paper without attempting any distinguishing definitions. We feel that it is useful to refer to "a profession" as an *ideal type* (in the Weberian sense) or model of occupational structure characterized by certain specified elements. Nelson Foote wrote about (1) "a specialized technique supported by a body of theory," (2) "a career supported by an association of colleagues," and (3) "a status supported by community recognition," as key intellectual, organizational, and cultural aspects,[3] although others, like Wilensky, have placed greater emphasis upon the elements of "autonomy" and "the service ideal." (It might be pointed out that Wilensky, using a somewhat different definition has not really come to grips with what Foote was writing about. Also, with Parsons,[4] we question the significance of the ideal of service as a special hallmark of professionalized activity in any deeper motivational sense.)

[3] Nelson Foote, "The Professionalization of Labor in Detroit" *American Sociological Review*, LVIII (1953), 371–80; our replication used the same criteria (H. M. Vollmer and D. L. Mills, "Nuclear Technology and the Professionalization of Labor," *American Journal of Sociology* LXVII [1962], 690–96).

[4] Talcott Parsons, "The Professions and Social Structure," *Social Forces*, XVII (1939), 457–67.

"Professionalization," we suggest, ought to refer to the *social process* whereby any occupational group may take on one or more of the elements of an ideal type profession. Insofar as these elements tend to be sequential in nature as Wilensky claims, or at least correlated with each other, and insofar as we can assess the extent of their presence in various situations, we can then identify the degree to which an occupational group has become more or less professionalized.

In contrast, we suggest that "professionalism" be reserved to refer to *an ideology* that places strong emphasis upon elements that are attributed to a profession. Accepting these distinctions, we may then find that a given occupational group shows strong indications of professionalism in the ideology and attitudes of its membership, but shows only a minimal degree of professionalization—that is, actual movement along a continuum from a "non-profession" to a "profession"—in its occupational structure.

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BOOK REVIEWS

Men at the Top: A Study in Community Power. By ROBERT PRESTHUS with a chapter by L. VAUGHN BLANKENSHIP. New York: Oxford University Press, 1964. Pp. 485. $8.50.

This is a study of two small (6,000 and 8,500) communities in upper New York State. It provides an extensive, exhaustive, and sometimes exhausting analysis of survey data from a set of questionnaires and individual interviews of the leadership population of the two towns. The material on the leaders is compared with that derived from a 1,200-member representative sample drawn from the rest of the population of the two communities. The study has, among others, two major purposes in view. The first is to test the pluralist hypothesis as to the structure of community power, which the author attributes to writers such as Robert Dahl and David Truman and in general to political scientists, and the elitist pyramidal omnicompetent decision-making model associated with the writings of Floyd Hunter and C. Wright Mills, and which the author regards as generally characterizing the writings of sociologists. The second is to test the comparative validity of the decisional and the reputational methods of determining the real wielders and holders of community power.

With respect to his first objective, Presthus comes out with findings much the same as everybody else's: a relatively small number of decision-makers, eighty for the two towns; relatively low rates of participation, and in general the reverse of storybook democracy. Sharing the orientation of Hunter and Mills and even citing Michels as an oracle, he shows considerable perturbation at his findings and their import for grass-roots democracy. His second objective, testing the comparative validity of the decisional versus the reputational method of determining power holders, is carried out by applying these methods to the actors in five decisions in each of the two towns. Four decisions—flood control, community hospital, school bond, bringing in new industry—are highly comparable; the fifth, community-building in one case and a housing authority in the other, is somewhat less so. These decisions, according to the author's findings, were locally regarded as the most important in the recent past. Examination of the issues reveals significant discrepancies between the two methods. Both methods prove faulty, though the reputational method does a fair job. The decisional method overemphasizes the role of what turns out to be "legmen" and fails to reveal behind-the-scenes wielders of covert power. The reputational method accords power to potential wielders of power, who in fact do not actively or even covertly participate in decision-making. The two methods in the author's estimation need to be used to check one another and indeed have to be checked by a third *verstehen*, a kind of Weberian judgment that immersion in the data provides. The author decries the methodological purists. Possibly in doing so he unwittingly decries the demand for replicability or public verifiability, but this may not be his intent.

As Presthus points out, the parties in contention, the pluralists and the elitists so called, seem to disagree less on their view of the facts than on their interpretation of them. Without a common standard that can be brought to the bar of facts, the controversy is scientifically barren, however politically and philosophically interesting. What does seem interesting is how, given the detailed and seemingly accurate picture of his Edgewood and Riverview, Presthus could feel alarm for democracy and the Republic. In both there appears, rare for big-city let alone small-town America, effective party competition. In each the "ins" had just been ousted. The "executive committee of the bourgeoisie" is divided, internally competitive, and limited by lower-class power acting through elections, unions, the Catholic church and ethnic organizations. One suspects that the decisions, not involving strikes, race riots, housing, school integration, or other decisions significantly affecting the opportunity structure, are as unengaging to the underlying community as those like Hunter's World Trade Center in Regional

City. The decisions seem to decide little.

Presthus' volume does provide a very intriguing picture of two highly credible communities. His Riverview smacks of the Dobuans and his Edgewood of the Age of Gold.

His criticisms of pluralist theory, even though his statement of the theory is almost eighteenth century, have the point in view of both his findings and those of many others. The confrontation of political theory with even this tiny reality is a clear gain. Beyond this, the emerging types of Edgewood and Riverview are a valuable addition to the portraits of small-town America. The dicta in the book will provoke some, but they are well worth putting up with for the valuable and suggestive data that accompany them.

NORTON E. LONG

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Leadership in a Small Town. By AARON WILDAVSKY. Totawa, N.J.: Bedminster Press, 1964. Pp. 369.

Aaron Wildavsky, following the leadership and methodology of Dahl, Polsby, and Wolfinger, attempts to comment on the theory of democratic leadership and mass society by means of a detailed community study of leadership in Oberlin, Ohio. Field work was conducted during the years 1957–61.

The basic method of research used by the author is a series of detailed case studies of all the specific issues reaching public prominence in the period covered by the field work. These issues involved the decisions to erect a new water plant and a new electric-power plant; to develop and enforce a new housing code; to provide integrated, low-cost housing for Negroes; to attract new industries to the community; and to renovate its downtown business area by constructing off-street parking facilities.

Each of the cases involved a public conflict, which led to a broadening of the issues in the 1959 city-council elections.

The over-all issues involved two factions. One, called the planning faction, consisted primarily of resident out-of-town businessmen, Oberlin University faculty members, Negro leaders, and Co-op leaders. It included several Democratic party leaders, who by virtue of a non-partisan local election were acceptable in an otherwise completely Republican community. The faction was committed to long-range community planning, modernization, attraction of new business to the community, and greater economic and political rights to the predominantly low-income Negro residents of the community. Members of this faction had, for the first time in Oberlin history, been elected to a majority of the council seats in 1957.

The other faction, the traditionalists, represented downtown businessmen and local bankers, conservative Republicans, who had dominated local government from 1869 to 1957. This faction was primarily interested in low expenditures; low taxes; the maintenance of traditional practices and control, including segregated housing; and opposition to the importation of new industry and new population to the community.

In the 1959 elections, when the issues between the two factions were crystallized and generalized, the planning faction was again elected.

Wildavsky treats the specific case material from the standpoint of the theory of democratic pluralism. His major thesis is that "the political systems of most American cities are best described as pluralist rather than as the rule by all the people or by a power elite." He specifically uses his study to attack Mills's theory of the power elite, the Lynds's conception of the reigning family, Hunter's image of business domination, and the radical version of theories of mass society based on the conception of a powerless mass that is manipulated by political elites.

To prove his thesis, Wildavsky points to specific features in Oberlin that appear to disprove these theories. These include the fact that the contending factions are not exclusive and tightly integrated blocks. Some downtown businessmen voted with the planning faction, and others appeared to vote against their immediate self-interest. Moreover, some members of the planning faction were "meteors" exclusively concerned with one issue which they advanced or propagated, then withdrawing from the political process. Others were specialists who sided with the faction only on issues which entered the domain of their specialty.

Evidence for pluralism is further found in the fact that the dominant faction did not win on all issues, was forced to compromise, to co-opt new leaders into its group, to modify its demands, and to bargain with others for support.

Economic or political coercion was not a major basis for the power of the dominant planning group. Instead, knowledgeability, persistent interest, activity, energy, intelligence, and control of official positions pyramided the influence of the dominant group.

The mode of analysis places major focus on leadership and leadership qualities in accounting for the possibility of political pluralism. Since the qualities necessary for political success are the personal qualities of interested, active, and energetic leaders, anyone who is interested, active, and skilful enough can become a major but not dominating force in politics. In short, modern politics is not to be explained in terms of the dominance of interest groups.

Severe reservations can be offered to Wildavsky's thesis, especially as to the relevance of his case materials to the theoretical conclusions he wished to draw.

Any community study reflects primarily the specific background and characteristics of the community studied. No one community, as Stein has convincingly demonstrated, can be considered typical of American society, and conclusions from any one study must be modest.

Oberlin is a college community with a population of 8,198 residents. Its faculty and administration comprise a sizable percentage of its adult citizens, as reflected in the fact that professional workers represent 38 per cent of the community's working force as compared with 4 per cent in the nation. Faculty members provided an important segment of the planning faction, as members of the city council, as advisers to the council and its commissions, as technical consultants, as educators of community leaders, and as proponents and advocates of some policies that were liberal in orientation and national in scope. Thus findings based on studies of communities like Oberlin and New Haven are likely to be of limited generality with respect to issues of business dominance, the power elite, and the impact of the military on American life and other national issues.

A more relevant theoretical focus and conclusion might be made with respect to the "new upper-middle classes." The conclusion is as follows: in communities where college-educated, relatively young, prosperous, employed professional classes comprise a major segment of the population, a conflict between local business and upper-middle-class professionals is likely to ensue over issues involved in community improvements, expenditures, and taxes. In these communities, which are predominantly suburban, local businessmen are likely to lose a historically held dominance. If such a pattern becomes general in the American community, it may well be a major change.

The issues in such conflicts are, by and large, local issues, and they are not likely to reflect issues of national policy or issues involving the role of the corporation, big business, or the military in American life. Pressure groups in national issues are likely to be organized on associational bases rather than in community politics.

This reviewer does not disagree with the possibilities or desirability of pluralism in American life. He does disagree with the relevance of Wildavsky's study to the issue.

The book is, on the whole, well written and interestingly presented. It comprises a valuable basic study of a local community in action and presents a vivid image of the day-to-day operation of local political life.

Insufficient attention is given to the nonpolitical life and structure of the community, and hence to the constituencies of political leaders.

Despite these shortcomings, the book is a valuable one. When analyzed together with other political studies of the community, it should contribute to a growing understanding of the political process of the small community.

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Reflections on Community Studies. Edited by ARTHUR J. VIDICH, JOSEPH BENSMAN, and MAURICE STEIN. New York: John Wiley & Sons, 1964. Pp. xiv+359.

It is only natural that sociologists, for whom the whole world is potentially a research setting, should occasionally turn their curiosity in upon themselves, their activities, and the social, ethical, and intellectual problems and dilemmas and discoveries that arise in the practice of their craft. This book is an examination of some sociological field studies of communities and their effects both on the sociologists who did them and on the communities studied. It seeks to consider such problems as the relationship between the community study and the other research work of the investigator, the investigator's emerging consciousness in the course of study of issues not anticipated in the original research plan, the response of the community to the research process and to publication of the study, and the ethical problems of the researcher thereby engendered.

Rather than do a study themselves as outside observers of one or more sociological projects, the editors have chosen to present a series of highly personal—almost confessional—essays by sociologists who report their own experiences in community research.

There are eleven essays in the book, including a reprint of William F. Whyte's classic reconsideration of *Street Corner Society*, short essays by Hughes on studying French Canada and Frazier on studying the Negro middle class, Art Gallagher's account of his revisit to Plainville, and a ventilation in two essays, by Howard Becker and Vidich and Bensman, of the uproar surrounding the publication of *Small Town in Mass Society*.

Unavoidably, the results are a bit episodic and disjointed, and perhaps also, in places, more than a little self-serving. Local citizens who object to the process of sociological scrutiny are, by and large, portrayed in these essays as a little sick, as displacing excess hostility upon innocent researchers, as rationalizing their own selfish interests. For example, to the charge that publication of their book damaged the access of other researchers to Springdale's small businessmen, Vidich and Bensman reply, "We refuse to take full responsibility for the resentment because, in terms of our analysis, the small businessman will seize on any easily available object of resentment so long as the object in question absolutely lacks defenses" (p. 338).

But I think it is asking too much to require that the sensitive and difficult questions of ethics, research strategy, and professional (as well as community) politics be fully answered as well as raised in a volume of this kind. The fact that it does consider these questions is reason enough to recommend the book to thoughtful social scientists, to teachers of courses in sociological research broadly conceived, and especially to graduate students and to others considering sociology as a career.

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Metropolitics: A Study of Political Culture. By SCOTT GREER. New York: John Wiley & Sons, 1963. Pp. xiii+207. $5.75.

The meat of this book is a modest survey of 246 persons in St. Louis County, Missouri, taken shortly after the defeat in 1959 of a referendum on a proposed "metropolitan" district plan. Hence the title *Metropolitics*. Its 200-odd pages are taken up with a description of the persons and activities involved in staging the event and, to flesh things out, with journalistic accounts of parallel attempts in Dade County, Miami, Florida, and Cuyahoga County, Cleveland, Ohio. There also is a gratuitous introduction on "Morality Plays of American Civic Life," added at the last moment, it would seem, to give philosophical tone to an empirical content.

The 1959 survey was a follow-up of households included in a 1957 survey of preferences of "possible changes in the St. Louis city-county governmental structure. . . . before the metropolitan government was even a public issue" (p. 169). The subject materials thus are related but by no means identical. This is a "resurvey," as stated, but hardly a "panel study," as also stated (p. 98), and only one table uses 1957 data (p. 170). The principal data are from 81 city and 165 county respondents in 1959.

The burden of the content is that "transmitting to the electorate the complex issues of structural change in government is a thankless and near impossible task" (p. 199). For in spite of strong campaigns by civic leaders in all three places, "sixty per cent did not vote in St. Louis County; 80 per cent did not vote in the city. Sixty per cent did

not vote in Cuyahoga County; 74 per cent did not vote in Dade County" (p. 196). And of the minority that did vote in St. Louis, "most voters had only a vague notion of what the Plan was about" (p. 187). "Only a minority of the voters could have been correctly informed on any aspect of the District Plan . . ." (p. 163). Yet at the hands of a small number of badly informed voters, the referendum failed dismally in St. Louis and failed in Cuyahoga, while it just skimped through in Dade County.

Some of the St. Louis specifics cited on these pages will distress those who still hold with the myth of attentive, approachable Rational Man in politics. For example, the St. Louis League of Women Voters had waged a "furious" campaign of telephone calling in favor of the plan. Yet in interviews only a month or so later the calls "were recalled by only 5 per cent of the suburban respondents, 1 per cent of those in the city" (p. 118). In another instance, with the exception of the internationally influential *St. Louis Post-Dispatch*, which scored 49 per cent in the county but only 28 per cent in the city, no other organization which had openly taken sides in the campaign was correctly identified as to its position by more than one-fourth of the sample; most organizations, in fact, were entirely unknown to large majorities (pp. 110–11). In all, this is a story of frustrated communication. "Seldom have so few worked so hard and succeeded in confusing so many" (p. 193).

Several technical aspects of the survey deserve passing mention. One that makes this reviewer uneasy at times is the serious use for discrete categories of responses to questions on where information was obtained; whether, say, from newspapers, television, radio, or organizational contact, especially when the categories add neatly to 100 per cent of the base number of respondents (p. 112). Who can say absolutely where he gets his ideas? Information absorption is a multidimensional process; it is not either-or. The same goes for "reasons given" pro and con, which also in places add to a tidy 100 per cent of respondents (e.g., p. 159). Did not *someone* offer two or possibly three different reasons? And in later chapters there are frequent, and well-warranted, admonitions that the cells on which conclusions are based are "small," that such-and-such is a "crude index," that it "does not justify percentaging," and other clear signals of over-analysis Might not the main points be made stronger with the omission of these? And finally, despite no fewer than 55 tables, this reviewer was unilluminated on any new principles of communication or political behavior other than that it is difficult to get complex messages across to great numbers of people.

In sum, the St. Louis findings are largely a replication of the first of five points made earlier by Hyman and Sheatsley in their widely known and more comprehensive paper, "Why Information Campaigns Sometimes Fail" (*Public Opinion Quarterly*, Vol. XI [Fall, 1947]): the fact that most news, as well as efforts of interested parties to communicate their views to broad masses of people, simply is not recevied by any appreciable number of those in the grass roots. A limitation of the present volume is that it adds little more to our knowledge. It is, however, a detailed record for those who might wish it of what happened in another instance of attempted political change, aided fortuitously by the growing degree of interest in the "metro" movement.

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Urban Renewal Politics: Slum Clearance in Newark. By HAROLD KAPLAN. New York and London: Columbia University Press, 1963. Pp. xiii+219. 50*s*.

Harold Kaplan has provided us with a very important study in the neglected area of interorganizational relations. Sociologists, recently enthusiastic about the possibilities of organizational theory, have been relatively timid in studying the relations among organizations. Selznick's *T.V.A. and the Grassroots* is one notable brilliant exception, but, by and large, we have been bemused by the housekeeping problems of large formal organizations, on the one hand, and the emergence of order in small face-to-face groups, on the other.

In studying urban renewal it is, fortunately, impossible to confine one's self to such a focus

of attention. For urban renewal programs, those amazing efforts to harness in one team public powers and the private market, the Federal government and the local political community, simply cannot be conceived of as unitary formal organizations; George Duggar's term, "the enterprise," is much nearer the mark.

The Newark urban renewal program has been one of the most thriving in the country. Under the powerful leadership of Louis Danzig (known to sociologists for his role in the Deutsch and Collins study of interracial housing), Newark is one of the top "urban renewal cities" as measured by either total dollars or dollars per capita spent. Furthermore, the program has been conceived by Danzig and the Newark Housing Authority according to their own notions of proper renewal policy, and its implementation has been just about what they wanted.

Paying little attention to rehabilitation, they have concentrated upon clearance and redevelopment. They have carried on a vigorous program of public housing construction for Negroes but have not hesitated to relocate Negroes in fringe areas, accelerating the pace of "invasion and succession." They have cleared Italian neighborhoods and replaced them with public housing units partly occupied by Negroes. They have wiped out strip developments of small business, replacing them with giant housing schemes financed by national redevelopment firms. They have ignored city planning, under the assumption (probably correct, in this writer's opinion) that the comprehensive plan is a grandiose and foolhardy effort. In short, NHA has gotten just about what it wanted at every turn. Considering the wide variety of fates among urban renewal programs, ranging from those "run over the hill" in western cities to those slowly subsiding into a passive state (of hibernation if you are a positive thinker, rigor mortis if negative), this thriving program is of considerable interest.

Kaplan's analysis is skilful and complex; I cannot do it justice in the space available. In brief, he traces the power of NHA to control its own destiny to (1) the developing role of the *public entrepreneur* and his staff, ably filled by Danzig and his career public servants, (2) the "political settlements" with competing authority, in which the keystone is the partnership with the mayor, (3) the development of strong alliances with the Federal Housing Agency and the Urban Renewal Agency, insuring a flow of federal funds, and (4) the development of a good "market rating" among the giant national redevelopment corporations, of which the Zeckendorf operation is a symbol. The mayor effectively controls the locals, seeing to it that, for example, areas are designated as "blighted" whenever necessary. In return, NHA gains a very large share of the Federal Urban Renewal budget, justified by its ability to interest large builders in a local investment. This, in turn, is due to the friendliness of the local climate to renewal. The continual flow of new capital investment into Newark redounds to the mayor's credit and is generally considered a good thing by local civic leaders and other businessmen.

A key theme of the book is the ineffectuality of any other local group in urban renewal policy. This Kaplan sees as deriving largely from the great resources, in money, personnel, and skills, of NHA compared with any local competitor. For the national system gives NHA tremendous advantages in determining the future of the local community, and others, failing to lick them, join them (even including such foes of renewal as the local real estate board). The enterprise is possible, however, because all participants share a general set of norms. The most important substantive norm is "anything bringing new capital into Newark is good"; the most important procedural norm is the requirement that "conflicts be settled through informal bargaining, not through overt attacks or public agitation" (p. 167). Together, these eliminate the possibility of democratic decision-making in the local political community. For once co-opted into the enterprise, all participants see the opposition of people most directly affected by urban renewal as "obstructionism" and "publicity seeking."

Underlying the entire story, however, is the state of the Newark community—a fair example of our older industrial cities. The upper middle class has long left the city limits and the other Nordic white Protestant "allrightniks" are on their way; the Negro population is increasing rapidly in all but one of the city's wards. The civic leaders, chiefly businessmen, are plagued by a persistent ambiv-

alence about Newark and its future; one has the impression that, on balance, Newark's public issues are not nearly as important as their serious affairs—their business. Thus the community power structure suffers from the absence of important moving parts—the committed economic leaders with their cadres of civic-minded bourgeoisie. Into the resulting vacuum have stepped the politicos, but they have little concern for the city beyond its utility as a hunting preserve for what Norton Long has called the "hunting and gathering politics" of patronage. As a final result, the key movers, by default, are the politically skilful public servants who have a vision of what Newark *might* be. Such public enterprisers are going to be increasingly important in the political system and the evolving history of American cities, if the trends underlying the state of Newark are as general as I believe them to be.

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The Rulers and the Ruled: Political Power and Impotence in American Communities. By ROBERT E. AGGER, DANIEL GOLDRICH, and BERT E. SWANSON. New York: John Wiley & Sons, 1964. Pp. xx+789. $7.95.

This book is an important source of new information on community politics in America. It informs the reader of the nature of local politics in four communities from the end of World War II through the 1950's, with careful attention given to reporting political events during the years preceding and following the arrival of the research team. Two small Western communities, Farmdale (population 2,500) and Oretown (population 15,000), were studied, during 1951–52 and 1952–53, respectively. Two middle-sized (population 100,000) southern cities, Petropolis and Metroville, were added in 1957–58. The follow-up period for all four communities extended into 1961. It is impossible to report more than the bare outline of the information in this thick book. The first three chapters are a rambling presentation of the theoretical framework that guides the analysis of empirical materials. The authors note that this covers well-known ground and may be omitted by most social scientists. A detailed account of the political histories and socioeconomic characteristics of the communities is followed by a discussion of the pertinent issues around which political decisions were being made in each community. The authors have no difficulty finding an adequate supply of issues—something that sociologists often complain is lacking from many an American community. Perhaps it is the grooming in political science that allows these writers to follow with considerable gusto the evolution and resolution of political struggles over local issues that might not attract the attention of some sociologists.

The central part of the book consists of the presentation of the results of analysis of citizen participation, political leadership, ("power by reputation" method used), types of power structures, and types of "regimes." Although I was not pleased with much of the data analysis, there is a great deal of material of importance in these pages that will add substantially to what is known about the American community. The authors' main goal is to present comparative empirical materials on these four communities within the framework of a modest analytical scheme. But, in spite of the richness of their basic information, they imperfectly realize their goal.

The book is poorly written, being marred throughout by redundancy. The basic concepts and variables are not clearly defined even though many pages are dedicated to this task: 125 near the beginning and 70 in an appendix. The data are presented in an unsystematic and sketchy manner. This is supposed to be a comparative study of four communities. Most of the discussion, however, is in terms of different pairs of communities or unique events in a single community. Also, there are too few tables. I find it difficult to get a comparative picture of four communities unless the data are presented simultaneously. Further, it is cumbersome to verbalize the relationships among a large number of percentage figures when one could more simply and precisely use a statistical measure of association. Not a single measure of association can be found in this book. Nor are tests of significance used on any of the sample survey data. I'm sure this is the first book to publish percentage distributions with a case base of two.

Related to the poor presentation of data

is the lack of focus in many sections. After reading a section of a chapter—with or without concrete data—one is often at a loss to know just what the authors are trying to explain. The purpose of many sections seems to be simply to discuss the data rather than to organize them in order to explain phenomena.

The final chapter does test some interesting hypotheses concerned with changes in power structure and other political characteristics in the four communities over a fifteen-year period. Much of the potential force of these tests is lost because the empirical meaning of the variables is not sharply defined. The variables lack a wealth of empirical meanings that is necessary before a test of their relationship with each other can evoke a sense of urgent interest in the reader. The authors' conclusion that no relationship exists between demographic or economic factors and the political variables seems implausible and, for that matter, at odds with their discussion of the issues that were provoking political conflict in the communities.

PHILLIPS CUTRIGHT

Social Security Administration

At the Pleasure of the Mayor. By THEODORE J. LOWI. New York: Free Press of Glencoe, 1964. Pp. xvi+272. $4.95.

New York City operates under a "strong mayor" charter. Theodore Lowi's book is an examination of the personal and political characteristics of the powerful appointive officials who serve "at the pleasure of the Mayor," and of the changes in the power structure during the past six decades, from the time of Van Wyck and Low to that of Impellitteri and Wagner.

Although Lowi is a political scientist and his book is the outgrowth of a doctoral dissertation, it is useful for both urban and political sociologists. It is a study of a single area in community power structure: why and how the mayor makes his highest appointments, and the sociological and political influences that have guided the past twelve mayors.

Lowi finds that top appointments are by no means merely "payoffs." Demand for expertise and efficiency in city administration has steadily undermined the traditional guides. Instead of the "casual generalist" of the old days (usually a lawyer), the high official is now usually a career specialist. Recruitment is no longer primarily from the party organization and the political club, but from interest groups whose motives are impersonal and specialized. Weber's thesis of the transformation from amateurs and dilettantes to specialists is upheld.

Economic class no longer gives city politics its flavor; rather "ethno-religious sub-cultures." (One-third of Seth Low's Board of Education were gentlemen in the Social Register; since 1917 no member of the Board has been in the Register.) More than in any other American metropolis, in New York City the mayor must find his specialists in the politically proper ethno-religious groups. Moreover, "each satisfactory appointment by the Mayor becomes in varying degrees a commitment that affects his successor."

Reform movements follow a cyclical pattern in the city during this century. Lowi analyzes these cycles and finds that innovation is a function of the minority party. He notes that "reform politics is Yankee politics."

The author's anecdotes and general observations are sociologically interesting. For example: Each new nationality group has had to go through a period of waiting (usually a generation) before arriving politically. The Board of Education has been the most important "point of entry" for the first Italian and Jewish representatives, whereas Negroes have made their entry into top positions by way of the Civil Service Commission. The political party is no longer the clear channel of social mobility in New York.

The two major criticisms of Lowi's book are his vagueness of sociological definition (especially of "social class") and the awkward construction of his charts and tables (many of which are unreadable except after careful study). The author is also careless in some of his writing; for instance, he calls Lazarus Straus "a Jewish Horatio Alger," when he means that Straus resembles one of Alger's characters.

JAMES G. LEYBURN

Washington and Lee University

Gentlemanly Power: British Leadership and the Public School Tradition: A Comparative Study in the Making of Rulers. By RUPERT WILKINSON. New York: Oxford University Press, 1964. Pp. 243. $6.00.

Rupert Wilkinson's book is timely. The English public schools are again being seriously criticized, not only on the basis of their divisive class effect, but, more important, on that of the social utility of the attitudes which they inculcate so skilfully. Hugh Thomas' *The Establishment* and Anthony Samson's *Anatomy of Britain* have recently challenged the amateur tradition which they honor. It is becoming clear that common sense and charisma are poor substitutes for expertise and an appreciation of the need for change.

Wilkinson is concerned with whether "totalitarian" educational systems, including the public schools, can instil patriotism, group loyalty, and respect for seniority while also encouraging intellectual independence. Perceiving these schools as major instruments of socialization, he analyzes the techniques by which they have long produced British leaders. Highly stylized behavior is the essential currency of elite rule. The "gentleman ideal" has three components: magic, the status symbols of leisure, and *noblesse oblige* toward the community. Personal bearing, confidence, dress, and accent are among the major ingredients of gentlemanly magic.

Similarities are traced between leadership behavior in Parliament and the civil service and typical public school values. Political rituals symbolizing God, Queen, Nation, and Empire have their analogues in the public school's worship of tradition, its annual Great Events, chapel, military training, and so on. In all this, the appeal is to seemingly permissive methods of control stressing such things as group harmony, collective wisdom, and good manners. The student is taught to want to conform.

The deference toward rank and authority evoked by the school's prefectorial system similarly asserts itself in a rigid party discipline in Parliament and in rules such as those that prohibit cross-examination of ministers during question time. Wilkinson believes that the superior discipline of the Conservative party compared with Labour rests in part on the public school–Oxbridge background more characteristically shared by its members. Conservatives regard their party as a military unit while Labour is more ideological and less cohesive.

Public school ethics, moreover, provide for a nice synthesis between public service and personal self-realization, a rationale strengthened by a lingering elite contempt toward business which has given British political leadership a generally high standard. This public-service thrust, however, is allied with an amateur spirit which inhibited, and inhibits, science and research by its reverence for common sense and intuition. Such qualities, the author suggests, have been in part responsible for the major failures of British leadership during this century: those of the military during World War I and of the politicians before World War II. Still, incompetent experts can be as disastrous as incompetent amateurs, and tragedies such as Passchendaele were the result of permitting military specialists, hopelessly bound to obsolete tactics such as the frontal assault, to operate independently. Before World War II, Chamberlain's public school–fostered inability to believe that men can be rogues, impervious to rational argument, fatally impaired his capacity to deal with Hitler and German totalitarianism.

To develop his assumption that "totalitarian" education cannot inspire independence of mind and the acceptance of change, Wilkinson surveys Confucian education from A.D. 600 to 1900; Japanese national education from about 1870 to World War I, and Jesuit education from 1500 to 1800. He finds that the Japanese did produce a spirit of inquiry and innovation in technical and scientific areas, since these were in effect "value free." This in contrast to Britain where the system weakened the development of applied science: "If the Industrial Revolution began in England, it was not because of the public schools" (p. 215).

This comparative analysis is less satisfactory than the first half of the book, in part because one finds it difficult to accept analogies selected from such extended time spans and varied cultural milieus. Moreover, the author's intellectual orientation, as might be expected from an old Wykehamist, is essentially historical, literary, and qualitative. Words such as "hypothesis," "experiment," "rigorous," and "decision-making" appear, but they seem strangely

out of context in an analysis much too sweeping to permit systematic handling of the evidence. Despite this common problem of cross-cultural analysis, the book clarifies the important relation between Britain's present difficulty in meeting technological change and the public-school training of its leaders.

ROBERT PRESTHUS

Cornell University

Negro Thought in America, 1880–1915: Racial Ideologies in the Age of Booker T. Washington. By AUGUST MEIER. Ann Arbor: University of Michigan Press, 1963. Pp. x+ 336. $7.50.

Professor Meier's book presents in great detail the nature of the perennial ideological struggle among Negro leaders. Essentially, it is a struggle regarding two diametrically different approaches to the achievement of personal dignity and civil rights. The dominant ideology in the "age of Booker T. Washington" advocated self-help. The author gives a succinct definition of this philosophy by quoting a Negro leader who insisted that the Negro " 'must be the architect of his own fortune.' " Leaders who advocate this approach generally hold that Negroes can best advance as individuals and as a race by separating themselves, as it were, from white society and developing their own institutions, including a "black economy."

The other main ideological position is based upon the conviction that Negroes can attain full citizenship only by pressing unceasingly for total integration in American society. This means that they should take advantage of every opportunity to become a part of erstwhile "white" organizations and exert every effort to break down all legal and traditional barriers to full participation in the institutional life of this nation.

According to the author practically all recognized Negro leaders during the "age of Washington" were arrayed on one or the other side of this issue. Those advocating the self-help approach were ably represented by the very powerful Booker T. Washington of Tuskegee Institute. W. E. B. Dubois, the most outstanding Negro intellectual of that period, symbolized those who insisted that Negroes should agitate for complete integration in all aspects of American life.

Meier manifests astute knowledge of the Negro in American history. This is indicated by his selection of key issues to illustrate the ideological conflict among Negro leaders. Among the issues he analyzes are the following:

1. To what extent should Negroes press for full political participation?

2. What is the real merit of Negro institutions?

3. Should Negroes migrate from the South to other sections of the United States, or even Africa?

The goal of the book is best achieved in two specific instances: chapters xi and xii where he makes a detailed analysis of "The Paradox of W. E. B. Dubois" and "Booker T. Washington and the 'Talented Tenth,' " and in Part IV where he describes the "Institutionalization of Self-Help and Racial Solidarity, 1880–1915." Part IV in some ways may be regarded as presenting the main theme of the book. Here the author gives the setting and motivation for the institutional development in the Negro community. He brings together an array of facts without which one can hardly understand the nature and function of segregated Negro institutions.

On the whole this book is well written. However, Meier tends to become involved in endless details. He often introduces personalities and situations that are unknown except maybe to a few specialized historians. Also, some data are presented as though they have little or no relationship to the social context characteristic of the period he describes. Because of this he presents the ideological stances of Negro leaders during the period of Washington as though they were actors on the social stage today. Therefore, concepts like conservatism, radicalism, and militancy do not reveal as much about the personalities of the leaders as they might have if they had been interpreted in terms of their proper social setting.

This authoritative book should be especially valuable to those interested in Negro history and to action leaders who are looking for fundamental strategies and techniques in the civil rights movement.

DANIEL C. THOMPSON

Howard University

Assimilation in American Life: The Role of Race, Religion, and National Origins. By MILTON M. GORDON. New York: Oxford University Press, 1964. Pp. 272. $5.25 (cloth); $2.50 (paper).

This provocative book lends itself to the task of demonstrating how the nature of group life within a large, industrialized urban nation composed of a heterogeneous population generates conditions under which prejudice and discrimination emerge. Milton Gordon contends that these conditions are salient in his analysis of "large groups," an area which, he notes, is deserving of more attention than it has so far received.

He begins his initial investigation by interviewing leaders representing twenty-five leading agencies devoted to the elimination of prejudice and discrimination in interracial, intercultural, and interreligious relations. His principal objective is to "find out how much thought and consideration has been given by these agencies to problems of social structure, theories and models of 'assimilation,' 'integration,' and 'group life.'" His findings reveal that three-fourths of the agencies have given relatively little thought to these problems, but rather have placed more emphasis upon day-to-day problems—which, he adds, does not necessarily discredit these organizations.

He next concerns himself with identifying three levels of organization: the national society, the subsociety, and the group, each containing corresponding "cultures." To exemplify the parallels between each level of organization and corresponding culture, he provides the following: "within the (ethnic) group there develops a network of organizations and informal social relationships which permits and encourages the members of the ethnic group to remain within the confines of the group for all of their primary relationships and some of their secondary relationships throughout all the stages of their life cycle." What is particularly interesting is the question "Who are you?" and the resultant description given by various respondents. On the basis of this and similar questions and careful observation, Gordon isolates four factors or social categories which serve to create subsocieties within the national society of America. An individual responds that he is not simply a white Protestant, but that he is also a lower-middle-class white Protestant living in a small town in the South. This notion leads to the intersection of vertical and horizontal stratification lines and to the formation of an "ethclass," a subsociety emerging out of intersection. The general question of the implications of "ethclass" for theories of assimilation is treated.

Anglo-conformity, melting pot, and cultural pluralism notions are considered, with cultural or "structural" (as Gordon prefers it) pluralism emerging the victor insofar as contributing to a deeper understanding of the ethnic makeup in American society is concerned. Gordon sees the structure of American society consisting of many "subsocieties," each of which desires to maintain a certain degree of identity while at the same time desiring to minimize prejudice and discrimination which derive, in part, from visible subsociety interdifferentiation. This is the major problem or dilemma which Gordon perceives as critical.

After describing various specific subsocieties and/or subcultures in American society, Gordon summarizes a series of ameliorative suggestions based upon the foregoing analysis: (1) structural assimilation of immigrants to the United States is neither to be encouraged nor desired (hence, the elimination of strain and disruptive consequences of forced adjustment to "new" normative patterns of behavior); (2) cultural assimilation or acculturation should be employed as an adjustive measure for immigration-adjustment agencies; (3) the institutional and subcultural life of the immigrant should be perpetuated as a means of decreasing general opposition to acculturation; and (4) the second-generation American-born children should be viewed as a generation irreversibly on its way to virtually complete acculturation to native American cultural values at selected class levels.

All in all, this book is a stimulating exercise in the problems of minority groups. This reviewer is pleased with the insight it provides into these important problems. There are points, however, which border the realm of reality, particularly in regard to assimilation. It is indeed difficult in treating "group problems" to avoid reducing members of these groups to uniform units. While I am sure Milton Gordon does not wish to convey this impression, it is felt that an emphasis upon "out-group" assimilation should be made. Not everyone nor necessarily the majority of immigrants to the United States each year find

their way into "helping" agencies. If there is reason to contend that ghettos and subculture collectivities are declining in number and in identity, it also seems feasible to postulate that a goodly number of immigrants are finding their way into "acculturated homes" and experiencing no more than a three- or four-month state of anomie. This, of course, can be disputed endlessly. Second, might not physical differences of various specific minority groups serve to perpetuate subculture identity even in the event of extreme second-generation assimilation? Nevertheless, the book does provide a valuable contribution, particularly to those engaged in "applied" programs of action and social research.

DEAN J. CHAMPION

Purdue University

Immigrants on the Threshold. By JUDITH T. SHUVAL. Introduction by LOUIS GUTTMAN. New York: Atherton Press (Prentice-Hall, Inc.), 1963. Pp. xxiv+216. $6.75.

The opening of Israel's gates to all the Jews of the Diaspora, immediately upon its establishment as a state in May, 1948, led to a rapid influx of migrants from Europe, Asia, and Africa. The country was at war, its economy disrupted, and its social structure chaotic—hardly the most propitious circumstances for welcoming and absorbing the mass of newcomers. Some 150,000 of them had to spend an extended period in transit camps. That population provides the subject for this volume.

The field work on which the book rests was done in 1949 and 1950 under the auspices of the Israel Institute of Applied Social Research. Some 1,800 residents of the camps were interviewed in order to test their attitudes and adjustment. Dr. Shuval, who had herself not participated in the field work, received access to the data some years later and has elicited from them a thoughtful volume.

The relevance of her data is limited to the segments of the immigration process with which the information deals—the experience of the transit camps before the newcomers actually entered into the receiving society. The newcomers who were able to go directly to friends, families, and jobs did not stay in the camps and do not figure in this analysis. The author deals therefore primarily with expectations rather than with realizations, with crisis and strain rather than with adjustments to normal life. Her most interesting results, such as the discussion of the effects of ideology or of previous concentration camp experience, have a bearing on transition rather than on settlement.

The distinction emerges significantly in the material on acculturation, which is defined largely in terms of acceptance of the norms and of the existing conditions of the host society. That definition is relevant to the immediate situation of the camp, where the inmate has little freedom of choice; it is not altogether appropriate to the total process of settlement in which the host society itself changes under the impact of immigration.

It is unfortunate, too, that the study was not able to give full weight to the ethnic differences among the immigrants. Although the countries of origin were diverse, the comparisons made are between the gross categories of Europeans and non-Europeans. As a result, it is not possible to take adequate account of the differences between rural and urban origins and of previous degree of westernization. The observations on the Europeans certainly must be conditioned by the fact that a majority of them came from Poland and Romania.

OSCAR HANDLIN

Harvard University

Rich Man, Poor Man. By HERMAN P. MILLER. New York: Thomas Y. Crowell Co., 1964. Pp. xxvi+260. $4.95.

Herman P. Miller is one of that small band of researchers and writers who have helped bring about a revival of debate about the structure of the American political economy. Miller's particular contribution in this book is to present, in popular style, a summary of recent United States Census data concerning the distribution of income in this society.

The theme of *Rich Man, Poor Man* is the persistence of inequality in American life. Despite a continuous (although substantially decelerating) rise in real wages and standards of living since the start of World War II, it is now evident, Miller makes clear, that various systematic biases exist in the way that abundance has been distributed.

Many of Miller's findings have already

gained some popular currency, but they deserve compressed summary here. One of his major contributions is to dispel the myth of the "income revolution." The fact is that, as Miller demonstrates, "there has been no appreciable change in income shares for nearly twenty years." The bottom 40 per cent of families received 13 per cent of the income in 1929, had increased their share to 16 per cent by 1944, and this figure has remained stable to the present day. The top 5 per cent showed a much sharper change during the depression and war years (dropping from a share of 30 to 21 per cent) but their share of the income remains the same today as it was at the end of the war. Equally significant, but perhaps even less noticed, has been the shift in allocation of benefits due to prosperity: the tremendous gains in real wages which occurred during the war and postwar booms benefited, in relative terms, the lower-paid occupations most. During the 1950's, however, this trend was reversed—the highest relative gains were made by the professional and managerial workers, the lowest gains by unskilled and semiskilled workers.

Miller demonstrates further that the reversal of the war-induced trend toward income equalization has, perhaps, been most deeply felt by Negroes. Despite the widespread notion that the Negro standard of living has substantially improved recently, Miller suggests that, in fact, Negro gains have been declining relative to the white population. Negro income as a percentage of white income is today less than it was in 1949 in twenty-two of the states in which Negroes are highly concentrated; Negro occupational status remains roughly what it was twenty years ago in most states. A striking finding is that, despite the vaunted benefits of a college education, nonwhite college graduates "can expect to earn only as much in a lifetime as whites who have not gone beyond the eighth grade." It is clear that we are now beginning to learn the social costs of perpetuating this kind of inequality.

Miller compiles a number of other useful statistics: the social characteristics of the impoverished fifth and the top 5 per cent, relationships between education and income, the role of working wives in determining family income position. All of this is presented in a highly entertaining and readable style which makes the book particularly worthwhile for the lay reader.

Two criticisms are relevant: first, emphasis on census *income* statistics tends to underestimate the degree of inequality—for example, Lampman's finding that 1 per cent of the population controls 30 per cent of the *wealth* suggests a far greater amount of inequality than is apparent from Miller's data. Second, his proposals for reform betray some sociological naïveté: for instance, he can see little solution for Negro poverty without stabilization of Negro family life. In short, Miller sometimes attributes a causal relationship to a statistical correlation.

RICHARD FLACKS

University of Chicago

Education and Society in Modern France. By W. R. FRASER. New York: Humanities Press, 1963. Pp. x+140. $4.00.

Badly titled, for it speaks little about what education does to society, this book is a singularly lucid analysis of the difficulties of directing change in the educational system of a complex society. With minor alterations of detail, the same plot could be used for almost any other advanced society. "Even when Ministers hold executive power without parliamentary restraint," the structure of and established expectations concerning education resist, on substantial or illusory grounds, new impulses and new programs. There is, after all, little distinction about Soviet education other than its scale. Fraser's account definitely supports those Americans who have been contending that fundamental changes in schools can more quickly be effected in our federal and decentralized system.

France has one of the more dynamic economies; it has an elaborate and respected organization for economic projections if not planning; qualified applicants for university have doubled since the war. But consensus on how to adapt schools to the new era does not emerge. In principle the need for more equality of opportunity, for vast armies of technological specialists, and for a more flexible system will be conceded. But the obstructors to each proposal are numerous, and they can even split nearly every political party.

Fraser takes up these resistances one by one. Traditionally each kind of school had its separate clientele and its separate branch of the central administration. The professional obstacle is expressed by anxieties among teachers as to who will gain or lose the abler

pupils or the symbols of intellectual repute. Graduate teachers who staff secondary schools but lack pedagogical training contend that any scholar can teach children, that their own dignity would be affronted if they were asked to teach primary classes, that new subjects cannot so well prepare a technical elite as does the study of Latin. "What one must on no account alter, on pain of wrecking by such change our [secondary education], is the way in which we teach" (p. 40). "Technical progress may have transformed many things; it has not yet modified the structure of the human mind" (p. 42). Naturally the scholastic purists rallied to the secondary teachers' side; one must train, not fill, the mind, and "nothing is better fitted to that purpose than the exercise of translating the ancient languages. Because it teaches nothing. Because it trains" (p. 46).

No secondary schoolmaster or a worshiper of the classics, naturally, could favor recruiting new elites through techniology and science. And, naturally, the social sectors that have supplied the learned cadres in the past view with alarm any sordid mixture of men with different training. Only such obtuse men could contend that "class issues" are irrelevant to the schools of France. Yet in France also, university faculties look askance at the results of their secondary-school colleagues' work; so France is now introducing an orientation year in the university.

All these dimensions of controversy focus on the battle over religion, a heritage from the conflicts of the Revolution; and local autonomy becomes, as here, a refuge for political forces who fear they will lose the school battle in the main arenas of opinion.

As in other countries, conflicts may not be resolved but they may become pointless, though not tomorrow. Expansion of secondary-technical education may outflank the classicists. Pressures of numbers and the demands of the technocrat planners open up crannies in which new lines of study take root.

C. Arnold Anderson

University of Chicago

The Educational Decision-makers. By Aaron V. Cicourel and John I. Kitsuse. Indianapolis: Bobbs-Merrill Co., 1963. Pp. 178. $2.95 (paper).

This provocative study is based on interviews with about ninety students and their parents, plus almost all the twenty-five counselors and social workers at a large midwestern suburban high school where most students are from Hollingshead's classes I–III. (A separate interview of nineteen guidance counselors from a wide variety of high schools is used comparatively in one chapter.) The authors have two major points to make, each logically distinct and argued from different kinds of evidence.

The first concerns why some high school students go on to college and is statistical. The authors stress a bureaucratic situational determinism, as against Parsons' emphasis on long-range values and ascribed characteristics of social classes. Almost all parents in the sample expected that their children (regardless of present grades) would attend college, and the students were only slightly more realistic. Yet in fact only about 73 per cent at this high school did go on to college, and only 15 per cent of these to prestige Eastern institutions. Widespread parental ignorance exists of the curricular means for obtaining the ascribed goal (especially at the crucial ninth-grade level), thus making the guidance counselor's role all the more decisive. These counselors, often acting independently of parental pressure, select who may take the preparatory courses and thus wield extraordinary power, ultimately affecting both the national social mobility rate and countless individual lives.

Second, the book considers changes in these counselors' norms and values. Their professional orientation is increasing, bringing quasi-Freudian conceptualizations as distinct from the old common-sense categories. These encourage school personnel not only to think of students' problems as "deep" (i.e., non-situational) but to see problems where none before existed. Here enters a respectable excuse for favoritism. A problem-oriented, seemingly "scientific" vocabulary permits decisions which ironically, may be less impersonal (hence fair) than in pre-bureaucratic days. But do these counselors misuse their power? What "really" motivates them remains largely unrevealed, and there is no comparison of the rate of favoritism with analogous arbitrariness in other institutional settings. Instead the authors are too willing simply to be shocked by its existence. The counselors' perspective is discussed impressionistically. Large chunks of interview material illustrate but do not verify

the authors' thesis. The contrast between professional and common-sense conceptualization is probably overdrawn; both are manipulative.

Statistically, the study shows that there is an undependable correlation between students' testable academic talent and their assignment to honors courses. As democrats the authors not only lament the role of intuition (buttressed by Freud) in making these assignments, but also come close to inferring a distaste for all segregated ability groups. Occasionally the book almost becomes an old-fashioned plea against elitism, despite its future-oriented assertion that "Lakeshore" high school, now atypical, will be widely imitated.

LAURENCE R. VEYSEY

Harvard University

The Dynamics of Bureaucracy. By PETER M. BLAU. Chicago: University of Chicago Press, 1963. Pp. xiii+332. $7.50.

This book, a case study of lower-level civil service employees in two agencies, is an expanded edition of a study first published ten years ago. Virtually all of the original monograph remains intact. The new material consists of interpretative supplements to most chapters and two new methodological chapters. The supplements attempt to place the findings in the context of recent research.

Part I of the volume deals with the activities of twenty-four interviewers in a state employment agency serving the apparel industry. The central theme concerns the unintended consequences of a change in performance records of interviewers with respect to competition, productivity, social cohesion, and organizational goals. Blau is exceedingly deft in combining qualitative observations with quantitative data. Harry Cohen, who collaborated with Blau in writing the new material, replicated Blau's study. Instead of a brief summary of Cohen's results, it would have been more useful if the replicated tables had been presented. Though most of the interpretations are persuasive, some important facts appear to be omitted. For example, a few months before Blau observed the agency the head of the department was changed, and the successor instituted a new system of interviewer performance records. No mention is made where the predecessor went, where the new head came from, or why the old system of performance records was abandoned.

Part II deals with a federal law enforcement agency. Blau is concerned with performance of duties and informal relationships. The duties of the agents primarily consisted of developing legal cases against employer-offenders. The informal relationships deal mainly with patterns of consultation among the agents about intricate legal problems. Officially, agents were to consult with their supervisor if they had difficulties, not with other agents. Blau explains the frequent interagent interaction and rare supervisor consultations as arising out of fear of lowered civil service ratings (an explanation which is not altogether credible since almost all had more than ten years' seniority). The patterns of consultation are analyzed in terms of a social exchange process: the most knowledgeable agents give legal help in return for informal status. The theoretical utility of the social exchange perspective is exemplified by Blau's study.

The new methodological material includes a five-page discussion of structural effects, a chapter on field work in bureaucracy, and a chronicle of the conceptual development of the research. The explanation for disentangling group versus individual effects is made unnecessarily difficult by an example involving pure interaction. Throughout the book, however, Blau is at his best when theorizing about group and individual effects. The new methodological chapters are useful additions, though the field work chapter probably should have contained Blau's interview schedules.

As a whole, the new material enriches a first-rate monograph.

BERNARD LEVENSON

Johns Hopkins University

The Social Science of Organizations: Four Perspectives. By HENRY A. LATANÉ, DAVID MECHANIC, GEORGE STRAUSS, and GEORGE B. STROTHER. Edited by HAROLD J. LEAVITT. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. ix+182.

This small book about large organizations is written by participants in the First Seminar in the Social Science of Organization, an interdisciplinary meeting held at the University of Pittsburgh in 1962. As its introduction ex-

plains, the book is not meant to be a comprehensive survey. Instead, it has the more modest but equally valuable aim of presenting some of the major positions and problems which are current in the study of organizations. The initial conception is promising. The four authors, each from a different discipline, were commissioned to write papers that would reflect not only their own views on organizations but also the influence of their interaction with men of other views during the seminar. The result is four quite knowledgeable and generally interesting papers, but only a rather sketchy introduction to the major concerns of organizational social science, especially its major substantive problems and theoretical orientations.

In the first paper, "Problems in the Development of a Social Science of Organization," George B. Strother presents a historical overview. But it is really much too brief, beginning as it does with Plato's *Republic* and ending fourteen pages later with Presthus' *Organizational Society*. However, Strother's observations on the logical and practical problems involved in establishing an interdisciplinary field are sensible and are frank in noting the obstacles that exist in an area which has been a subject of multidisciplinary interest for a good while.

"Some Notes on Power-Equalization," by George Strauss, is a useful analytic and critical summary of the "human relations" propositions that large-scale organizations block self-realization and that reduced power and status differentials lead to more productive, happier subordinates. Strauss clearly distinguishes the different forms that power equalization may take, suggests a number of conditions that may affect its feasibility and successful use, considers some of its possible dysfunctions, and questions some of its assumptions (e.g., do most people really want more freedom and responsibility in their work?).

The third paper, by Henry Latané, concerns "The Rationality Model in Decision-making," using portfolio management as an example. Decision-making theory is an important part of organizational social science, and this paper will almost certainly interest students of decision-making theory. However, the paper is much too technical and specialized to be of much help as an introduction and seems a poor choice for a book of this sort which is aimed at a general audience and is presumably meant to reduce distances among men of different skills and orientations.

In the last paper, David Mechanic gives "Some Considerations in the Methodology of Organizational Studies," dealing with the choice of problems and variables, the role of values, the utility of maintaining the distinction between formal and informal processes, the use of types and typologies, the relative advantages of a one-sided versus an eclectic approach, and the uses of experimental techniques. Mechanic's considerations are well worth reading, for they meet a major criterion of good methodological writing, which is that they are helpful for research. For example, his discussion of the role of values in scientific work, which makes informed use of Weber's ideas, shows how the consideration of values may provide a productive way of analyzing organizational behavior in terms of goal-structure and means-ends relationships instead of providing the basis for what has all too frequently been sterile controversy.

JOHN D. BREWER
University of Chicago

The Science of Human Communication: New Directions and New Findings in Communication Research. Edited by WILBUR SCHRAMM. New York: Basic Books, 1963. Pp. 158. $4.50.

This volume of eleven papers by a group of distinguished specialists describes current work in the field of human communication. Widespread interest led to the present publication of what was originally a Voice of America series. In an opening essay Wilbur Schramm describes the general characteristics of communications research in the United States. The central ideas of the theory of cognitive dissonance are presented by Leon Festinger, along with experimental results bearing upon these ideas. Charles E. Osgood discusses the semantic differential as a tool for the measurement of meaning and describes the cross-cultural program employing this measure which is currently investigating the universal and variable aspects of affective meaning in human cultures. Elihu Katz discusses communications studies bearing upon the diffusion of new ideas and practices and sketches an emerging theoretical framework for research on the

communication processes involved in the acceptance of innovations. The closely related problem of the interplay between mass media and personal influence is discussed by Paul F. Lazarsfeld and Herbert Menzel. Eleanor Maccoby brings some fresh research findings to bear upon the problem of the effects of television on children. The remaining chapters contain summaries by Irving Janis and Nathan Maccoby of experimental research in the Hovland tradition, a paper by Klapper on the social effects of the mass media, an analysis of the effect of communication in voting campaigns by Ithiel de Sola Pool, and a discussion by Lumsdaine of teaching machines and programmed learning.

These developments in communications research, for which "current" is perhaps a more appropriate term than "new," reflect several important characteristics of recent activity in this field. They are clearly multidisciplinary in range and behavioral-scientific in spirit and procedure. They involve approaches that emphasize basic similarities between interpersonal and mass communications and also stress the interplay between these two aspects of communications systems. Readers will be aware of the omission, due to the behavioral science emphasis, of work like Goffman's analysis of face-to-face communication and historical and evaluative approaches to the study of the mass media. The omission of important developments in linguistic research, computer simulation of communication systems and mathematical models will also be noted. Communications research, however, has been the scene of vigorous activity in recent years and it has been some time since a publication providing orientation for the general reader has appeared. The editor has again taken the lead in pooling the resources of this field in a manner which should serve to disseminate more widely some of the key contributions currently being made by communications specialists.

Herbert Hamilton

University of Illinois

Childhood and Society. By Erik H. Erikson. 2d ed. New York: W. W. Norton & Co., 1963. Pp. 445. $6.50.

Not much has been changed in, or added to, this second edition of Erikson's monumental contribution to psychoanalytic ego psychology. Faithful to his belief that ethical practice should be based on the imperatives of the life cycle, Erikson explains that his freshman seminar students at Harvard "decided almost unanimously that I should not make any drastic changes—as if tampering with an itinerary written in younger years was not one of an older man's prerogatives." He adds that revisions and corrections were mainly restricted to "those passages which on re-reading I did not understand myself. Secondly, I have amplified or corrected descriptions and explanations which have often been misunderstood or repeatedly questioned by students." Erikson has also footnoted the text with critical comment on his ideas of fifteen years ago.

As for major additions, the author has extended his discussion of infantile sexuality with a research of his own, one that suggests important relationships between psychosexual modalities, and specific form and content in the play of boys and girls. The author's elegant little study demonstrates that there is no necessary incongruity between "Eriksonian" thinking and careful research; it is a matter of what is observed, how it is observed, and how one thinks about the data.

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DAVID L. GUTMANN
University of Michigan

Identification and Its Familial Determinants. By ROBERT F. WINCH. Indianapolis, Ind.: Bobbs-Merrill Co., 1962. Pp. 223. $2.95.

This book has obvious attractions. It is a moderately priced paperback. It is part of a series with an impressive name, "Advanced Studies in Sociology." Most inviting of all is the book's title. Few phenomena in social psychology are more important than identification. When such a topic is linked with "its familial determinants," the reader cannot help but feel tempted. Unfortunately, these attractions about exhaust the book's value.

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The first 100 pages discuss the concept of identification and review studies which illustrate varieties of independent and dependent variables. The last 100 pages present the methods and results of Winch's pilot work. Unfortunately, these two parts do not fit well together. Most of the theory developed in the first part fails to emerge from the pilot data. But there is no revision of theory in the light of the data. As a result, one has the feeling that this book was written like an explorer's journal and that the expedition was a failure. We will begin our comments with the second part.

Winch's pilot studies are limited to college students. He asserts (p. 110) that this is because their identifications are well crystallized, but one suspects (p. 111) that it is their accessibility which counts. Whether or not college students have well-crystallized identifications is a good question. If they do, why is it so difficult for them to get out of college and start life? If it is crystallization we are looking for, what about studying adults who have chosen their way and are settled in it? To take a contrary point of view, would not the best time to study identification be when it is forming? In adolescence, for example, or even childhood.

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communication processes involved in the acceptance of innovations. The closely related problem of the interplay between mass media and personal influence is discussed by Paul F. Lazarsfeld and Herbert Menzel. Eleanor Maccoby brings some fresh research findings to bear upon the problem of the effects of television on children. The remaining chapters contain summaries by Irving Janis and Nathan Maccoby of experimental research in the Hovland tradition, a paper by Klapper on the social effects of the mass media, an analysis of the effect of communication in voting campaigns by Ithiel de Sola Pool, and a discussion by Lumsdaine of teaching machines and programmed learning.

These developments in communications research, for which "current" is perhaps a more appropriate term than "new," reflect several important characteristics of recent activity in this field. They are clearly multidisciplinary in range and behavioral-scientific in spirit and procedure. They involve approaches that emphasize basic similarities between interpersonal and mass communications and also stress the interplay between these two aspects of communications systems. Readers will be aware of the omission, due to the behavioral science emphasis, of work like Goffman's analysis of face-to-face communication and historical and evaluative approaches to the study of the mass media. The omission of important developments in linguistic research, computer simulation of communication systems and mathematical models will also be noted. Communications research, however, has been the scene of vigorous activity in recent years and it has been some time since a publication providing orientation for the general reader has appeared. The editor has again taken the lead in pooling the resources of this field in a manner which should serve to disseminate more widely some of the key contributions currently being made by communications specialists.

HERBERT HAMILTON

University of Illinois

Childhood and Society. By ERIK H. ERIKSON. 2d ed. New York: W. W. Norton & Co., 1963. Pp. 445. $6.50.

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University of Michigan

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An example of Winch's approach to measurement is his attack on the question, "Do

subjects give the same answers when asked who has *influenced them* as when asked who has *done things for them?*" Winch handles this by asking two questions which must appear to his subjects to be essentially the same. First, he asks them to choose from a list the five persons who have been most influential in their lives, then to choose from the same list the five persons who have done the most for them. If there is any important distinction between these two questions, it is subtle. It is hardly surprising that most subjects chose the same five people both times. Winch feels (p. 127) that he has protected himself against this kind of problem by alternating the order of the two questions from subject to subject. We do not see how this alleviates the equivalence of the questions for the subjects.

Winch's presentation of data on this same question illustrates the statistical quality of the book. The essential statistics would be a cross-tabulation between the two questions. In how many instances was the same figure *not* mentioned twice? Did these omissions pile up on the "influence" or on the "done things for" question? This elementary piece of statistical display is missing. Instead, Winch presents the univariate distributions of each question (p. 129) as though they were collected from independent samples.

The statistical data that are presented are poorly handled. On page 116 one set of data is presented twice in two adjacent tables. The only difference is the direction of contrast. The efficient way to handle these data is in one table along with a simple, but useful, two-way analysis of variance. This would not only encompass both contrasts but also show any interaction effects.

Omission of factor loadings from the report of the results of a factor analysis (p. 114) is another example of missing statistics. This omission is unfortunate because it prevents others from replicating Winch's work. Perhaps Winch's general attitude toward his data is characterized by the decapitated layout of Table 6. The heading is at the foot of page 193, but the body is on pages 194–95.

Winch finally gets down to data on family determinants in chapter viii. He does this by comparing one- and two-parent families. However, he has only five one-parent families for males, only eight for females. The results of the two sets are not parallel, and we find that the main promise of the book, *Identification and Its Familial Determinants*, comes down to the responses of thirteen college students.

In all hundred pages of pilot study, the only unusual item has to do with the effect of older same-sex siblings on college students' perceptions of family function. On page 138 Winch shows that in families *without* older same-sex siblings, the perceived functionality of the father is augmented for males, that of the mother for females. In addition, for males, the absence of the same-sex sibling, despite augmentation of the father's functionality, decreases the total functionality of the family. The study of siblings is new in the study of identification, and these data are intriguing.

Most readers will agree with Winch's own evaluation of his pilot work: "The results are to be interpreted with the understanding that the samples were small, quite homogeneous, and not randomly selected; that the measures were not entirely satisfactory" (p. 133).

Comment on the theoretical portions of the book is more difficult. The psychological concept of identification originated with Freud. But beyond his penetrating but incomplete discussions, no satisfactory theory of identification has developed. Winch begins by taking a defensive position with respect to Freud. "From time to time one hears that without psychoanalytic training a person is incompetent not only to do research on identification, but even to think about it. . . . It is our view that the Freudian conception of how identifications are formed has a plausibility about equal to its testability" (pp. 2–3). But this does not prevent Winch from making many references to Freud's "assertions."

Winch does, however, omit much of the psychoanalytic literature on identification. In particular, he neglects two of Freud's three fundamental explorations of the topic, the initial one in "Mourning and Melancholia" and the last original contribution in chapter iii of *The Ego and the Id.*

Actually, in the two pages devoted to Freud's contributions, Winch presents Freud's theory in terms of statements made by Urie Bronfenbrenner in his article "Freudian Theories of Identification and Their Derivatives" (*Child Development*, XXXI [1960], 15–40). We wonder if perhaps Winch was satisfied with Bronfenbrenner's Freud and did not feel

impelled to go much into the subject for himself.

Winch's presentation of current sociological meanings of identification will be useful to some, but his attempts to clarify and operationalize the concept are not successful. He defines three types of identification, "Similar," "Reciprocal," and "Opposite," but his discussion of how to distinguish "Reciprocal" from "Opposite" empirically is unintelligible (p. 14).

Winch's "Opposite" identification implies a negative feeling of subject toward model, but his operational specification calls only for a high amount of a trait in the model and low amount in the subject. It would seem that a negative relationship between subject and model on a given characteristic would be required.

The apparently successful operationalization of "Reciprocal" identification (reciprocal-to-nurturance and reciprocal-to-control) is promising, but the operationalization of "Similar" remains murky. Items such as "the person whose interests are most like mine," or "whom I most resemble" seem more appropriate for measuring identity than identification.

The chapters dealing with independent, dependent, and intervening variables are probably the most useful. Sociologically oriented readers desiring a broad overview of the various ways in which societal and familial structures influence the child may find something in the chapter on independent variables. But Winch was unable to make direct use of social function as the ultimate independent variable in his pilot work. The parental functions of nurturance and control, designated as intervening variables, turned out to be more useful. In view of this, one wonders why Winch himself does not conclude that identification is primarily a phenomenon of depth psychology after all.

Benjamin Wright
Shirley Tuska

University of Chicago

Social Psychology. By William W. Lambert and Wallace E. Lambert. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1964. Pp. viii+120. \$3.95 (cloth); \$1.50 (paper).

Consensus appears to be lacking among social psychologists as to the best way to develop the field. The strategy of these authors is to summarize a vast array of empirical studies and to report the results of these investigations under selected concepts. Most of the studies are small-group experiments conducted by psychologists using undergraduates as subjects. The concepts utilized range from such old standbys as "needs," "attitudes," and "aggressiveness" to more recent innovations like "the line of regard," "balance," and "cognitive dissonance." Unfortunately, the range of experimental data covered is extensive within too narrow a perspective. The studies of sociologists and anthropologists which are relevant for social psychology are largely ignored. The concepts, whether old or new, are rarely defined explicitly; and even when they are defined, one frequently wonders how a given event could be clearly delineated as falling within or without the scope of the concept.

The authors ritualistically pay lip service to the "interdisciplinary" nature of social psychology but the selection of their subject matter (as indicated above) suggests that their contribution to this desired state ends once the reader is advised of the fact. The authors also accept the current dictum that a close relationship between theory and research is a most desirable thing. Yet unless this harmony is to be had magically by counting off pairs of experiments "Noah fashion," this work will contribute little to this sought-after synthesis. There is no systematically developed theoretical formulation which is related to the research studies surveyed. Instead, numerous disparate concepts and apparently unrelated hypotheses are suggested throughout the work, and one is left with the impression that social psychology consists of making up esoteric-sounding terms and conducting "gimmicky" experiments on unsuspecting undergraduates.

In the last chapter of the text an attempt is made to bring together "macro-" and "micro-" processes, and that discussion strikes this reviewer as the most promising part of the book. Ideas hinted at and concepts developed there are worthy of greater elaboration and might even serve as the organizing framework for the entire book. If this were done, the book would come closer to providing an interdisciplinary analysis in which theory and research are synthesized than it does in its present form.

The book is one in a series of texts designed to provide the instructor with brief surveys of a large number of topics generally covered in

introductory psychology courses. Psychologists will probably find the book more appealing as a text than sociologists, but sociologists may find it useful as an exploration of a large body of research that might otherwise be overlooked.

WILLIAM J. CHAMBLISS

University of Washington

Passage through Crisis: Polio Victims and Their Families. By FRED DAVIS. Indianapolis, Ind.: Bobbs-Merrill Co., 1963. Pp. vii+195. $2.95.

An interdisciplinary team recorded the physiological, psychological, and sociological impact of poliomyelitis on the child and his family over the two-year period following diagnosis. This is the sociologist's report of the family experience of fourteen who had paralytic polio of varying severity affecting the lower extremities. Davis describes the development of family adaptation through four phases of treatment: prelude to the illness, warning cues that the illness was serious, impact of the paralytic condition and inventory of the injury's extent.

The study produces two social psychological and one practical conclusion. The former are that family structure and interaction remain essentially static during the crisis and that, depending on the severity of the handicap and the child's age, families attempt either to "normalize" in interpreting the child's condition or to "disassociate" from it. While family dynamics are temporarily altered during the hospitalization of the child, greater indulgence and permissiveness being exhibited by parents toward all their children, customary modes are re-established with the child's return home. When the affected child is young, the family tends to treat him as a normal child. Families with older and severely handicapped children lean toward disassociation of the child from other children and of their expectations from those which are normal.

The practical conclusion is that hospital personnel avoid informing the family of the extent of the child's handicap even when they are relatively certain of the extent of recovery to be expected. Davis argues that this enhances confusion, strain, and optimistic distortion of the condition and recovery chances of the child. In the obsence of any evidence from cases in which family members were told the naked truth by them, medical practitioners can reasonably be expected to resist the recommendation that fuller information would reduce uncertainty and promote healthier social and emotional conditions in the family. It could be that optimistic distortion is functional in motivating family members to support and seek maximal recovery for the child. Davis is properly circumspect in suggesting alteration of hospital practices.

The style is naturalistic and clear. Although the description is convincing because of its documentary realism, the reader's impression is that the methods used do not test or amend the numerous concepts to which the author repeatedly refers. The pragmatic empiricism of Davis' approach limits the theoretical contribution of the research to illustration of a variety of concepts from a number of theories.

ROBERT J. POTTER

Flint College
University of Michigan

Culture and Personality. By VICTOR BARNOUW. Homewood, Ill.: Dorsey Press, 1963. Pp. 410. $7.50.

The field of culture and personality is respectable at last. It has a substantial, albeit varied, body of data; it has several methodologies, though some are of dubious validity; and it has some findings, even though there is occasional uncertainty as to their relevance. It also has ancestors, but there is an unfortunate tendency at times to disturb their rest and thus upset the stasis of the living. There is near unanimity that personality and culture are somehow correlative (the dissenters in our scientific radical right are probably too few to worry about). Very good. Where do we go from here?

When measured against this last question, Barnouw's *Culture and Personality* is terribly disappointing. It is more than disappointing when evaluated by the question, "What do we know in this field?" Barnouw begins with the assertion that culture and personality are the study of the "significant determinants of personality" (p. 17) and the field is cast in terms of the psychology of individual personality (p. 26). What, it can be asked, has this to do with the study of society and culture, their

change and stability, their structures and dynamics? Without advocating a bureaucratization of social science, is not this the proper province of psychology? This question is not considered by Barnouw.

With this questionable definition of the field, Barnouw launches into a history of sorts of culture and personality, with hardly a mention of Sapir but with an inordinate amount of attention to Benedict, Linton, and Kardiner. (Really, now, Columbia is not the center of gravity of culture and personality.) There is a chapter devoted to "Malinowski's Criticism of Freudian Theory" but there is neither a fair nor clear exposition of what Freudian theory is. A chapter devoted to Margaret Mead tends to confine her impact on us to problem-oriented field work; no attempt is made to evaluate her theoretical position in a historical framework, her initial hypotheses, their changes, and their stimulation of further work in the field. We read that there has been some skepticism about some of the hypotheses of these people; but what is the balance, what can be gleaned from them, where does Barnouw stand? This quasi-historical view of culture and personality may reflect the growing reluctance in contemporary science to engage in controversy; but more than this, it fails to note for the student that these older ideas were closely related to now-rejected views of culture and social systems. It barely skims the surface of available and relevant theory.

A major portion of the book is devoted to methodology, and it is this section which I find the most disturbing. Briefly stated, what Barnouw calls methodology in culture and personality emerges as the procedures of the psychologist who is working in a clinical setting. (Gentlemen, let us declare a moratorium on obeisances to the couch that lays the golden egg.) No attempt that I could see is made to relate these procedures to the study of social and cultural systems. A clear example is Barnouw's summary of the Spindlers' work among the Menomini. The Spindlers set out to show (and succeeded, I think) that such data as Rorschach and TAT must be tied ever so carefully to detailed analyses of social organization, cultural patterns and economic adaptations and maladaptations. In Barnouw's summary of this work, the Menomini emerge as little more than a series of FM responses, controlled extroversion, polar points in a continuum, emotional control, and so forth. Only the barest mention is made of the lumbering industry on the reservation (inappropriately referred to as a "saw mill") as a determining factor. The Tuscarora in Barnouw's hands seem to have no social structure but only show a variety of frequencies of M, FC, W, D, etc. So what! Similarly, the chapter on cross-cultural methodology is incredibly naïve.

The inclusion of a chapter on art in a textbook on culture and personality is an unusual and promising innovation. But it suffers in Barnouw's treatment by an embarrassing superficiality as well as some curious tilting at windmills: Wallace's analysis of Mayan art—which leans heavily on the theory of drawing analysis—is criticized because its "cross-cultural applicability is often doubtful" (p. 330), even though Barnouw has just finished devoting a twenty-three-page chapter to a sympathetic treatment of drawing analysis which "seems to have promising potentialities" (p. 297).

In summary, then, whether measured against the criterion of what is known in the field or in terms of where we go from here, this is a painfully inadequate book. We really know much more about culture and personality than Barnouw's book would suggest.

YEHUDI A. COHEN

*University of California
Davis*

Das Mitbestimmungsproblem in der deutschen Sozialforschung ("The Co-Determination Problem in German Social Research"). By RALF DAHRENDORF. Tübingen: Soziologische Seminar der Universität Tübingen, 1963. Pp. vi+123. DM. 2.00.

If sociological studies of the same topic were undertaken independently, would the results be substantially the same? The essay under review helps answer that question. Dahrendorf critically reviews and compares four studies which, in varying degrees, report worker perceptions and evaluations of co-determination. (Co-determination entails worker and union representation at the top management and board of directors levels.) The studies are: (1) *Arbeiter-Management-Mitbestimmung*, by T. Pirker, S. Braun, B. Lutz, and F. Hammelrath; (2) *Die Deutsche Betriebsverfassung*, by Otto Neuloh; (3) *Das Gesellschaftsbild*

des Arbeiters, by H. Popitz, H. P. Bahrdt, E. A. Jüres, and H. Kesting; and (4) *Betriebsklima*, by the Frankfurter Institut für Sozialforschung. The studies differ in research methods and sponsorship.

In the first part of the essay, Dahrendorf compares and integrates the findings of the studies. Insofar as similar questions were asked of the respondents, similar results are reported. The studies concur that the workers think of co-determination as more than the establishment of certain organizational arrangements; the studies report considerable and widespread ignorance about the specific arrangements of co-determination; the studies agree that the overwhelming majority of the workers expect improvements as a result of co-determination and generally think of the improvements in the form of higher wages. In the evaluation of co-determination, a discrepancy is to be noted: in the Frankfurt study disappointment was reported to be relatively great since only about 10 per cent of the workers found that they had personally benefited from co-determination; but the other studies report a majority of the workers expressing general approval of co-determination; clearly, different questions yielded different results.

Dahrendorf's major concern, however, is not to assess the reliability of independent studies. He is concerned with evaluating a range of research styles and assessing the consequences of different styles. Among the studies reviewed, he has greatest praise for the Popitz *et al.* study since it has the most comprehensive context for the discussion of the data and the interviews were least structured. Dahrendorf concludes, however, by being critical of these kinds of reports: he argues for works which present a comprehensive theoretical interpretation. It is difficult to argue against this, but it seems to me incorrect to belittle the contribution of straightforward research monographs. They provide data for testing and developing comprehensive theories, as well as having intrinsic interest and utility. Dahrendorf's criticisms of the research techniques used by the authors of these studies are well taken, but at least some of the inadequacies are attributable to the way the techniques were used and are not inherent in the techniques.

LOUIS KRIESBERG

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Casework: A Psychosocial Therapy. By FLORENCE HOLLIS. New York: Random House, 1964. Pp. xx+300. $5.95.

The author's stated concern in writing this book is "the analysis of a particular segment of casework practice—the treatment of individuals experiencing problems in their interpersonal relations."

A useful classification of treatment procedures is presented, with cases illustrating where the various techniques may be most suitable.

There is also a wealth of suggestions and admonitions for the practicing caseworker which, if followed, will undoubtedly be valuable in the therapeutic process. Many of these, however, serve merely to emphasize the depth of preparation which any therapist should have.

Dr. Hollis strives to depict casework as a unique kind of psychotherapy, more flexible in its methodology and broader in its theory than other helping professions. The potentiality is there, but her presentation leads one to wonder if casework is missing its opportunity to make a special contribution.

Traditionally and uniquely, social work has dealt with the "total" environment of the client. The capacity of the caseworker to treat with the family as a unit, and to intervene in the community, presents possibilities for helping beyond the limits of psychoanalyst, psychiatrist, or psychologist. For these external pressures are real and may be so damaging that while they continue unabated the client's intrapsychic processes have little chance of improving. But the important special advantages and disadvantages inherent in this approach are ignored in this volume.

The author suggests great flexibility of choice in treatment procedures. The caseworker is neither restricted to the traditional non-psychotherapeutic role of the social worker nor to the non-directive, non-intervening role of the psychoanalyst. Thus, while operating almost exclusively from Freudian principles, the caseworker is violating many basic Freudian tenets.

The modern concepts and findings derived from research on learning theory, group dynamics, and planned change could be utilized to formulate a theoretical basis for casework as psychosocial therapy. I fear that, failing to do this, Dr. Hollis left the caseworker where

she found him—without a satisfactory frame of reference.

ELLIOTT DANZIG

Laboratory of Social Relations
Harvard University

Eugenics: Hereditarian Attitudes in American Thought. By MARK M. HALLER. New Brunswick, N.J.; Rutgers University Press, 1963. Pp. 264. $6.00.

Animals and plants can be improved dramatically by selective breeding, so why cannot man himself be similarly improved by application of scientific principles? Although seemingly quite reasonable, this notion has served as a trap for powerful human emotions. Early in this century, the intellectual climate seemed right for the development of some programs of eugenics. The beginning was properly cautious and responsible. Very soon, however, and culminating during the 1920's, the movement degenerated into a morass of commingled science, politics, religion, and racism. Even before these attitudes hardened and were followed by the horrible genocide of the Nazi era in Germany, most persons of scientific bent had dissociated themselves from the eugenics movement. Nevertheless, the wave of hereditarian thinking that struck American life in the twenties has left deep scars and, indeed, many open wounds.

To unravel the influences that surrounded the rise of eugenical extremism is a formidable task. The author of this book, a historian, is admirably qualified and has done a magnificent job. With patient detachment, he sorts through the cross-currents of religion, politics, sociology, and biology and isolates the motivations and influences often cryptic and vaguely formulated, that have revolved around eugenics. All scientists concerned (the reviewer is a geneticist) will feel a debt of gratitude for the sure and penetrating competence that the author displays in his handling of this extraordinarily difficult task.

The book begins by tracing the origin of what the author calls hereditarian attitudes in American thought. To one who has a modern understanding of the delicate interplay of heredity and environment in almost every important human characteristic the earlier views are frequently incredible and sometimes chilling and abhorrent. The author manfully reports them as they were held and has generally been restrained in the use of adjectives such as this reviewer felt compelled to use in the preceding sentence. The tortuous reasoning of those who had formed a conclusion before considering the facts and then proceeded to bend every fact to fit the conclusion is portrayed in a direct, narrative style. Such restraint in a historian is admirable, of course, but I rather suspect that many readers of this book will find themselves pausing to wonder about the current status of some of these ideas. The emergence of modern understanding of heredity and environment is eventually clarified in late chapters; it is my feeling that this could have been done sooner.

Since the days of emphasis on laws requiring sterilization and the restriction of immigration, human genetics as a science has come of age. The geneticist, like every other scientist, has a dual responsibility to those who study and contemplate the meaning of his work. First, he owes allegiance to the rigors of scientific proof. Second, he must see that what he finds is not distorted by non-scientific zealots. To those who work in the biology of human heredity this latter consideration is of supreme importance because of the ease with which his science can be subverted to serve the ends of inhuman and ruthless bigotry. As cautious attempts are again made to put genetic knowledge to work to alleviate suffering from diseases with strong genetic components, the geneticist needs to be continually alert to the misuse of genetic facts.

This book, perhaps wisely, stops short of the attitudes in American life in the 1960's. We are now relieved of the "threat" from the Jukes and Kallikaks; we do not now blame alcohol for "hereditary degeneration" and our feebleminded are no longer considered a menace to our very existence. Certainly, however, no one can contemplate the civil rights debate of 1964 without realizing that "scientific" racism did not die with Hitler. The historical perspective provided by this book on past excesses arising from hereditarian attitudes is a valuable lesson for every sociologist and human biologist in this decade when distortion intensifies once again.

HAMPTON L. CARSON

Washington University
St. Louis, Missouri

Penology: A Realistic Approach. Edited by CLYDE B. VEDDER and BARBARA A. KAY. Springfield, Ill.: Charles C Thomas, 1964. Pp. 345. $9.50.

Any attempt to present an integrated picture of the field of corrections is a formidable undertaking. Changes in correctional practice and several notable research efforts over the past two decades have supplemented some older perspectives and challenged others, thereby complicating the task of selecting and organizing relevant materials. It is easy to flounder in the attempt to represent this field in a book of readings. Vedder and Kay do.

Except for the editors' preface of less than two pages, this "book" is a collection of articles bound together without comment or index. Approximately one-third of the thirty-one articles are reprinted from the *Journal of Criminal Law, Criminology and Police Science,* another third come from *Federal Probation,* and the remaining third are drawn from diverse sources including one article from an inmate newspaper. The papers are grouped into six sections: "Recapitulation," "Who Is the Inmate?" "Problems of Correctional Programs," "Probation and Parole," "Pros and Cons of Capital Punishment," and "Some Realistic Goals." The section titles constitute the only introduction that the editors have provided for the subdivisions; no editorial comment accompanies the individual papers. The fact that footnotes remain in the varied formats of the journals in which the papers were originally published is an additional indicator of the editorial effort invested in this volume.

It is perhaps unnecessary to say that the quality of the papers is uneven; that is inevitable. But this particular collection has probably set a record for variation in quality. Sellin's fine paper on capital punishment and the stimulating papers by Sykes, Hakeem, and Cressey approach the high end of the continuum. Even without these to provide a standard for comparison, papers approaching the opposite end of the scale would be clearly evident. One rough and very lenient index of intellectual quality may be the proportion of papers that cite references; one-third of the papers in this collection do not refer to any prior publication. The editors' criteria for selection are obscure, at least. What criteria could lead to the inclusion, for example, of a plodding paper detailing the medical problems of aged inmates?

The tedium of pedestrian papers is, happily, interrupted by an occasional touch of inadvertent humor. There is, for example, the statement that "individuals began to question the use of the death penalty except for capital crimes" (p. 250). I doubt that I shall soon forget the paper that begins with the statement: "At the outset I would like to make my personal position clear" and five lines later announces: "Therefore, I have refrained from taking any personal position on this question" (p. 266). And I was pleased to find that one researcher had had an enthusiastic reception of her questionnaire by women inmates, and amused at the researcher's equally enthusiastic way of reporting it: "Cooperation was tops. The girls themselves enjoyed the experience, and appeared to get a lot from it" (p. 66).

The editors indicate in the preface that these readings have been collected "for use in Penology and Correction courses at the college level, for practitioners in the field of correction, and for the discerning public interested in this important problem of social control." Fortunately, few in any of these potential audiences will take this volume as adequately representing the best efforts of the field.

HERBERT L. COSTNER

University of Washington

The Second Generation: A Study of the Family among Urbanized Bantu in East London. By B. A. PAUW. Cape Town: Oxford University Press, 1963. Pp. xviii+219. R.4.

This volume is one of a trilogy, "Xhosa in Town," that deals with the experience of the urbanized Bantu population of South Africa. Published under the auspices of the Institute of Social and Economic Research, Rhodes University, and edited by Philip Mayer, the series reports in detail the results of a survey of the Africans in the medium-sized city of East London. Earlier volumes examined the economic situation of the group and the special experience of those who have migrated from the countryside of their forebears. The present work focuses upon the permanent townsmen, the 14 per cent of the adult Bantu who were born in East London and who are therefore a second generation.

Dr. Pauw's study rests upon carefully assembled demographic, social, and cultural data. It analyzes the rural origins and tribal characteristics of the parents of the second generation and then proceeds to describe the urban location. These sections lead to an examination of the variety of cultural types and of the process of personal, family, and social adjustment. The material is coherently and convincingly presented and the conclusions are suggestive.

Dr. Pauw emphasizes strongly the influence of ideology on the process of urbanization. He distinguishes between those Xhosa whose values and attitudes are strongly oriented toward their tribal heritage and those who have accepted Western patterns, the latter group characteristically making a more successful adaptation to the life of town. Economic opportunity and education, both associated with the Western orientation, correlate positively with the ease of adjustment.

The findings on family life are also significant. The high rate of illegitimacy and the strong matrifocal tendency among the Bantu townsmen are both contrary to tribal tradition and both seem products of urbanization, which from the start devalues the husband's role and diminishes his authority. There is a useful basis here for comparison to urban experiences in other parts of the world, including the United States.

Oscar Handlin
Harvard University

Class and Nationality: English and American Studies. By Joel B. Montague, Jr. New Haven, Conn.: College and University Press (in association with Washington State University, Pullman, Washington), 1963. Pp. 246. $5.00.

This volume is a collection of eighteen essays and empirical studies dealing largely with social class in England. Some discussion of social class in the United States is included, mainly for purposes of comparison. Much of the material is taken from work previously published by the author and various collaborators over the last decade. New items appear to be little more than fillers to round out the collection, which is arranged in four parts.

Essays on the concepts of social class, nationality, and national character comprise the first part of the volume. These are little more than brief summaries of the work of selected authors and do not take us anywhere we have not already been. This section in particular was poorly edited and is filled with such ingenious remarks as "Karl Marx was not primarily a sociologist but rather should probably be classified as a classical economist" (p. 20).

Part II begins with an essay pertaining to class, status, and power in England which summarizes the positions taken by some British writers (e.g., Carr-Saunders, G. D. H. Cole, and Lewis and Maude) on such matters as the occupational composition of the middle class. The companion essay on the same subject for the United States hardly gets beyond the suggestion that power and economic interest have been neglected by the emphasis on prestige in American sociology and that these concepts would prove valuable "in analyzing social change, in studying social movements and in analyzing problems of social control" (p. 81). Neither essay fulfils the promise of the subtitle of the section to illustrate "the analytical use of the concepts of class, status, and power" and both have been outdated by recent work on power in the local community.

Part III is comprised by selected papers ranging over such diverse topics as "The Contemporary Working Class Situation," "Socialization of the Professions," and "The Welfare State and Social Class—Socialized Medicine and Education." Also included are brief reports of short-term participant-observation studies of class in two British towns. Unlike the essays in Parts I and II, the present ones do not attempt to manipulate concepts but are based on the author's observation of English social structure. Although I do not feel that I understand social class in England any better for having read them, the author does prove himself more competent at observation than at elucidating concepts.

The most useful part of the volume is the last, which contains some suggestive comparative studies. One paper originally published in 1954 demonstrates very clearly that incumbents of different occupational groups rate the prestige of occupations in much the same way. Although the paper is based on a small sample from a single city, the author should have received more acknowledgment for this effort than one finds in the literature. Another paper

compares the status terminology used by English and American boys, while a third contrasts the attitudes of white and Negro boys toward social class and social mobility. The latter paper provides some interesting differences: within each of two social class groupings about 30 per cent of white boys (sampled in Seattle and Spokane, Washington) affirm the statement that "some classes of people are not as good as other classes," whereas nearly 60 per cent of the Negro boys (sampled in the South) make a realistic assessment by affirming the statement. Precisely what this and similar contrasts may mean are never fully explicated in the text. Nor is the question of whether racial (white versus Negro) or regional (Pacific Northwest versus South) differences in the samples account for these contrasts ever examined in detail. But these comparisons, like those found in another paper contrasting the anxiety of English and American boys, are perhaps interesting in their own right. In general, the topics studied in Part IV are important ones, but the discussion is cursory and the data are poor. A generous estimate of this volume would be that with careful editing and expansion of his observational and emipirical studies the author might well have provided us with three or four—but surely not eighteen—worthy efforts.

Robert W. Hodge

National Opinion Research Center

The Cutteslowe Walls: A Study in Social Class. By Peter Collison. London: Faber & Faber, 1963. Pp. 194. 25*s*.

This fluently written and unpretentious little book, the first in a new series of books on contemporary social problems under the general editorship of A. H. Halsey, is not really "a study in social class"—as subtitled—but rather a sociological reportage on a well-known attempt to separate housing for different spatially contiguous classes of people in an Oxford residential district and on the attendant dispute.

A close relationship between social and spatial "distances" and some measure of residential segregation based on socioeconomic status are characteristic of all (stratified) societies. What makes the Cutteslowe Walls interesting is the artificial and contrived nature of the barrier, which has increasingly come to be felt as an affront by those whom it was intended to bar and regarded as a "social monstrosity" by the general public.

The story is as follows: In 1933, the city of Oxford sold to a private developer an area in north Oxford adjacent to its own public housing project. A year later, the developer, solicitous that there might be trouble between the middle-class residents of its own (private) Urban Housing Estate and the predominantly working-class Cutteslowe Estate people, decided to keep the roads on its estate as private roads and inclosed the estate by building walls across the two roads linking it with the council estate. All this was done purportedly in response to extreme pressure from the private estate tenants who were alarmed by the fact that some people displaced by slum clearance were being rehoused on the adjoining estate.

People from Cutteslowe Estate sought to bring about a removal of the walls in a variety of ways. Unsuccessful street demonstrations were followed by a more orthodox search for some legal means of bringing down the walls. A public inquiry and a series of legal proceedings meant only so many setbacks for the Corporation of the City of Oxford. It was only after the end of the war that new town-planning legislation (1947) provided for compulsory purchase of the strips of land on which the walls rested. It took seven more years for the city's development plan to be completed and another public inquiry before the compulsory purchase of the sites was authorized by the Minister of Town Planning. Search for the lawful owner of the walls, negotiation over price, and a title search caused further delay; but in March, 1959, after a quarter of a century since their erection, the walls were at long last demolished.

Appended to this odd chapter in local history is the analysis of findings of two surveys conducted by the author in 1956, after the Minister confirmed the compulsory purchase, and in 1960, after the walls were down, to determine the state of opinion in that area concerning the walls. Predictably, a large majority of respondents in Cutteslowe Estate favored the removal of the walls, whereas a majority of people at the private estate were in favor of keeping the walls in 1956, but their opposition to the removal of the walls decreased considerably by 1960. In accounting

for the change in attitude of Urban Housing people, the author offers a plausible hypothesis, that is, that as one way of accepting what became inevitable after the Minister's confirmation an increasing number of residents of the private estate came to the point of thinking not only that the walls had to come down but also that they ought to come down.

The book concludes with an all too brief survey of the relations between the two estates. We learn that the removal of the physical barrier lessened substantially also the psychological inhibitions on contact between the residents and brought about an over-all increase in favorable mutual attitudes, although at both points of time only very few close friendships existed between the residents of the two estates.

Collison's book is obviously of limited scope and does not invite a major criticism of the study design underlying it. The author's use of documentary evidence, such as committee minutes and newspaper files of the period, adds an important dimension to the book which a pure survey-type research would lack. His reliance upon these sources, however, is on the whole excessive, uncritical, and unnecessarily limited to official documents. In commenting on the long story of failure of the Corporation's many ventures in relation to the walls, Mr. Collison alludes to his suspicion of "blundering and incompetence in one form or another," but throughout the book he makes no effort to advance his analysis one step further and to explore the possible structural causes of what must be regarded as a monumental delaying action.

The interview schedule as reproduced is peculiarly bland and devoid of probing questions. The discussion of the relations between the estates is based solely on interview data, although much more could probably have been learned by observational techniques. The council estate and the private estate are used, at least by implication, as synonyms for working class and middle class. Although the two estates do contrast sharply in their social class composition, they are far from homogeneous, and it might have been profitable to pay greater attention to the "deviant minority" in each estate.

One of the useful conclusions which the reader may draw from this book is that even in a traditionally deferential society people resent too obvious a demonstration of social superiority and snobbishness on the part of their "betters," especially if the actual socio-economic distinction between them is relatively small and growing constantly smaller. But the book also intimates that an element of invidious separation and condescension is a very tenacious component of all status relationships which is likely to outweigh, and outlast, all forms of economic privilege and exploitation.

FERDINAND KOLEGAR
Roosevelt University

The Making of a Maori. By JAMES E. RITCHIE. Wellington, New Zealand: A. H. & A. W. Reed, 1963. Pp. 194. £1 1*s.* 6*d.*

This book reports a research project carried out by Dr. Ritchie and five students, under the direction of Professor Ernest Beaglehole, in the Maori community of "Rakau" during the four university summer vacations between 1953 and 1956.

Subtitled "A Case Study of a Changing Community," it is however a social-psychological study rather than a social-anthropological monograph reporting a community study.

The book is divided into four major parts. Part 1, "Rakau," describes the ecological, historical, and contemporary setting of the community in Maori and New Zealand society. Part 2, "Together and Against," describes community social organization in terms of topics "which tradition and practise within anthropology have endorsed as useful in describing the functions of any human group": kin, work, and religious groups, leadership, and companionship. Part 3, "The Divided Person," discusses personality development among the Rakau Maori, while Part 4 is concerned with the conflict in which the Maori is placed by the attractions of Maori goals on the one hand and those of white New Zealand society on the other.

It is in the third and fourth parts that the author's main interest clearly lies, and these seem to me the most valuable. The first two parts suffer from lack of a clear sociological perspective and do not add up to a coherent account of the community. The author would disarm such criticism with his claim that his analysis reflects the lack of structure in contemporary Maori society, but it is one thing to say that Maori society does not present a

perfectly integrated structure (surely something which no anthropologist would expect in *any* society) and another to assume that a purely sociological analysis is inappropriate. I feel that the author is trying not so much to analyze Rakau as a social group as to present social facts which we will need when we get to the psychological parts.

The discussion of kinship is unclear. Ritchie has not made up his mind whether traditional Maori kinship was patrilineal or bilateral (cf. pp. 18 and 47), and his use of terms such as "lineage" and "segmentation" does not accord with the quite precise usage worked out in recent social anthropology.

Some of Ritchie's sociological explanations seem unwarranted. For example, in a brief discussion of religion he establishes that little is known of traditional Maori religion and that life in contemporary Rakau is secular, with the people little committed to the various denominations to which they belong. But he concludes as if this lack of commitment were the deliberate result of the people's attempt to resolve the challenge which different religious affiliation makes to the social wholeness of their village by consciously avoiding involvement in religious concerns. It is a valid point that religion is not a major challenge to village integration because the people are not in fact deeply committed to their differing sects, but Ritchie's argument involves more than this and demands further evidence.

Despite the critical tenor of the remarks above I am nevertheless sure that this is a useful book. Modal personality is a variable too often ignored in analyses of the interaction of the constituent ethnic groups in multiracial societies, and Ritchie advances our understanding of Maori personality and its genesis and establishes its relevance to the understanding of Maori-Pakeha relations in New Zealand.

M. G. Swift

University of Sydney

Langa: A Study of Social Groups in an African Township. By Monica Wilson and Archie Mafeje. Cape Town: Oxford University Press, 1963. Pp. ix+190.

Monica Wilson and Archie Mafeje have provided the first adequate analysis of African townsmen for whom "the transformation from a society based primarily on kinship to one based on association is complete." The Langa townsmen of Cape Town "are no amorphous mass of persons but are linked in a network of families, churches, and clubs which unite those with a common interest, whether it be sport, music, dancing, politics, or merely smoking dagga." Most African towns are too new for this to have occurred. In the old towns of West Africa, despite their complexity, kinship was still the primary base.

But Langa also has migrants, despised as *amaqaba* who smear themselves with red clay, although in fact most have been to school. Not only are they unassimilated, but they may refuse assimilation to the immoral city, as Philip Mayer has shown in *Townsmen or Tribesmen*. The major determinants of categories are age and the composite factors of degree of urbanization and class. South Africa has bred a new set of colorful status identifications all its own. Next to the migrants are the *iibari*, the roughs or barbarians trying to assimilate but not yet accepted by the established townsmen. Not having been reared in town like the *ikhaba* (half-grown maize stalks), the younger *tsotsis* are angry young men, dressing flashily, speaking English ostentatiously, interested in liquor, clothes, women, and jazz shows, hoping at least to be eventually recognized as *ooMac* (a tribal nickname borrowed from Scotland), the elder category of *tsotsis* from about twenty-five to thirty-five years.

The established, educated middle class are *ooscuse-me* and the middle-aged respectable ones who tried but failed are *amatopi* after that ubiquitous African symbol of affected decency, the *solar topee*. The inadequacies of town life still show in the deep respect of the middle class for their ancestral home places and their tendency to send teenage children away from wild town life to rural boarding schools or relatives. All these categories are traced through the ramifications of kinship, churches, schools, traditional rites, clubs, classes and leaders, disputes and their arbitration.

The migrants live, eat, and even work in groups of "home-boys" (*abakhaya*) from one rural neighborhood. They have an elaborate and usually effective system of arbitration, based on traditional values, imposing fines and general reconciliation for minor offenses such as theft and insult. The respectable, hav-

ing eschewed such values, are unable to achieve this.

The tragic paradox of color runs all through. The only model for the respectable is the white model which rejects them. "Those with highest status in Langa are those who have absorbed most of Western culture." White organizations are emulated in the numerous rugger, soccer, and other sports clubs, music and dance clubs, choirs and churches. But, frustrated on all hands and starved of funds, leadership, and status, these associations are used not only for sport and recreation but as an arena for conspicuous giving and an occasion for the enjoyment of office and leadership, whereas in white clubs, with their fields and club-houses, members join for the sake of the game or for status, and office tends to be a burden. Ownership of property and success in common purpose discourage fragmentation. Langa clubs, lacking the former, split frequently in accordance with their achievements in the latter. Similarly, churches least attached to doctrine are most exposed to fission.

Despite the instability of the uncommitted migrant, the wilu violence of the *tsotsi* and the sophisticated aspirations of the educated balked at every turn by race prejudice and repressive legislation, Africans in Cape Town have achieved a social life and culture of which this book portrays the vivid color and the systematic structure.

AIDAN SOUTHALL

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Kampala

The Human Dimension in International Relations. By OTTO KLINEBERG. New York: Holt, Rinehart & Winston, 1964. Pp. viii+173.

Otto Klineberg opens the preface to this book by stating: "As this book goes to press, I face my potential readers with a mixture of confidence and concern. The confidence comes from a genuine belief in the contribution which the science of psychology can make to international relations, and from my conviction that I have brought together a wider range of relevant psychological material than can be found in any other single volume with which I am familiar." Nowhere in the book does Klineberg adequately defend the first statement, and my impression is that he did not accomplish the latter task.

The book that will accomplish the task which Klineberg set for himself is still to be written. Such a book must relate the principles and data of psychology to the variables of the other social sciences. In what matrix of variables does the decision-maker find himself, and what psychological factors will operate to influence the decision arrived at? No answer to this question can ignore the economic or political or sociological variables operating in the situation. Otherwise the plain psychological predictions will appear to be (and often are) naïve. Psychologists are typically "psychocentric" in their outlook. That is, they tend to assume that the social sciences can either be reduced to psychology or that psychological factors operate in a vacuum. In this book, Klineberg indicates no knowledge of the constraining forces in the environment of the actors on the world stage.

In anthropology we find L. White suggesting that psychology is irrelevant for analysis of culture and in political science we find Hans Morgenthau stating unequivocally that psychology has no place in the analysis of power. If psychologists are to convince others of the relevance of their subject matter to large international problems, it will be necessary to examine these arguments closely and meet them with sound counterarguments and with analyses which lend themselves to empirical test. Kelman's analysis was one such attempt as are the articles written by many others (e.g., Riesman, 1964). I suspect that one reason Klineberg avoided this kind of discussion was that he attempted unsuccessfully "to write for two distinct kinds of reader": the academic audience and the interested layman. Very few books are successful in this dual task. The book would be unsatisfactory in a social psychology course for the reason that there is almost no material that would not be covered more adequately by Krech and Crutchfield's *Individual in Society*. It is unsuccessful in appealing to the layman for the reason that Klineberg must make his points by referring in typical academic style to experiments to back up his generalizations. He might have been better advised to write in more dramatic style without the jerky tug and pull expected of the academic textbook.

The book discusses the bases of stereotypes, attitudes, attitude change, race, leadership,

opinion polling, and so on. In no way does the book hang together in terms of a genuine theory of the relationships of these variables, nor is any systematic program or strategy offered. It is a piecemeal hodgepodge of psychological studies almost all of which were done prior to the 1960's. There is no indication of the number of recent studies reported in the *Journal of Conflict Resolution* and the *Journal of Social Issues* or the number of studies reviewed as in progress by Feldman (1963). Much more interesting and probably more of a contribution would have been some more information about Klineberg's experiences with UNESCO. The stated assumptions of that organization and of some of the work it has been doing were of real interest.

Beyond his failures in systematization and in choosing an audience, Klineberg fails to include in his book a whole range of psychological contributions that can be made to the solution of outstanding world problems. Where is the indication that work in game theory is heavily influencing the military strategist? In what ways would living in a shelter-centered society affect the individual in a democratic society? What psychological variables operate in planning children and putting an end to the population explosion? What effect does the cold war have on the attitudes of children? The list of topics which are currently being investigated by psychologists but which are omitted from Klineberg's book could well be very long. Of course it is true that a small book like this cannot do everything, but then the author should not have claimed so much.

Psychologists have another important function to serve in producing the world without war that all intelligent people seek. A psychological analysis should be made of assumptions made by persons not schooled in psychology but who have high decision-making positions. Nowhere do we find in Klineberg's book an analysis of the dubious psychological assumptions that underlie the concept of deterrence, game theoretical formulations, balance of power theories, or even Osgood's strategy of unilateral initiatives. Yet this may in fact be the most important function that the psychologist has in terms of his political impact on shaping a safer world.

JAMES T. TEDESCHI

University of Miami
Coral Gables, Florida

The Education of Sociologists in the United States. By ELBRIDGE SIBLEY. New York: Russell Sage Foundation, 1963. Pp. 218. $3.50.

This report, prompted by a 1958 recommendation of the American Sociological Association's Committee on Training and Professional Standards, might be subtitled: "Graduate Training to What End?" In the absence of consensus among sociologists as to the prime objective of education in sociology, Sibley was compelled to define its aim for himself. This he takes to be the development of "a growing corps of professional sociologists who are both innately talented and well trained in all basic aspects of their science" (p. 27). Accordingly, he evaluates present practices and considers modifications in terms of their effects on the goal.

As part of his critical review, Sibley undertook personal visits to 13 universities offering doctoral training in sociology, where he interviewed staff and students. By questionnaire he surveyed 83 departments reported to offer doctoral degrees, of which 71 apparently do so; 68 returned information about faculty and student-body size and the curriculum. A request to 44 departments for names of terminal M.A.'s in the period 1953–58 yielded only 28 lists, however.

Professional sociologists also were surveyed, including all members of the ASA in 1959 reported to have received Ph.D.'s before 1936; a sample of ASA members reported to have received Ph.D.'s from 1936 to 1959, and a sample of non-members who received Ph.D.'s from 1936 to 1959 listed by the Office of Scientific Personnel, National Research Council. Because of incorrect or ambiguous information in the source lists, not all persons surveyed met the criteria. The pre-1936 Ph.D.'s were asked only age, sex, date of degree, awarding institution, and present employment. More recent Ph.D.'s were asked about their career line and training as well; and some received follow-ups on particular points. In addition, the "non-sample" of 454 terminal M.A.'s were surveyed about their training, present employment, career goals, and plans for the Ph.D.; of an estimated 359 eligibles, 271 provided information.

I cannot judge how much the survey results influenced the author's conclusions. The strength of this report, however, lies in the

judiciousness, sympathy, and open-mindedness of the critic; guided by a less knowledgeable man, the study probably would have floundered. The numerous non-respondents probably are selected on the aspects of training under scrutiny. Survey results can scarcely be accepted at face value. For example, 5 per cent of the Ph.D.'s report course work in mathematical statistics with no work in college mathematics. Men trained in the fifteen "prestigious" departments consistently report less satisfaction with their training than those trained elsewhere.

It is, then, not surprising that Sibley's recommendations seem only remotely linked to the materials gathered. By this I mean, for example, that he regards the median interval of nearly ten years including "5.4 years of 'predoctoral professional experience'" (p. 100) between Bachelor's and Ph.D. degrees unduly long and recommends "conferral of the doctoral degree after three or four years, followed by a postdoctoral internship" (p. 43). If these patterns are simply alternative routes to independent research and scholarship, a preference for one over the other might rest on a demonstrable difference in subsequent performance as professionals among men trained in the respective styles. Potentially relevant survey data are not brought to bear on the issue.

We have here some thoughtful observations on the state of the profession, the lack of mutually accepted standards with respect to professional training, the weakness in recruiting and softness in judging student performance, the discontinuity between undergraduate and graduate education, and other issues which should be of concern to professional sociologists. Three courses of action now are open: (1) ignore the report; (2) try to check empirically the propositions set forth; or (3) try to reach agreement on the minimal qualifications of a professional sociologist and set about implementing their fulfilment.

BEVERLY DUNCAN
University of Michigan

Education, Manpower, and Economic Growth: Strategies of Human Resource Development. By FREDERICK HARBISON and CHARLES A. MYERS. New York: McGraw-Hill Book Co., 1964. Pp. xiv+229. $7.50.

In this study covering seventy-five nations, the authors "have analyzed economic, political, and social development from the perspective of the education, training, and energizing of human resources" (p. v). The study focuses upon human resources as the point of departure, on the assumption that the degree of "human resource development . . . may be a more realistic and reliable indicator of modernization or development than any other single measure" (p. 14). Although the authors note that human resources are developed in many ways, they focus on formal schooling and on-the-job apprenticeship. In fact, a "composite index" used to rank the nations is based *entirely* on the proportion of school-age persons enrolled in high school and university. No systematic attention is given to the educational impact of participation in revolutionary, political party, labor union, entrepreneurial, and other activities. Surprisingly, the potential educational function of military service is passed over in a single brief paragraph. As the book purports to be "designed as a blueprint for action rather than solely as a scholarly academic exercise" (p. v), the focus on orthodox schooling is unfortunate.

The composite index of formal education is used to group the seventy-five nations into four "levels of human resource development." The educational needs of each level are then discussed, and reference is made to the peculiar problems of many individual nations. Besides published reports the authors use unpublished studies made available by various planning commissions.

The first group, called "underdeveloped," consists primarily of African nations. The authors recommend giving priority to secondary education to increase the number of trained functionaries, in-plant apprenticeship training financed by corporations, scholarships for foreign university training rather than building a university at home, and utilization of "expatriate" experts.

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Third-level nations are termed "semi-advanced." They include primarily the richer South American countries and those of south

opinion polling, and so on. In no way does the book hang together in terms of a genuine theory of the relationships of these variables, nor is any systematic program or strategy offered. It is a piecemeal hodgepodge of psychological studies almost all of which were done prior to the 1960's. There is no indication of the number of recent studies reported in the *Journal of Conflict Resolution* and the *Journal of Social Issues* or the number of studies reviewed as in progress by Feldman (1963). Much more interesting and probably more of a contribution would have been some more information about Klineberg's experiences with UNESCO. The stated assumptions of that organization and of some of the work it has been doing were of real interest.

Beyond his failures in systematization and in choosing an audience, Klineberg fails to include in his book a whole range of psychological contributions that can be made to the solution of outstanding world problems. Where is the indication that work in game theory is heavily influencing the military strategist? In what ways would living in a shelter-centered society affect the individual in a democratic society? What psychological variables operate in planning children and putting an end to the population explosion? What effect does the cold war have on the attitudes of children? The list of topics which are currently being investigated by psychologists but which are omitted from Klineberg's book could well be very long. Of course it is true that a small book like this cannot do everything, but then the author should not have claimed so much.

Psychologists have another important function to serve in producing the world without war that all intelligent people seek. A psychological analysis should be made of assumptions made by persons not schooled in psychology but who have high decision-making positions. Nowhere do we find in Klineberg's book an analysis of the dubious psychological assumptions that underlie the concept of deterrence, game theoretical formulations, balance of power theories, or even Osgood's strategy of unilateral initiatives. Yet this may in fact be the most important function that the psychologist has in terms of his political impact on shaping a safer world.

JAMES T. TEDESCHI

University of Miami
Coral Gables, Florida

The Education of Sociologists in the United States. By ELBRIDGE SIBLEY. New York: Russell Sage Foundation, 1963. Pp. 218. $3.50.

This report, prompted by a 1958 recommendation of the American Sociological Association's Committee on Training and Professional Standards, might be subtitled: "Graduate Training to What End?" In the absence of consensus among sociologists as to the prime objective of education in sociology, Sibley was compelled to define its aim for himself. This he takes to be the development of "a growing corps of professional sociologists who are both innately talented and well trained in all basic aspects of their science" (p. 27). Accordingly, he evaluates present practices and considers modifications in terms of their effects on the goal.

As part of his critical review, Sibley undertook personal visits to 13 universities offering doctoral training in sociology, where he interviewed staff and students. By questionnaire he surveyed 83 departments reported to offer doctoral degrees, of which 71 apparently do so; 68 returned information about faculty and student-body size and the curriculum. A request to 44 departments for names of terminal M.A.'s in the period 1953–58 yielded only 28 lists, however.

Professional sociologists also were surveyed, including all members of the ASA in 1959 reported to have received Ph.D.'s before 1936; a sample of ASA members reported to have received Ph.D.'s from 1936 to 1959, and a sample of non-members who received Ph D.'s from 1936 to 1959 listed by the Office of Scientific Personnel, National Research Council. Because of incorrect or ambiguous information in the source lists, not all persons surveyed met the criteria. The pre-1936 Ph.D.'s were asked only age, sex, date of degree, awarding institution, and present employment. More recent Ph.D.'s were asked about their career line and training as well; and some received follow-ups on particular points. In addition, the "non-sample" of 454 terminal M.A.'s were surveyed about their training, present employment, career goals, and plans for the Ph.D.; of an estimated 359 eligibles, 271 provided information.

I cannot judge how much the survey results influenced the author's conclusions. The strength of this report, however, lies in the

judiciousness, sympathy, and open-mindedness of the critic; guided by a less knowledgeable man, the study probably would have floundered. The numerous non-respondents probably are selected on the aspects of training under scrutiny. Survey results can scarcely be accepted at face value. For example, 5 per cent of the Ph.D.'s report course work in mathematical statistics with no work in college mathematics. Men trained in the fifteen "prestigious" departments consistently report less satisfaction with their training than those trained elsewhere.

It is, then, not surprising that Sibley's recommendations seem only remotely linked to the materials gathered. By this I mean, for example, that he regards the median interval of nearly ten years including "5.4 years of 'predoctoral professional experience'" (p. 100) between Bachelor's and Ph.D. degrees unduly long and recommends "conferral of the doctoral degree after three or four years, followed by a postdoctoral internship" (p. 43). If these patterns are simply alternative routes to independent research and scholarship, a preference for one over the other might rest on a demonstrable difference in subsequent performance as professionals among men trained in the respective styles. Potentially relevant survey data are not brought to bear on the issue.

We have here some thoughtful observations on the state of the profession, the lack of mutually accepted standards with respect to professional training, the weakness in recruiting and softness in judging student performance, the discontinuity between undergraduate and graduate education, and other issues which should be of concern to professional sociologists. Three courses of action now are open: (1) ignore the report; (2) try to check empirically the propositions set forth; or (3) try to reach agreement on the minimal qualifications of a professional sociologist and set about implementing their fulfilment.

Beverly Duncan
University of Michigan

Education, Manpower, and Economic Growth: Strategies of Human Resource Development. By Frederick Harbison and Charles A. Myers. New York: McGraw-Hill Book Co., 1964. Pp. xiv+229. $7.50.

In this study covering seventy-five nations, the authors "have analyzed economic, political, and social development from the perspective of the education, training, and energizing of human resources" (p. v). The study focuses upon human resources as the point of departure, on the assumption that the degree of "human resource development . . . may be a more realistic and reliable indicator of modernization or development than any other single measure" (p. 14). Although the authors note that human resources are developed in many ways, they focus on formal schooling and on-the-job apprenticeship. In fact, a "composite index" used to rank the nations is based *entirely* on the proportion of school-age persons enrolled in high school and university. No systematic attention is given to the educational impact of participation in revolutionary, political party, labor union, entrepreneurial, and other activities. Surprisingly, the potential educational function of military service is passed over in a single brief paragraph. As the book purports to be "designed as a blueprint for action rather than solely as a scholarly academic exercise" (p. v), the focus on orthodox schooling is unfortunate.

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and east Europe. Priority should be given to higher education, primarily research institutes, to adapt new inventions to local needs. A problem here is the *over*supply of humanities graduates; the authors propose solving this problem by "restructuring incentives" so that persons will train for critical but unprestigious positions. Nationalist and revolutionary elites characteristically manipulate not just "incentives" but the entire value system; it would seem impossible to analyze "manpower development" without examining these efforts.

The nations of northern Europe and their former settler colonies plus Japan comprise the fourth level, termed "advanced." Policy recommendations focus on broad education, retraining, and periodic refresher courses to increase flexibility necessary for rapid technological change.

The final three chapters deal with "targets for development" expressed in such general terms as to be of little use to research sociologists. Strategy is formulated without concern for *who* does the planning beyond cryptic statements such as "priorities have to be determined by the country's responsible leaders" (p. 203). Only the final four paragraphs of the book are devoted to "problems of implementation."

The chapters dealing with the four levels are most useful in providing a broad view of the state of education throughout the world and provide a basis for further comparative analysis.

RICHARD A. PETERSON

University of Wisconsin

Emerging Techniques in Population Research. By MILBANK MEMORIAL FUND. New York: Milbank Memorial Fund, 1963. Pp. 307. $2.00.

That new demographic techniques are emerging is amply demonstrated in this bargain-priced volume, which consists of fifteen papers presented at the Annual Conference of the Milbank Memorial Fund in September, 1962.

The most exciting new technique is Ryder's translation model of demographic change. This model, which details interrelations between cross-sectional data and longitudinal processes, promises to have important applications in many studies of social change.

Coale's paper on "quasi-stable" age distributions is an important extension of formal demographic analysis permitting the sophisticated utilization of limited bits of data to obtain useful estimates of basic demographic rates for areas of the world with poor statistical systems.

Seven papers are concerned with fertility. Those by Goldberg and Coombs, Sagi and Westoff, and Potter and Tietze are research reports which demonstrate the fruitfulness of the increasing level of statistical sophistication in demographic analyses. Four papers are programmatic, suggestive of the wide diversity of interests and approaches in fertility research—attitudinal surveys (Campbell), cohort methods (Whelpton), analyses of census and vital statistics data (Kiser, Grabill, and Schachter), and action programs to lower birth rates (Berelson).

Mortality research is represented by three papers. Bourgeois-Pichat reports one phase of his ingenious and controversial factor analyses of age-sex-specific mortality rates. Spiegelman discusses the new vital and health statistics monograph program. Kitagawa and Hauser discuss their study of social and economic differentials in mortality, based on the collation of death certificates, which contain little useful socioeconomic information, with census returns, which contain a wide variety of such data. Matching studies and record-linked surveys have great potential for overcoming many of the limitations imposed by the demographer's traditional reliance on discrete bodies of official data.

Migration research, despite many interesting developments in recent years, is touched upon only in a paper by Price on the use of computers in the simulation of migration and in facilitating the handling of bulky origin-destination tables that to date have always been grossly underanalyzed. Computers have already had a large impact on data-processing, and reports by Brunsman on the use of computers at the Bureau of the Census and by Linder and Simmons on their use at the National Center for Health Statistics make it abundantly clear that not only do these tools expand the statistician's ability to perform old tasks more quickly and reliably, "but even more significant is the possible impact of the

computer on the statistician, as it influences . . . his methodology, his patterns of thinking, and even his choice of areas of study" (p. 303).

In sum, this symposium includes individual articles of generally high quality and great interest to demographers, but does not offer the balanced view of the entire spectrum of demographic research or the comprehensive compendium of new techniques that might be of interest to the sociologist with less of a demographic bent.

KARL E. TAEUBER
University of Wisconsin

Vilfredo Paretos System der Allgemeinen Soziologie ("Pareto's Systematic Sociology"). By GOTTFRIED EISERMANN. Stuttgart: Ferdinand Enke Verlag, 1962. Pp. vi+264.

Dr. Eisermann, who is professor of sociology at the University of Bonn, tells us that this volume represents the first time that the "complete sociological theory" of Pareto has been made available in German. The lack of resonance of Pareto's work in Germany is indeed a curious phenomenon, and Eisermann appears to be the major German sociologist intent on filling the gap, as evidenced by several valuable articles and monographs which he has published within recent years. (See, e.g., his "Pareto als politischer Denker," *Kölner Zeitschrift für Soziologie und Sozialpsychologie*, Vol. XIII.) The work under review consists of an introduction and selections, with editorial notes, from the *Trattato*.

The introduction is not a systematic appraisal of Pareto's sociology but rather a biographical sketch of the "sage of Celigny," coupled with a delineation of the social and historical milieu in which he lived and worked. Eisermann draws upon a wealth of materials, of which the most important is the recently published collection of letters written by Pareto to his friend, the economist Maffeo Pantaleoni. (American readers will find a more accessible source—the charming reminiscences of Manon Michels-Einaudi, "Pareto as I Knew Him," in the *Atlantic Monthly* of September, 1935.) Eisermann's stance is sympathetic to his subject but stops short of uncritical adulation. He has performed a valuable task for his German readers by letting "the real Pareto" speak for himself.

Pareto considered himself the "unattached intellectual" par excellence and hence particularly well equipped for the scientific study of sociology. In one of his letters he wrote, "not because of my own merits, but because of the circumstances in which I found myself, I possess no biases of any sort and thus need not be concerned, unlike others, about doing scientific work in this area. I'm bound to no party, no religion, and no sect; therefore I have no prejudices about phenomena. Nor am I tied to any country and therefore am free of the patriotic biases which cause so much damage in the social sciences. I demand nothing, hope for nothing, and fear nothing, and therefore nothing prevents me from telling the truth" (p. 21). Unlike a Weber or a Durkheim he insisted that "for the scientific study of sociology one must in the present historical circumstances (though not necessarily at all times) stand absolutely outside the life of action and live the life of a hermit as I do" (p. 22).

It is only too easy to show up Pareto's ideological biases by turning his methodological weapons upon their inventor. What is more important from the standpoint of the sociology of knowledge is the problem of the relative role of the "circumstances" in which he "found himself." Were they not to a great degree of his own making? Even though he failed abysmally in his first attempt to enter politics actively he could have tried again. He could have had a successful career as an engineer, and he might have led a secure, if inconspicuous life as a scholar in his native Italy. The fact that his "residues" contributed to his thought as much as his social detachment cannot be gainsaid. Pareto's recognition of this possibility may be at the bottom of what Livingston tells us in his preface to *The Mind and Society*, namely, that "Pareto was most averse to any introduction that should attempt to summarize, epitomize, or otherwise interpret his thought. He left directions covering the point with his heirs."

The initial impact of Pareto on American sociologists and intellectuals generally was intense but shortlived. It is to be hoped that Eisermann's efforts will lead to a revival of interest in the work of one of the outstanding practitioners of "grand theory."

WALTER HIRSCH
Purdue University

Social Science and Political Theory. By W. G. RUNCIMAN. Cambridge: Cambridge University Press, 1963. Pp. 200. $3.95.

W. G. Runciman is an outstanding British political philosopher who has become interested in political sociology, and this volume presents his overview of the subject. He presents his appraisal of the leading figures of political sociology—Marx, Weber, Schumpeter—and evaluates the work of contemporary American political sociologists—both empirical and theoretical. The author has a clear programmatic objective in that he argues that "the empirical and philosophical study of politics has more to gain from a maximum contact with each other than many of the practitioners—whether American sociologists or Oxford philosophers—have been apt to suppose." But it would indeed be unfortunate if it were to be seen merely as another step in the cautious and by now tedious effort to overcome opposition to sociology in Great Britain. The book is a worthy intellectual contribution in its own right.

First, I do feel he neglected to highlight issues in political philosophy on which the mutual interaction with political sociology would be relevant. The formulation of the political process derives mainly from sociologists. Second, there is a kind of gentlemanly and casual approach to the subject matter of contemporary political sociology. Runciman is undoubtedly proud of how much of political sociology he has mastered, but what is presented is hardly a balanced overview or selection along some organizing principle. The result is that election studies are grossly overplayed and the effort to understand the political structure in the United States at the community organizational and professional level is completely untouched. Third, there is an effort to mold contemporary political sociology into some holistic unity. For this purpose contemporary political sociology is dumped into a functionalist category in order to juxtapose it to the Marxist approach. As a result Runciman fails to appreciate the diversity of interests and the cumulative and trend aspects of the empirical research.

However, there is something eminently sensible about the author's perspective, his observations, and his conclusions. He is able to cut through to core issues. His formulation of the classical writers and how they have influenced the frame of reference for contemporary political sociology is first-rate. He see equally well the crucial contributions of the central transitional figures such as Joseph Schumpeter, in seeking to develop some evaluative criteria for judging the political processes of a democratic society. And there is much that is sensible as he deals with the problems of causation and inference in the study of political processes. And his devasting criticism of the naïve tautologies of particular survey researches is very much to the point. But there is little by way of rigorous formulation or new conceptualization. It is indeed interesting for sociologists to read this book in order to see how others see us, and if faint praise is not damnation, it is indeed a useful book to assist a graduate student in familiarizing himself with the field and in preparation for his examinations.

MORRIS JANOWITZ

University of Chicago

Sociology: A Guide to Problems and Literature. By T. B. BOTTOMORE. London: George Allen & Unwin, Ltd., 1962; Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. 330. $5.95.

This book is the result of a request made to UNESCO by the Indian National Commission to UNESCO, which wanted an introduction to the concepts, theories, and methods of sociology presented in a way that would have some relevance to the culture and institutions of India. T. B. Bottomore, reader in sociology at the London School of Economics and formerly executive secretary of the International Sociological Association, accepted the assignment and was able to discuss its content during the period of its preparation with a number of Indian sociologists in India.

Bottomore has succeeded in producing a text that is much like its American counterparts except that its illustrations and many of its references are to India and its history. In discussing the story of sociology, as he does at the beginning, he makes a number of interesting and useful observations. One of these is that the encyclopedic conception of the discipline that obtained in the nineteenth century, however obsolete it may now seem, had at least the advantage that sociology then

was less provincial and even less ethnocentric than the sociology of the last few decades, in which sociologists have tended to focus their research on small segments of their own societies. Another is the penetrating point that sociology today repeats within itself those divisions—political, economic, religious, and family institutions—which reflect an older classification of the social sciences and one which it was originally designed to transcend.

The organization of the book is a fairly standard one. There are separate parts devoted to the scope and methods of sociology, population and social groupings, social institutions, social control, social change, and applied sociology. With respect to approach, the author is as critical of functionalism as he is of evolutionism—the former because of its misleading view of social cohesion—and also expresses skepticism about a too-rigid adherence to the canons of a strictly scientific method in sociological research on the ground that it tends to create a conservative attitude with respect to the larger issues of society.

Because of its unusual emphasis upon India, Bottomore's book will have little use or influence in the United States. It nevertheless merits the attention of those who can appreciate a straightforward, unpretentious, and knowledgeable contribution to general sociology.

ROBERT BIERSTEDT

New York University

Explorations in Social Change. Edited by GEORGE K. ZOLLSCHAN and WALTER HIRSCH. Boston: Houghton Mifflin Co., 1964. Pp. xxviii+832. $10.95.

This is not a compendium of previously published papers or bits of books, but a symposium written to order by a variety of hands. This element of deliberate construction, together with conscientious editorial introductions to each of the book's major sections, provides a measure of continuity and orderly sequence lacking in the ordinary reader. It is truistic to say that a symposium is uneven, but I must record that even the organization of the book strikes me as having little logic; randomization of the topical order would make little difference. The book also appears to me unnecessarily long; some chapters are prolix, and I feel that the lack of a clear organizational scheme has tempted the editors to range far too widely in the quest for contributions.

Nevertheless, this book was on balance eminently worth doing and thus is worth reading, at least selectively. Various readers will of course be drawn to different topics. I found Section 1 on "General Perspectives on Social Change" and Section 8 on "Special Areas of Institutional Change" particularly chaotic, though many individual chapters have very high merit. Largely because of the strong hand of the senior editor as author of the four chapters of Section 2 ("Working Papers in the Theory of Institutionalization") and a good brief editorial introduction to Section 3 ("Social System Models of Change"), those two sections form a fairly coherent package of goodies. Though any selection by me will be improperly invidious, I especially liked Boskoff's "Function Analysis as a Source of a Theoretical Repertory and Research Tasks in the Study of Social Change." Beshers' "Mathematical Models of Social Change" is a marvel of clarity, and clearly on the right track.

Section 4 is devoted to "Psycho-social Models of Change" and Section 5 to "Broad Historical Perspectives on Change." This latter section opens with Louis Schneider's "Toward Assessment of Sorokin's View of Change," followed by Sorokin's reply. It is an instructive exchange, though both chapters would have benefited by stronger editing with the aim of abbreviation. Section 6. "Cultural Change, Culture Contact, and Social Movements," has about as much diversity as the heading advertises. Section 7, on "Economic Development and Cultural Change," I found particularly disappointing, although on this subject other sources are fairly readily available.

It is not just in its large size that this book must be regarded as a major contribution to social dynamics. Indeed, as I have already indicated, I think a shorter and more coherent volume would have been preferable. Yet some new descriptive knowledge is now available, together with attempted systematization, both partly at the hands of relative newcomers to sociological writing. It is an encouraging development.

WILBERT E. MOORE

Russell Sage Foundation

Internal War: Problems and Approaches. Edited by HARRY ECKSTEIN. New York: Free Press of Glencoe. Pp. 336. $6.50.

What should be more natural for the sociologist interested in the problems of social order and social change than the study of revolution and internal war? Yet, in a decade when political sociology has grown faster, possibly, than any other specialization in the discipline, it is ironic that not a single sociologist of the first rank has made his reputation through the analysis of revolution and rebellion.

Macro-sociologists have avoided the study of internal war because they have been more concerned with structure than with change and process. But I suspect that a trained incapacity to deal in detail with historical materials compounds the problem.

Eckstein's book is a testimony to the problem of studying internal war and to the state of our knowledge in this area. Composed of twelve essays by a variety of social scientists, the volume is the result of a conference sponsored by Princeton's Center of International Studies. Of the five distinguished sociologists contributing to the volume—Arnold Feldman, Marion Levy, Seymour Martin Lipset, William Kornhauser, and Talcott Parsons—only Kornhauser is a student of rebellion and revolution. His paper, one of the best in the volume, is an attempt to relate rebellion to the level of centralization and type of legitimation of political authority in the society. The other sociologists dodge the issue: Parsons, following his recent style of elaborating concepts left untouched in his major books, deals with the role of force in the social process; Lipset presents part of his comparative analysis of values and social structure published in *The First New Nation* (New York: Basic Books, 1963) (as usual, his footnotes are an education in themselves); in spite of himself, Levy presents an interesting discussion of the role of norms of tolerance and rationality in disrupting traditionalistic societies; and Feldman presents some assumptions and concepts useful in examining conflict management and potential in a society. Except for Kornhauser, the sociologists contributing here do not even try to come to grips with concrete cases or to present propositions in a way that would be useful for research purposes.

Some of the political scientists do better. At least Thomas Perry Thornton attempts to specify the timing and role of terror in insurgency and counter-insurgency, and Sidney Verba and Gabriel A. Almond present interesting materials from their cross-nation study of political cultures that show the effect of Mexico's successful revolution on the social-psychological ties of the citizenry to the government.

Without commenting on each essay, what can be said of the volume as a whole? I think the volume is a failure. Partly the failure may have been the fault of the organization of the original conference. If the organizer of the conference had had a general framework for analysis of internal war, specific essays could have been commissioned instead of this unrelated potpourri.

MAYER ZALD

University of Chicago

History, Psychology, and Science: Selected Papers. By EDWIN G. BORING. New York: John Wiley & Sons, 1963. Pp. xii+372. $8.95.

In this book, the distinguished Harvard psychologist presents to us part of his life's harvest. The essays collected together range over a wide field, but they can be grouped under five headings: the *Zeitgeist* and the psychology of science, the history of psychology, the scientific method, the mind-body problem, and the psychology of communicating science. For the sociologist, only the first and the last sections are directly relevant, and as the last comprises only a very few occasional papers, it is the first that really matters here. One's interest is immediately aroused when one reads that Boring is developing a "sociology of science."

Unfortunately, one's fond hopes are soon dashed. From the passage which best expresses the author's fundamental attitude, we can see that he so defines his terms that they cannot accommodate a true sociology of scientific knowledge. There are, we are told, two views of the operation of the human mind, the one personalistic, the other naturalistic. "The personalistic view is circumscribed. It exhibits man as a free originator, the genius as existing in his own right. . . . The naturalistic view is broader and is capable of including the personalistic view. It shows genius, not only as the

antecedent of new thought, but also as the consequence of its antecedent conditions. Thus the personalistic view preserves the dignity of man, for it leaves him a free agent accomplishing great deeds by the voluntary exercise of his capacities; whereas the naturalistic alternative robs man of his dignity by taking away his mystery and showing up his achievements and capacities as consequences of the conditions which create them. Science is always the great debunker because it reduces choice to necessity" (p. 30).

The sociologist will find in this highly characteristic passage two false antitheses. The first is that between nature and person. Kant saw matters in this light, but we are living five or six generations later. We have learned to understand that between nature and person there are the crucial realities of society and culture, and anyone who disregards them in an attempt to analyze thought cannot possess the main key to the whole phenomenon. The second false antithesis is that between determination and freedom. Since the sociological specialty called the sociology of knowledge has come into existence, it has increasingly been borne in on us that this confrontation is far too simple. Concepts like multiple determination and multivocal determination have been introduced; and, indeed, the whole idea of determination has been refined and redefined until the contrast between superpersonal tendency and personal expression has been reduced to its proper dimension—indeed, has been well-nigh dissolved. It is sad to see that so widely read, well-read a psychologist as the author of this volume is completely unaware of the work that has been going on in a sister science.

When it comes to more detailed investigations, we can yet learn much from Boring. There is, for instance, his enlightening discussion of why, in the clash between the "Great Men Theory" and the "Great Events Theory," so many have plumped for the former alternative which to him appears decidedly inferior. There is, first of all, the fact that ascription of an achievement to one person rather than to many is a great simplification, and our mind is such that it craves for simplification, to bring things into focus and help memory. Second, man is an inveterate hero-worshiper. To accept one founder, one father, means to contract into a fellowship—and who is not happy to do just that? Third, man is ambitious. To become great, rather than to make a great discovery, is the true aim of many scientists. Thus it suits them to see the history of ideas as a succession of eponyms rather than as an anonymous process. According to Boring, the historian of science must eschew such error-producing prejudices. The truth as it appears to him is that "currents of credence" dominate our thinking, and these currents are, in Durkheim's terminology, collective rather than individual representations.

WERNER STARK

Fordham University

Six Cultures: Studies of Child Rearing. Edited by BEATRICE B. WHITING. New York: John Wiley & Sons, 1963. Pp. 1017. $12.50.

Six Cultures is the first of a projected series of publications reporting the results of research undertaken in 1954 by teams of social scientists from Harvard, Yale, and Cornell universities. The general focus of this extensive study has been on the cross-cultural exploration of the relation between different patterns of child-rearing and subsequent differences in personality. The personality areas given most attention were aggression, dependency, and the internalization of various mechanisms of behavior control, for these were areas of special theoretical interest to the senior investigators, William W. Lambert of Cornell, Irvin L. Child of Yale, and John W. Whiting of Harvard. One of several unique aspects of this study is the attempt to assess individual as well as cultural differences, to test hypotheses on the relations of child-rearing practices and consequent personality not only cross-culturally, but intraculturally.

The six field teams each spent from six to fourteen months during 1954 and 1955 gathering the cultural and personality material for the study. The present volume is essentially six separate ethnographic reports prepared by these field teams, guided by an agreed-upon format for presenting and partially analyzing the cultural material they had gathered: Robert A. LeVine and Barbara B. LeVine on "Nyansongo: A Gusii Community in Kenya"; Leigh Minturn and John T. Hitchcock on "The Rajputs of Khalapur, India"; Thomas W. Maretzki and Hatsumi

Maretzki on "Taira: An Okinawan Village"; Kimball Romney and Romaine Romney on "The Mixtecans of Juxtlahuaca, Mexico"; William F. Nydegger and Corinne Nydegger on "Tarong: An Ilocos Barrio in the Philippines"; and John L. Fischer and Ann Fischer on "The New Englanders of Orchard Town, U.S.A."

The organization of these reports follows the conceptual system developed by Child, Lambert, and Whiting. There are two main sections to each report: Part I covers the ethnographic background, a description of the adult world into which the child is born, including the environment and local setting, the village plan, the houses and their interior arrangements, a profile of the daily routine of living and the economic activities of men and women in the society. Ecology, economics, and social and political organization are considered as the maintenance systems which set the parameters for the behavior of the child-rearing agents. Part II of the reports focuses on a chronological description of how the child is trained. This chronology of child development stops with the preadolescent stage in all except the LeVines' study of Nyansongo, which includes considerable detail on adolescence as well. This was a practical decision necessitated by the voluminous detail the field teams had to secure; but it represents a limitation to any future social anthropologist who might wish to utilize the rich detail of these cultural reports for analytic purposes different from those guiding the investigators of the present study.

Both parts of the reports, the adult culture as well as the child-training of each society, present material relevant to nine behavior systems: succorance, nurturance, self-reliance, achievement, responsiiblity, obedience, dominance, sociability, and aggression. It was assumed that each of these behavior systems would exist in some form and to some degree in every society. Furthermore, they represented the core systems of most relevance to the central hypotheses of the study. At the heart of each team's field research was a concentrated study of twenty-four mothers, studied as individuals in their relationship to one of their children, and of each of these twenty-four children (ages 3 to 10), who were observed and interviewed in great detail. Although some projective techniques were utilized, they are clearly subordinated to the systematic observation of concrete behavior. The behavior of the child is viewed as an index of child personality; the behavior, beliefs, and values of the adult are seen as indexes of adult personality.

This is not a book that can be evaluated in any usual review fashion. It is the cultural data volume to be followed by at least two major analytic monographs, in which the material in the present volume will be used to test hypotheses in a detailed and systematic way. The study cannot be assessed until these analytic volumes are published. I think it is unfortunate, however, that the senior investigators did not prepare a separate volume to precede or accompany *Six Cultures*, or a lengthy introductory chapter in the volume under review, in which they develop the conceptual framework and the hypotheses of the study in an orderly and systematic way. The draft of such a document exists in the "Field Guide for a Study of Socialization in Five Societies," by John W. Whiting *et al.*, but this is a mimeographed document and hence not generally available to readers of *Six Cultures*.

A thousand pages of detailed cultural material merit more than Beatrice Whiting's thirteen-page introduction, yet this, together with general familiarity with the previous work of Irvin Child, William Lambert, and John Whiting, constitutes the only guide to the data that is offered to the reader. What is lacking is a detailed presentation of the conceptual system, the hypotheses to be tested, and the methods by which they are to be tested in the volumes to follow. Until this gap is filled, and the analysis volumes are published, *Six Cultures* can only be commended for its six individual contributions of finely detailed accounts of child-rearing practices, with suggestions rather than confirmations of their impact upon adult personality.

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A Durkheim Fragment

George Simpson

ABSTRACT

In 1921 Marcel Mauss published from manuscript and with notes of his own the last lecture of a course on the family which Émile Durkheim had given at the University of Bordeaux in 1892. This article is an edited English translation, with an introduction by the American editor, of this historical document—one of the earliest indications of the direction some of Durkheim's later work was to take and an early contribution to the sociology of the family in its own right.

In preparing a book on Émile Durkheim containing an arrangement of selections from his work in sociology with commentaries and a short introduction,* I sought to include some piece of systematic thinking by him on the family. I could find readily available only an article published posthumously in *Revue philosophique* (XCI [January–June, 1921], 1–14). This article, prefaced by a short note by Marcel Mauss and edited by him, consists of a lecture Durkheim had delivered at the University of Bordeaux in 1892 at the end of a course on the sociology of the family. This lecture had been delivered from manuscript. But the manuscript as it came down to Mauss was faulty in spots, and he added to the text from the notes he had taken as a student at that lecture. Mauss also appended footnotes to the printed version to explain or expand, on the basis of Durkheim's general views, sentences and ideas which by themselves might appear abstruse.

Mauss confirms in his preface, written in 1921, that there is little publicly available by Durkheim on the sociology of the family as a specific topic and that Durkheim never got around to preparing for publication material that he had worked on relative to it.

I therefore gave up the idea of a separate section on the family in my book on Durkheim, but not the thought of publishing this significant document saved for sociological posterity by Mauss. The translation of this article hence resuscitates a neglected document tucked away for nearly a half-century in a French journal and accordingly lost sight of. The article is an example of Durkheim's very early sociological thinking and shows him to have been involved with ideas that were to mark much of his later work in sociology. The term conjugal family—in recent years so widely stressed as the accurate description of the present form—shows up here as one of long standing with Durkheim, predating current usage.

In 1892, when he delivered this lecture on the conjugal family, Durkheim had not yet published his controversial dissertation at Paris on the division of labor. But the

* George Simpson, *Émile Durkheim* (New York: Thomas Y. Crowell Co., 1963).

emphasis in this lecture on comparative law and jurisprudence as indexes of the state of social relationships at given times in given societies and of the direction in which they were changing foreshadows his extensive use of them in the *Division of Labor in Society*,† both to establish the types of solidarity he found in the evolutionary scheme and to propound a now widely recognized, ingenious criminological theory. Furthermore, this article shows Durkheim to have mapped out the guidelines of his later theory of the evolution of individual rights and personal autonomy, in this case shown by the changes that had taken place in the modern conjugal family as compared with earlier forms of the family. Then, too, we discover that Durkheim's work on suicide was already under way and that his ideas on the relation of marriage and the family to the suicide rate were beginning to crystallize. Surprisingly, even his idea on the significance that occupational or professional groups were destined to have in future social organization shows up here, although generally it has been assumed that this idea grew out of his work on the division of labor and that it was publicized first in his preface to the second edition of that work in 1902. Finally, Durkheim's view of society as the center of morality appears in rudimentary form in this article on the conjugal family.

Three-quarters of a century ago Durkheim's thinking must have been an eye-opener to the students at the University of Bordeaux. Durkheim was giving the first courses in sociology ever given in France, and the sophistication that subject requires and can arouse when learnedly pursued must have stood out boldly against the pretentious bourgeois backdrop of life under the Third French Republic.

The translation presented here is a refinement by me of one first roughed out by Miss Katharine O. Parker at my request through the kind offices of Mr. John T. Hawes of the Thomas Y. Crowell Company and then gone over by Dr. Louis Lister. Material which appears in brackets was inserted by Marcel Mauss as French editor, and the numbered footnotes are also his. Mauss put his initials after each footnote but I have omitted them here.

G. S.

THE CONJUGAL FAMILY

Conclusion of the Course on the Family

This is the seventeenth and last lecture of the course on the family that Durkheim gave in 1892 at Bordeaux. It was delivered to us students on April 2 of that year.

For a long time Durkheim had intended to publish the whole body of his researches on the family. Shortly before the war however, while undertaking the publication of his treatise on ethics, he wavered. He thought of publishing only the substance contained in his course on ethics in the family, which constitutes the second part of his course on ethics. The war intervened. Long before his death Durkheim had definitely given up the project—a project that everyone who had taken this course wanted him to complete. He suggested that we publish only his work on ethics in the family.

Durkheim's treatment of the family could certainly not have been completed definitively without a great deal of work concerned with verification and elucidation. Knowledge of family law, especially family law in primitive societies, has come a long way since 1892.

But, seen again after more than a quarter of a century, so much of the course is still so apt and profound that we consider it

† Émile Durkheim, *De la division du travail social* (Paris, Felix Alcan, 1893) trans. by George Simpson as *Division of Labor in Society* (New York, Macmillan, 1933) and republished by The Free Press in 1948.

our duty to allow the widest possible public to derive benefit from it.

This concluding lecture is quite short, and Durkheim would no doubt have felt called upon to expand it. Considerable data on the history of the family and of marriage in the Middle Ages may be found in *Année sociologique*, beginning with the first issue, under the heading "Family Organization," for which Durkheim was responsible until his death. These data would easily help to support this contention.

MARCEL MAUSS

THE CONJUGAL FAMILY

I use this name for the family established in societies descended from the Germanic peoples—that is, among the most civilized peoples of modern Europe. I shall describe its most essential characteristics as they emerged through a long evolution to become fixed in our Civil Code.

The conjugal family comes about as a contraction of the paternal family.[1] The latter included the father, the mother, and all generations descended from them excepting daughters and their descendants. The conjugal family consists only of the husband, the wife, and minor and unmarried children. Among the members of a group thus constituted there are definite distinctive kinship relationships which exist only among them and within the limits to which paternal authority extends. The father is responsible for supporting the child and for his upbringing until he attains majority. The child in turn is entirely under the father's authority; he controls neither his person nor his wealth both of which are in his father's keeping. He has no civil responsibility; that belongs to his father. But when the child is of age to marry —for the civil majority of twenty-one years still leaves him under his father's tutelage as far as marriage goes—or as soon as the child, at whatever moment, is legitimately married, all these relationships cease. The child henceforth has his own personality, his separate interests, and responsibility for himself. He can, to be sure, continue to live under the father's roof, but his presence there is only a material or purely moral fact; it no longer has any of the legal consequences that it had in the paternal family.[2] Cohabitation, moreover, very often ceases even before the child reaches his civil majority. In any event, once the child is married he generally sets up his own home. To be sure, he maintains ties with his parents; he owes them support in case of illness, and he has, inversely, a right to a certain portion of the family wealth for he cannot [in French law] be totally disinherited. These are the only legal obligations that have survived [from earlier family patterns] and the second seems destined to disappear. Nothing here resembles that state of perpetual dependence that was the basis of the paternal family and of the patriarchal family. We are now face to face with a new family type. Since the only permanent elements in it are the husband and the wife, since all the children sooner or later depart from the [paternal] homestead, I propose to call this new family type the "conjugal family."

The new element manifested in this family type with regard to internal organization is a shattering of the ancient family communism such as never before encoun-

[1] The preceding lecture had dealt with the paternal family. This was the name given by Durkheim to the family institutions of the Germanic peoples, which he sharply distinguished from those of the Roman patriarchal family. The principal difference lay in the absolute and overwhelming concentration of power in Rome through the *patria potestas* exercised by the *paterfamilias*. Characteristic of the paternal family were the rights of the child, of the wife, and especially the rights of relatives on the maternal side.

[2] Collective responsibility, etc.

tered. Up to this time,[3] indeed, communism remained the basis of all familial groupings with the possible exception of the patriarchal family. In this latter type, in effect, the position of ascendancy acquired by the father[4] had cut into the communistic character of the family association. But this character was far from disappearing completely. Paternal power in this case ultimately emanates from a transformation of the old communism; it is communism no longer based on the family itself [living] conjointly but on the person of the father alone. Accordingly, the family group there forms a whole whose units no longer have distinct individuality.[5] But with the conjugal family things are no longer the same. Each of the members of the conjugal family is an individual with his own sphere of action. However, because of his immaturity, the minor sphere of action is subordinated to that of the father. The child may have his own material wealth, although it is controlled by the father until the child is eighteen years of age. Yet this control does not relieve the father of certain obligations toward the child (see article 385, Civil Code). The minor may even possess certain goods that are free of such control—goods that he has acquired through his own labor or that he has received with the proviso that his parents may not make use of them (article 387, Civil Code). Finally, as regards personal relationships, the father's disciplinary rights over the minor are severely limited. All that remains of traditional communism, along with the parents' right of usufruct over the property of the child under sixteen years of age, is the rigidly circumscribed right of the descendant[6] to ancestral property as a consequence of the restrictions on the willing of property.

But what is even newer and more distinctive in this type of family is the ever growing intervention of the state in the internal life of the family. The state has indeed become a factor in family life. The state can intervene and punish the father if he oversteps certain limits. Through its magistrates, the state presides over boards of guardians; it takes the orphan minor under its wing where there is no appointed guardian; it declares and sometimes petitions for injunctions against the adult. A recent law even authorizes the court to abrogate paternal power in certain cases. But one fact, more than any other, shows how great is the transformation the family has undergone in these circumstances. The conjugal family could not have sprung from the patriarchal family [or even from the paternal family or from a mixture of the two types without the intervention of this new factor, the state.][7] Up to now family ties could always be broken, either by the relative. . . .[8] who wished to quit his family or by the father on whom he depended. The first case could occur in the agnate family [and also] in the paternal family.[9] The second [case] could occur only in the patriarchal family. With the conjugal family the ties of kinship have become completely indissoluble. The state in taking them under its protection has

[3] Until this type of family appeared.

[4] Durkheim alludes here to the right of bequeathing property and to the right of sale.

[5] Durkheim had abundantly shown that the patriarchal family, particularly the Roman, involved the concentration in the person of the *paterfamilias* of the rights of the old group of joint agnates.

[6] In French law. But it should not be forgotten that in the first paragraph of this lecture Durkheim set himself the task of explaining particularly the family as seen in the Civil Code of 1892.

[7] I have added these two phrases from old notes taken during this course and in accordance with the context. In the manuscript the sentence appears only in the margin.

[8] Himself (?) Word illegible but of no significance.

[9] Durkheim here alludes to one of his earlier lectures where he contrasted looseness in barbarian laws with expulsion from the patriarchal family that in Greece and Rome broke asunder agnatic ties.

deprived individuals of the right to break them.

Such is the central zone of the modern family.[10] But this central zone is surrounded by secondary zones which complement it. These latter zones are—here as elsewhere[11]—nothing else but earlier family types which have, so to speak, moved down a step. There is first the group formed by ancestors and descendants: grandfather, grandmother, father, mother, brothers and sisters, and the other ancestors from the earlier paternal family now relegated from the first rank to the second. In French law the group thus constituted has preserved a fairly distinct character. Thus where a man dies without descendants, his property is divided among his parents and his brothers and sisters or their descendants. Second, beyond the paternal family one finds the cognate family,[12] that is, the totality of all the collaterals other than those we have just mentioned but even more diminished and weaker than in the paternal family. In the paternal family, collaterals unto the sixth and seventh degree and sometimes even beyond still had very important familial duties and rights. We noted examples of this fact last time.[13] But their role in the family henceforth is practically nil. Scarcely anything remains but a contingent right to inherit, which itself can be nullified through the exercise of testamentary power where there are no descendants or ancestors. For the first time there remains no trace of the clan. (The specificity of the two secondary zones no longer seems to be as distinct as in earlier types.)[14]

* * * * * * *

Now that we are acquainted with the latest family type that has been established, we can take a look at the ground traversed and take stock of the results emerging from this long evolution.

The law of contraction or progressive emergence has been thoroughly verified. Invariably, we have seen emerging from primitive groups increasingly restricted groups which tend to absorb family life completely.[15] Not only is the uniformity

[10] The word "zone" is used by Durkheim to designate fairly close circles of kinship; it forms part of his general nomenclature, made sufficiently clear elsewhere.

[11] Just as the phratry exists alongside the clan, the clan alongside the uterine or masculine or agnatic family, the agnatic family alongside the patriarchal family, etc.

[12] In a preceding lecture, Durkheim, in analyzing the paternal Germanic family, had shown that, for the first time in the history of family institutions, both maternal and paternal descent had been placed on the same footing. The paternal uncle and the maternal uncle, the uterine nephew and the masculine nephew, have the same rights. He said: "That is why I propose to call the collateral family thus constituted the cognate family"; and he quoted: " 'The *Sippe,*' says Heusler, 'is absolutely cognate. Thus kinfolk [translation of the word *Sippe*]. [The French as printed here reads "Ainsi la parentèle (traduction Latine du mot Sippe)," but the Latin really is "parentela" and "parentèle" is the French translation.—G. S.] in the Salic law refers to relatives descended from one or the other of the two sides, *parentes tam de patre quam de matre* (chapter 42) . . . , etc' " (*Institutionen des Deutschen Privatrechts,* II, 172). Cf. *Année sociologique,* VIII, 429.

[13] Durkheim here refers to what he had said to demonstrate the extension of kinship in the uterine line: the facts of penal responsibility in the case of Wergeld (*Salic Law,* chap. lxxxviii) the facts of the repurchase of the widow's right to remarry, by the new husband, from her uterine nephew and, in the absence of any other degrees of relationship, even from the son of the maternal cousin (*Salic Law,* chap. xliv); and other traces of the maternal family properly so-called.

[14] This sentence is in parentheses in the text and may be skipped by those not familiar with Durkheim's nomenclature and with the importance he attached to the study of what he called the secondary zones. In brief, Durkheim means that whereas up to this point there are still, alongside the restricted family, distinct traces of the consanguine family and of the clan. In the modern conjugal family, on the contrary, there are no distinct traces even of the cognate family, which is now thought to be derived from the conjugal relationship, that is, from a single basic couple.

[15] Durkheim's whole theory, and particularly the proofs, cannot be summarized in a note: the steady contraction of the politico-domestic group, the

of this evolution a resultant of what has preceded it but it can be readily seen that it is tied in with the most fundamental conditions of historical development. A study of the patriarchal family has clearly shown us that the family must of necessity contract in proportion to the expansion of the social environment in which each individual is directly immersed.[16] The more limited the social environment, the better it is situated to oppose individual divergences. It follows that the only divergences that can make their appearance are those common to a sufficiently large number of individuals to produce a mass effect and triumph over collective resistance. Under such circumstances only large family groups can disengage themselves from political society. On the other hand, as the social environment expands it makes possible greater play for private differences, and those which are common to a very small number of people accordingly cease to be restrained and can develop and persist. At the same time, moreover, in accordance with a general law already observed operating in biology, differences among individuals increase with the expansion of the environment. Now, if there is one fact that dominates history, it is that of the continuous expansion of the social environment within which each one of us is bound up. The city succeeds the village; to the urban environment with its dependent hinterland of villages succeed nations comprising different cities; to small nations such as the Germanic principalities succeed the huge societies of the present day. Contacts between the different parts of these societies are at the same time getting closer because of the growth and increasing rapidity of communications, etc.[17]

The constitution of the family is modified as its volume contracts. From this point of view, the great change produced is the progressive destruction of familial communism. Communism originally encompasses all kinship relationships; all the relatives live together and own property in common. But as soon as a first dissociation appears in the original amorphous mass, as soon as the secondary zones appear, communism retracts exclusively into the primary or central zone. When the agnate family[18] emerges from the clan, communism ceases to be the basis of the clan; when the patriarchal family detaches itself from the agnate family, communism ceases to be the basis of the agnate family. Communism is finally, though gradually, cut down to the core of the primary circle of kinship. The father in the patriarchal family is liberated from communism because he can dispose of domestic property freely and on his own. Communism is more marked in the paternal family because it is an earlier family type[19]; yet members of the paternal family may possess personal property though they cannot use it or administer it by themselves.

transformation of the amorphous exogamous clan, a vast consanguineous group, to the differentiated clan, to families properly so-called, whether uterine or masculine; then to the joint family of agnates; next to the patriarchal family, paternal and maternal; and next to the conjugal family. The dominant theme of the history of family institutions is, according to Durkheim, the reduction in the number of family members and the concentration of family ties. We may refer to his summary of Grosse, *Formen der Familie* (*Année sociologique*, I, 326 ff.).

[16] Durkheim here alludes to his deduction of the patriarchal family, Roman and Chinese, which he interpreted as a feudal concentration of agnatic grouping under one family head. M. Granet in *Polygynie sororale* (1920), using excellent Chinese texts, has admirably thrown light on this fact.

[17] A conclusion is missing here (and also in my course notes) but evidently it is "the family group may thus grow smaller, to an extreme limit."

[18] Durkheim here means the joint agnatic family (the *joint family* of Sumner Maine, slavic *Zadruga*, etc.).

[19] In a preceding lecture Durkheim had demonstrated that the paternal Germanic family does not necessarily presuppose the joint agnatic family but issued directly from the family characterized by uterine descent and has kept many traces of it.

Finally, in the conjugal family there remain only vestiges of communism; this development is linked with the very same causes as the preceding one. The same impulses that led to the contraction of the family circle are responsible for the progressive individuation of family members. The more extensive the social environment, the less restrictive, we repeat, the development of private differences. Some of these differences are unique to each individual, to each member of the family, and they too become continually more numerous and more significant as the field of social relations widens. Where these individual differences meet feeble resistance, they inevitably develop broadly outside and are sharpened and systematized. Since they belong to the individual personality, that personality necessarily develops as a consequence. Each person takes on more of an individual physiognomy, a personal manner of feeling and thinking. Now, under these conditions communism becomes more and more impossible since it presupposes, on the contrary, identity—fusion of all consciences into a single common, all-embracing conscience. We can therefore be certain that this displacement of communism that characterizes our family law not only is not a passing accident but will, on the contrary, be increasingly accentuated unless, through some unforeseeable and practically inconceivable miracle, the fundamental conditions that have dominated social evolution from the beginning should change.

Does family solidarity emerge weakened or strengthened as a result of these changes? It is very difficult to answer this question. On the one hand, it is stronger, since kinship ties are now indissoluble. Yet, on the other hand, the obligations to which kinship gives rise are less numerous and far-reaching. What is certain is that family solidarity has been transformed. Family solidarity depends on two factors: persons and things. We are attached to our family because we are attached to the people who compose it. But we are attached to it also because we cannot do without material things, and under the regime of familial communism it is the family that possesses these things. With the destruction of communism, things eventually cease to cement family life. Family solidarity becomes completely personal. We are attached to our family only because we are attached to the persons of our father, our mother, our wife, our children. Formerly it was entirely different—the ties that rested on things were more important than those resting on people, the whole organization of the family was intended primarily to keep domestic goods within the family, and all personal considerations were, by comparison, secondary.

That is what tends to become of the family. But if this is so, if things held in common cease to be a factor in domestic life, then the right of succession no longer has any basis. The right of succession has, in fact, become nothing more than family communism persisting under the regime of private property. If communism is retreating, then, disappearing from all the zones of the family, how can this right be maintained? As a matter of fact it has been regressing in a most consistent fashion. Originally it appertains indefeasibly to all relatives, even the most distant collaterals. But soon there appears the right to bequeath property, which cripples the right of succession as far as the secondary zones are concerned. The right of collaterals to the property of the deceased takes effect only if the deceased has put no obstacles in the way, and the power which the individual possesses to set up such obstacles increases continually. Finally, the right to bequeath property penetrates even into the primary zone, into the group formed by parents and their children—the father can totally[20] or partially disinherit his children. There can be no

[20] Here, according to my old course notes, Durkheim indicated that Anglo-Saxon laws already recognized this absolute right to bequeath property.

doubt that this trend is destined to continue. By that I mean not only that the right to bequeath property will become absolute but that the day will come when the individual will no more be permitted to bequeath his property to his descendants, even by means of a will, than he has been permitted [since the French Revolution] to bequeath them his offices and his status. For the transmission of property by will is but the last, and the most tenuous, form of hereditary transmission. Today there are already assets of the highest worth that can no longer be transmitted in any hereditary way. [These are precisely] offices and status.[21] A whole category of workers can no longer today transmit the fruits of their labor to their children; that is, those workers who earn honor and fame but not wealth. It is certain that this rule will be generalized further and that hereditary transmission will become increasingly delimited.

From yet another point of view, this change is becoming more and more essential. So long as wealth is transmitted by heredity, there are rich and poor by birth. The moral conditions of our social life are such that societies cannot be maintained unless *extrinsic* inequalities among individuals are evened out. This statement must not be taken to mean that all men ought to become equal. On the contrary, intrinsic inequality will become more and more pronounced. But social inequalities must come to reflect only differences in personal worth without that worth's being exaggerated or debased by some external factor. Now, hereditary wealth is one of these extraneous factors. It renders to some advantages not derivative from personal worth that nevertheless bestow upon them superiority over others. This injustice, which strikes us as increasingly intolerable, is becoming increasingly irreconcilable with the conditions for existence of our present-day societies. Everything thus serves to demonstrate that the right of succession, even through the vehicle of a will, is destined to disappear step by step.

Yet, however necessary this transformation, it will be far from easy. The rule of hereditary transmission of goods undoubtedly has its roots in the old familial communism which is disappearing. But in living practice we have become so accustomed to this rule, it is so tightly bound up with all of our organized activities, that if it were abolished without replacement social life would itself dry up at its living source. In fact we are so geared and accustomed to it as to make the prospect of transmitting the products of our labors through heredity the mainspring of our activities. If we pursued only personal ends, we would be much less strongly motivated to work, since our work has meaning only insofar as it serves something beyond ourselves. The individual is not an adequate end for himself. When he takes himself as his end he falls into a state of moral misery which leads him to suicide.[22] Our attachment to work lies in its capacity to enrich the domestic patrimony, to add to the well-being of our children. If this prospect were taken from us, a powerful moral stimulant would be removed in one fell swoop. Hence the problem is not as simple as it might, at first glance, seem. In order to realize the ideal we have just sketched, we must gradually substitute something else for the mainspring that threatens to fail us. Something other than personal and domestic interest

[21] According to my notes, Durkheim at this point in this lecture added important considerations on the outmoded character of literary, industrial, and commercial property (copyrights and patents) which fall in the public domain and which the proprietor cannot transmit after a certain lapse of time. He returns to this subject at another point in this lecture.

[22] By this time Durkheim had already given his first course on suicide. Here can be recognized ideas he published in 1896 in his book on the subject.

must stimulate us to work. But general social interest is too far removed from us, too vaguely perceived, and too impersonal to become this effective motive. We must, then, be tied to some other group outside the family, more circumscribed than political society, nearer to us, and touching us more closely. Those very rights that the family is itself no longer capable of exercising must accordingly be transferred to this other group.

What group can this be? Could it be matrimonial society? We have observed the continuous, steady growth of this society, its consolidation and its ever greater coherence. The importance it assumes in the conjugal family marks the apogee of this development. Indeed, not only does marriage become well-nigh indissoluble in this family type, not only does monogamy there become nearly perfect, but marriage itself now presents two new characteristics which evince the strength it has amassed.

First, it has completely left off being a personal contract and has become a public record. Marriage is contracted under the auspices of a [magistrate] of the state; not only does the ceremony have this public character but indeed the marriage itself is not valid unless the public formalities demanded are precisely fulfilled. Now, as we know, no judicial act assumes such solemn forms unless it is laden with vast significance.

Second, in addition to these external conditions for marriage, the organization of matrimonial relationships presents a special characteristic without parallel in the history of the family up to now. This characteristic is the presence of the rule of common property between spouses, a community of property which may refer to all goods or be restricted to acquisitions. Common property is the general rule in matrimonial society. Though it can be evaded, it exists in full right if there are no contravening conventions. Thus, while communism was departing from domestic society, it was making its appearance in matrimonial society.[23] Could not matrimonial society be destined to replace domestic society in the function spoken of above, and could not conjugal love be the mainspring capable of producing the same results as love of family?

Not at all. Conjugal society by itself is too ephemeral, its vistas are too restricted for such results. To become attached to our work we must be cognizant that it will survive us, that something from it will remain after us, that it will be of service to those whom we love even after we have departed. We possess this feeling as a matter of course when we work for our family, since it continues to exist after us. But conjugal society, quite to the contrary, is dissolved by death in each generation. Spouses do not long survive each other. Consequently, one of them cannot be a strong enough reason to make the other sacrifice momentary pleasures. That is why marriage does not have the same counteractive influence against suicide as the family has.[24]

Only one group is, accordingly, to be found that is close enough to the individual to hold him tightly and lasting enough to allow him a vast perspective. This group is the occupational or professional group. Only this group, in my view, is able to perform the economic and moral functions which the family has become increasingly incapable of performing. In order to work out of our present state of crisis, the termination of the rule of hereditary transmission is not enough. Men must gradually become attached to their occupational or professional life. Strong groups relative thereto must be developed. In the hearts of men, professional duty

[23] Durkheim here mentioned to us some rights of the surviving partner: the reservation of usufruct in French law and the right of succession *ab intestat* in Anglo-Saxon law.

[24] See Durkheim's *Suicide*.

must take over the place formerly occupied by domestic duty. This moral level has already been attained by that elite we have mentioned, which demonstrates that this transformation is not impracticable.[25] (This change, moreover, will not take place in one fell swoop); [for a long time] a great many traces of traditional usage will remain. Parents will always be impelled to work in order to feed and rear their families. But this motive will not alone suffice)[26] [to make the family disperse and disappear. The professional group, on the contrary, is by its very nature perpetual].

A few words on the secondary effect upon marriage. Under the paternal family, free, unregulated union coexisted alongside of marriage, but in the conjugal family such unregulated union is almost totally repressed. [It no longer gives rise to any rule of law.] The more tightly the family is organized the more does marriage tend to be the exclusive basis of kinship.

[The] causes [for this fact are as follows:] Marriage establishes the family [and at the same time] springs from it. Any sexual union not contracted under matrimonial regulation is accordingly subversive of duty and of domestic bonds. Where, moreover, the state itself is a party to family life, free union undermines public order. From another point of view, this result is inevitable. The members of every moral society have obligations toward one another; and, when they attain a certain importance, these obligations assume a juridical aspect. Free, unregulated union is a conjugal society without such obligations. Hence it is an immoral society. And that is why children reared in such environments show so many moral defects—they have not been exposed to a moral environment. A child cannot have a moral upbringing unless he lives in a society whose every member feels his obligations toward every other member. For outside such a society there is no morality. Hence [to the extent that the legislator and ethics concern themselves with this problem] the tendency is not to make a free union of every marriage but to transform every union, even a free union, into a marriage, however imperfect.

Such are the general conclusions to be drawn from this course of lectures. The family has progressed through concentration and personalization. The family undergoes steady contraction; at the same time, relations in it assume an exclusively personal character in consequence of the progressive obliteration of family communism. Whereas the family loses ground, marriage contrariwise becomes stronger.

ÉMILE DURKHEIM

[25] The manuscript contains no trace of the development given by Durkheim to this idea. Thanks to my notes, I can reconstruct it approximately as follows: ["Civil servants, soldiers, scholars who render to the state a lifetime of poorly compensated labor—can they look forward to transmitting property? Those authors, those artists, those scholars, those engineers, those inventors whose work so soon falls into the public domain, whose literary, artistic, and industrial ownership is so highly ephemeral—can they transmit to their children the material fruits of their work? Why do they work? Is not their work just as effective and even more effective than that of others? Thus one may work without the sole objective of accumulating an inheritance for one's children"].

[26] Durkheim himself added the parentheses in the manuscript. In any event he had uttered these ideas to us and I was able to complete the last of them. He undoubtedly intended to insert them in a later version. [This footnote as printed in the article in French ends with a comma and without Mauss's initials. G. S.]

Political Structure, Economic Development, and National Social Security Programs[1]

Phillips Cutright

ABSTRACT

A scale of national social security programs is developed and related to economic development, literacy, urbanization, and a political-representativeness index. The degree of social security coverage of a nation's population is most powerfully correlated with its level of economic development, but when economic development is controlled for, the more representative governments introduce programs earlier than the less representative governments. A separate analysis of the relationship between changes in political representativeness and changes in social security legislation found that new social security programs were more likely to follow positive than negative political changes.

Comparative sociological studies of political systems in modern nations have, in recent years, experienced impressive theoretical development. Attention has been focused on the "functional prerequisites" for political democracies, the structural conditions generating political stability or instability in "democratic" states, and the value structures necessary for a democratic order.[2] A number of excellent studies of political stability in non-democratic nations exist, but when more than one nation is studied, the comparison is usually limited to somewhat similar underdeveloped nations. When comparisons between democratic and non-democratic nations are made, the number of observations (nations) is severely limited by the absence of scales and indexes relevant to the analytical variables guiding the analysis.[3]

Elsewhere the author has developed and tested a scale of the complexity of national political organization.[4] The development of similar scales that would increase the number of nations in comparative studies and serve to aid in the selection of a few nations to fit the requirements of special studies is clearly a desirable goal. One aspect of this article is the development of a scale that can be applied to nations throughout the world. It measures the development of national programs to provide populations with insurance against severe loss of income under stated conditions, that is, the general level of social

[1] My thanks to E. Palmore for criticism and helpful suggestions. The opinions expressed are those of the author and do not reflect the opinion of the Social Security Administration.

[2] William Kornhauser (*The Politics of Mass Society* [Glencoe, Illinois: Free Press, 1959]), in particular, gives a detailed discussion of the concept of "representativeness" and democratic pluralism. Robert R. Alford (*Party and Society: The Anglo-American Democracies* [Chicago: Rand McNally & Co., 1963]) examines longitudinal data bearing on the relations between political parties and the social structure of five democracies. S. M. Lipset ("Democracy and the Social System," in Harry Eckstein [ed.], *Internal War: Problems and Approaches* [New York: Free Press of Glencoe, 1964]) develops a framework for analyzing the value patterns that support democratic government.

[3] See Dick Simpson, "The Congruence of Political, Social and Economic Aspects of Development," *International Development Review*, VI (June, 1964), 21–25.

[4] P. Cutright, "National Political Development: Measurement and Analysis," *American Sociological Review*, XXVIII (April, 1963), 253–64. That study also argues the case for scales rather than crude qualitative categories in international studies. See n. 12 of the present article for a description of the revised political representativeness index used in this study.

security development in the nations of the world. Because such national programs are an output of government activity, the analytical value would seem to go beyond the concrete phenomena directly measured. The scale can be used as a yardstick against which governments with varying characteristics can be compared.[5] It may allow one to measure, for example, one aspect of what democratic governments *do* that may distinguish them from nondemocratic governments.[6]

In this paper we develop an index of the general level of social security in different nations that is a direct consequence of one kind of government activity (i.e., legislation or government order). We then analyze the relationship of this index to an index measuring the political representativeness of nations and to other indicators of economic and social development. The general purpose of the analysis is to assess the importance of representativeness in governmental organization to the social security and welfare of national populations. Our working hypothesis is that governments in nations whose political structures tend to allow for greater accessibility to the people of the governing elite will act to provide greater social security for their populations than that provided by governments whose rulers are less accessible to the demands of the population. The theoretical contribution of this analysis is toward an application of the construct of representativeness (or accessibility) to actual government activity.

MEASURING NATIONAL SOCIAL SECURITY

Perhaps one of the more striking developments of the twentieth century has been the effort by national governments to protect that portion of the population that is, for one reason or another, not in the employed labor force. National social-insurance programs, initiated in Europe near the end of the nineteenth century, first dealt with the problem of income loss resulting from industrial work injury, a problem that was greatly intensified by the expansion of industrial activity. As urbanization and industrialization (and their social and political correlates) continued, social-insurance programs covering other types of risks—sickness, old age, unemployment—began to appear.

Although there exists some detailed information on the extent of coverage or the level of benefits provided by various national social-insurance schemes, these data are available for only a few nations. In a study that concentrated on the economic correlates of certain social-insurance programs, Henry Aaron was able to locate adequate detailed data on twenty-two nations—all economically well developed

[5] An inventory of municipal services in Bristol, England, and Seattle, Washington, revealed remarkable similarities "even in two cities where the govermnental power is based on different philosophies. In Bristol the Labor (socialist) party holds political dominance with its leaders in almost all key legislative positions. In Seattle, a conservative local government rules (Republican). Yet each city has almost the same amount of municipal ownership and control" (William H. Form and Delbert C. Miller, *Industry, Labor and Community* [New York: Harper & Bros., 1960], p. 501).

[6] There has been relatively little systematic work on the consequences for national populations of living under more or less representative governments. Studies of this sort often compare only totalitarian and democratic governments, highlighting the impact of government activity on the expression of individual freedoms.

Unfortunately, an inventory of the activities of national governments, or even a conceptual scheme to aid in their classification, is not at hand. Comparative studies of the outputs of national governments are limited by the lack of scales of those activities, and relatively little attention has been given to classification of the activities. Available indicators of government activities are not, however, being fully exploited. Thus, studies using measures of education, health, or demographic conditions do not examine these phenomena as though they were related to government activity. For example, we are more likely to see a certain level of education as a requisite for democratic government than to view government activity as vital for the development of national education levels.

—and subjected these data to an intensive multiple-regression dummy variable analysis.[7] These data on program coverage and benefit levels showed that the most powerful explanatory variables were (1) years of experience with the program (number of decades since its initiation) and (2) various indicators of national economic development. If one knows how long a nation has had certain programs and what its level of economic development is, then one can assess how the nation will rank in coverage and benefit levels relative to the remaining twenty-one nations in the sample. (The homogeneous economic and political character of the nations in Aaron's study should be noted. Correlations are often low within homogeneous groups; the present instance is an exception to this rule.) Aaron suggests that the relationships among his variables may not be the same in less-developed nations; but the question is not whether the regression is the same but whether Aaron's detailed study can be applied to a slightly different type of analysis that will allow us to get around the problem of lack of detailed program data in many nations. If number of years' experience with a program is highly correlated with the total expenditures, benefit levels, and coverage of a program, then the number of years can be used as an indicator of the level of program development.

However, the lack of detailed and comparable data on social security programs is not the only stumbling block in the way of international comparisons. We have also to establish that what we call social security programs are conceptually related. The fact that custom and administrative usage have grouped different types of programs under a common label (social security) is not proof that these programs are interrelated and form a continuum along which nations may be placed in order from high to low social security development. The first task is to offer some evidence that we can talk about the social security development of nations because a definite pattern of program occurrence or non-occurrence exists among the nations of the world.

PATTERNS OF SOCIAL SECURITY PROGRAM DEVELOPMENT

There are five major types of social security programs.[8] Of the seventy-six nations outside Africa[9] that had achieved independent political status by 1960, seventy-one had begun work-injury programs, fifty-eight had sickness and/or maternity programs, fifty-six had programs grouped under old-age, invalidism, and death, forty had some type of family allowance plan, and twenty-seven had unemployment-insurance programs. This frequency distribution does not, in itself, tell us that a nation with an unemployment program is necessarily more or less advanced toward a social-insurance goal than is a nation with only a work-injury program. There are several ways to approach this question, but perhaps the simplest is with the Guttman scale.[10] If

[7] Henry Aaron, "Social Security in an Expanding Economy" (unpublished doctoral dissertation, Harvard University, 1963).

[8] All data pertaining to social security programs are taken from U.S. Department of Health, Education, and Welfare, Social Security Administration, *Social Security Programs throughout the World, 1961* (Washington, D.C.: Government Printing Office, 1961). Discussion of the characteristics of each type of program can be found in that document.

[9] This part of the analysis is based on research initially focused on the correlates of political development, which for statistical reasons omitted all African nations in order to avoid spuriously high correlations. The seventy-six nations are listed in Appendix A.

[10] S. A. Stouffer, *et al.*, *Measurement and Prediction* (Princeton, N.J.: Princeton University Press, 1950), pp. 60–90. For a more recent explication of Guttman scaling see Allen L. Edwards, *Techniques of Attitude Scale Construction* (New York: Appleton-Century-Crofts, 1957), pp. 172–98.

several discrete items, in this case the five types of social-insurance and benefit programs, form a Guttman scale, we can say that the scale is measuring an underlying dimension along which each of the items may be placed in a known order, and that a given combination of items (i.e., social-insurance programs) represents a higher place along a continuum of social-insurance development than some alternative combination.

The five major types of social security programs do form a Guttman scale. A nation can have between zero and five programs, and we have six possible perfect-scale types with varying combinations of programs. Twenty-two nations in our sample are in the first perfect-scale type, having all five programs, while the second type contains thirteen nations lacking only unemployment insurance. It is interesting that seven of these thirteen nations were Soviet Russia and its satellites. A third perfect-scale type contains the twelve nations that had neither unemployment nor family allowances, but did have the three other programs. The five nations in the fourth scale type lack, in addition to unemployment and family-allowances programs, a program to provide for the aged, invalidism, and/or death. They have both work-injury and sickness programs. In the fifth scale type are six nations with work-injury programs only, and the final scale type contains five nations with no programs at all. Thus sixty-three of the seventy-six countries are in perfect-scale types. It is worth noting that, had we included African nations, the sixth scale type would have had many more nations, with a resulting increase in the coefficient of reproducibility (CR) to about .98. However, even without this group the CR is .966, considerably above the usually acceptable level of .90.

Because the items do form a Guttman scale, these scale types may be used to rank order the nations of the world according to the extent to which they have developed a social-insurance program. The scale does not necessarily tell us whether a nation in scale type 1 has a better or more comprehensive old-age program than a nation in scale type 2, but it does indicate that the general social security coverage of the population outside the employed labor force is better in scale type 1 than in scale type 2, 3, and so on.

A CUMULATIVE MEASURE OF THE YEARS OF SOCIAL-INSURANCE PROGRAM EXPERIENCE

Because the programs form a Guttman scale, it is possible to apply a measure that distinguishes more than six levels of social security development. The measure is, simply, the years of experience with social-insurance programs for each of the seventy-six nations for the period 1934–60. This statistic is similar to that used by Aaron for well-developed nations, and from the above analysis it appears applicable to less-developed nations as well.

An index of a nation's social insurance program experience (SIPE) can be computed by totaling the number of years from 1934 through 1960 that the nation had a given type of program in operation. For each of the five programs, a score of from 0 to 27 is possible. A score of 27 on each program would yield a maximum SIPE score of 135.[11]

The following analysis concentrates on the relationship of SIPE scores to various aspects of national political and economic

[11] SIPE scores for each of the seventy-six nations are shown in Appendix A. Data on political structure are available for as early as 1928, and that time is used in analyzing the introduction of new social-insurance programs later in this paper. The base year for computing the years of program experience is 1934 rather than 1928, however. This is the product of error, not design. However, if 1928 had been used, there would be little difference beyond adding five points to the range of the scale; the relationships with other variables would remain the same.

life. The final section focuses on a social-insurance completion index and relates scores on this index to political and economic levels and changes from 1928 to 1960.

SIPE scores are one possible index of the responsiveness of governments to the needs of the governed. A nation that lacks work-injury or old age insurance programs may or may not have the economic base capable of supporting such programs, but the extent to which governments initiate and improve insurance programs may reflect much more than the operation of an automatic and economically triggered mechanism.

Nevertheless, it is also the case that the ability of a government to begin a program is closely related to the nation's level of social and economic development. The SIPE scores of the seventy-six nations have been correlated with 1960 indexes of energy consumption, urbanization, literacy, and political representativeness (PRI),[12] and the full matrix of product-moment correlations is shown in Table 1.

The highest correlation is between energy consumption and SIPE. This correlation of .90 accounts by itself for 81 per cent of the variation around the mean SIPE score. It is much higher than the correlation of energy consumption with PRI, literacy, or urbanization. Further, it is considerably higher than the zero-order relationship between literacy and SIPE. This indicates that the level of economic development has a powerful role in determining the level of social-insurance development, and that we must control for level of economic development as measured by energy consumption as well as for level of political development in any analysis of the amount of change in social-insurance programs from the 1930's through the early 1960's.

TABLE 1

ZERO-ORDER CORRELATION MATRIX OF ENERGY CONSUMPTION, URBANIZATION, LITERACY, POLITICAL REPRESENTATIVENESS, AND YEARS OF SOCIAL-INSURANCE PROGRAM EXPERIENCE, 1960*

	Urbanization	Literacy	PRI	SIPE
Energy consumption	72	81	61	90
Urbanization	....	64	58	58
Literacy	....	....	76	83
PRI	....	....	....	74

* All variables except SIPE have been *T*-scored; *N* = 76.

Source Data for energy consumption, urbanization, and literacy are taken from the U N. *Demographic Yearbook* and the U N. *Statistical Yearbook* The primary source for political data is the *Political Handbook of the World: Parliaments, Parties and Press* (New York: Harper & Bros. [for the Council on Foreign Relations], annual publication 1928-62).

The high correlations between social security development and energy consumption or political representativeness do not mean, however, that urbanization and the literacy level of the population are not also important correlates of social security development. Quite the opposite is true. In general, nations with high levels of SIPE also have high literacy rates and tend to be highly urbanized. Nations with low levels of urbanization or literacy have less-developed social security programs as well as lower levels of energy consumption and political representativeness.

Although this analysis centers on the

[12] See Appendix B.

relationship of economic development and PRI, it should not be assumed that a change in one or both of these variables alone would be sufficient (although it might be necessary) to produce changes in social security. Changes in the levels of literacy and urbanization of the population usually occur concomitantly with changes in PRI or energy consumption. It would appear that the probability of an increase in the level of social-insurance development is greatest when all four variables are rising. This view is compatible with the proposition that changes in major institutional areas of the society do not proceed far without reacting on each other as well as on lesser aspects of life. Institutions are interdependent. The matrix of correlations is evidence in support of this hypothesis—at the level of national social systems.

In the following section we test the hypothesis that the levels of economic development and PRI will have independent and joint effects on SIPE. Economic development was selected because of its high correlation with SIPE and political development because it is central to analysis.

ANALYSIS OF NATIONAL DIFFERENCES IN YEARS OF SOCIAL-INSURANCE PROGRAM EXPERIENCE

Table 2 presents the mean SIPE scores for nations at five levels of economic development—as measured by energy consumption—and four levels of political development in 1930. The mean social-insurance experience of these nations and the number of nations is shown.

In the upper left-hand cell is a single nation with 99 cumulative years of program experience and immediately below it are three nations with a mean of 25 years' experience. (The reader may prefer to collapse cells having only one case with adjacent cells before comparing individual cells with column means.) The mean SIPE scores associated with each level of economic development are in the "row mean" column. The twenty-one nations with the highest level of economic development had an average of 114 years of cumulative social-insurance program experience from 1934 through 1960. Nations in the second highest group had an average of eighty-three years' experience while the ten nations at the lowest level had an average of only

TABLE 2

MEAN YEARS OF SOCIAL-INSURANCE PROGRAM EXPERIENCE BY MEAN 1930–60 ENERGY CONSUMPTION AND 1930 LEVEL OF POLITICAL REPRESENTATIVENESS*

Mean Energy Consumption 1930–60	1930 PRI†				
	Dependent Nations	Below Mean	Above Mean	Highest Nations	Row Mean
I	99 (1)	101 (2)	114 (4)	116 (14)	114 (21)
II	25 (3)	93 (4)	100 (5)	102 (3)	83 (15)
III	36 (1)	88 (4)	62 (5)	54 (2)	67 (12)
IV	30 (8)	44 (9)	54 (1)	....	39 (18)
V	18 (5)	7 (4)	63 (1)	....	18 (10)
Column mean	30 (18)	59 (23)	86 (16)	107 (19)	70

* Nations are placed into energy consumption levels according to their *T*-score. Groups I and II are above the mean of 50; Groups III, IV, and V are below the mean. Groups I and V contain nations that were more than 1 standard deviation from the mean.

† Number in parentheses = *N*.

eighteen years' experience. The same pattern of decreasing length of program experience is found within each column. We may conclude that the level of economic development is related to SIPE; statistical control for political development does not remove the positive association between the two.

The mean SIPE scores associated with each level of 1930 PRI are in the bottom row of Table 2. The scores show a steady gain with increasing PRI levels. The eighteen nations with dependent political status in 1930 have a mean score of thirty years compared to 107 years for the nineteen nations with the highest PRI scores. This pattern of larger SIPE scores with higher PRI levels holds at the first, second, and fourth levels of economic development. The third level does not fit the general pattern—this "deviant" row may explain why the correlation between PRI and SIPE was "only" .74—and the association in the lowest economic level is also of dubious strength.

Controlling first for one and then for the other variable, then, we see that each is related to the SIPE scores. We should note that the ten nations at the lowest level of economic development have considerably lower SIPE scores than the nations at the next highest level of development. For these nations, it is level of economic development rather than level of political development that determines the level of social-insurance program experience. In terms of causal sequence, it appears that before positive change in political structure can bring about positive change in social-insurance program development, a nation must have experienced some economic growth.

Table 2 supports our working hypothesis. In general, governments in nations with more representative political structures have provided greater social security coverage to their populations. Among self-governing nations there is a nearly uniform increase in government social security activities from one level of representativeness to the next. However, nations with the highest PRI scores—those we would normally call "democratic"—do not really differ qualitatively from those nations of parallel economic development that are in the next lowest PRI group. This finding will be discussed at the conclusion of this paper.

In the next section the idea that changes in political structure are associated with changes in years of social-insurance experience will be tested.

CHANGES IN YEARS OF SOCIAL-INSURANCE PROGRAM EXPERIENCE AND CHANGES IN PRI

Table 3 shows the relationship between changes in political representativeness and changes in SIPE. Since the amount of change in PRI is strongly associated with initial level of PRI (as seen in the disproportionately high gains made by the initially lowest PRI group), it is important to control for 1930 PRI level; level of economic development is not controlled. Nations are ranked in each column according to the size of their gain or loss in political representativeness between 1930 and 1960. Reading down the columns, a near-perfect correlation can be seen between changes in PRI and changes in SIPE. Even within the initially highest PRI group, the twelve nations whose political structure remained perfectly stable throughout the period had an average gain of *121* years of experience; the three that ended at the same level as they began but experienced instability in between (Japan, Uruguay, and Costa Rica) had an average increase of eighty-one; and the two that declined in political representativeness (Canada and Colombia) had a gain of only seventy-six years.

We might note that SIPE gains also are

strongly associated with initial PRI level. The largest gains in social insurance are found in nations that began the time period with a maximum political-representativeness score. Reading across the rows in Table 3, within each PRI-change interval there is a positive association (slightly reversed in only one case) between 1930 PRI level and SIPE gains.

A NON-CUMULATIVE MEASURE OF SOCIAL-INSURANCE DEVELOPMENT

An alternative measure of social-insurance development in a nation is, for some purposes, more satisfactory. Instead of using a cumulative measure of years of program experience, we can examine the political situation surrounding major changes in a nation's social-insurance programs (i.e., each time a new program is launched). An index can then be developed to measure the extent to which a nation is moving toward social-insurance program completion (SIPC). Completion is used here to mean that a nation has begun to tackle the needs associated with the five basic types of social security programs, and not to refer to the extent of coverage of the population by any or all programs. Some examples of how the SIPC index is constructed follow.

If a nation had three programs in 1928, it can introduce only two additional programs. Assume that such a country experiences a positive political change in 1932 and in 1934 adds one program. Between 1932 and 1934 no political changes occur. A score of 50 (i.e., 50 per cent of the total change possible in social-insurance program coverage) is awarded to that specific 1932 political change. If, however, the nation experiences a positive change in 1932 and a negative change in 1933 and adds a social-insurance program in 1934, the positive 1932 political change receives a score of zero, and the negative 1933 change a score of 50. The social-insurance-program completion index is, therefore, a measure of the amount of social-insurance change associated with each change in PRI.[18] The

TABLE 3

MEAN CHANGE IN SOCIAL-INSURANCE PROGRAM EXPERIENCE, 1934–60, AND SIZE OF CHANGE IN POLITICAL REPRESENTATIVENESS INDEX, 1930–60*

Ranked PRI Change Interval	1930 PRI											
	Dependent ($N=18$)			Below Mean† ($N=20$)			Above Mean† ($N=15$)			Highest† ($N=17$)		
	PRI Gain	N	SIPE Gain	PRI Gain	N	SIPE Gain	PRI Gain	N	SIPE Gain	PRI Gain	N	SIPE Gain
Largest	23	3	63	13	4	97	7	6	114	0	12	121
Second	16	6	31	5	7	56	3	3	70	0	3‡	81
Third	11	4	22	3	3	20	−2	3	66	−8	2	76
Fourth	6	5	14	0	6	49	−5	3	65	§	§	§

* Controlled for PRI.

† Soviet satellites (Albania, Bulgaria, Hungary, Poland, Romania, and Czechoslovakia) are omitted from this table as our scoring of PRI changes excluded those caused by external domination. They are included in all other tables.

‡ These three nations began and ended the period with the highest PRI scores, but were unstable during the period.

§ No cases.

[18] One feature of the SIPC index that should be considered in future work is the correlation between economic and political development and the size of the score awarded to a single program

mean SIPC change per nation is calculated in the following manner. For positive and negative PRI changes (computed separately), if a nation has more than one change, the sum of the SIPC index scores associated with these changes is divided by the number of changes. Next, the average SIPC scores for each nation are summed and divided by the number of nations to get the mean SIPC change per nation. For nations with no PRI changes, the mean SIPC change per nation is merely the sum of the nations' SIPC scores divided by the number of nations.

It was suggested earlier that social-insurance programs may represent a measure of the responsiveness of government to the social needs of the population. If so, we would expect not only that social-insurance development would be associated with a *high level* of political representativeness but also that a *positive change* in political representativeness would be *followed* by an increase in social-insurance program coverage. The gain in SIPC index should be larger in association with positive political changes than with negative political changes.

Table 4 presents the results of an analysis of SIPC changes associated with political changes of both types. In the time covered, a total of ninety-eight positive political changes and an average increase in SIPC of twenty-two are found. In the same period, there were seventy negative changes with an increase in SIPC of eleven per negative change; a net gain of eleven points is associated with each positive change.

A slightly different statistic which allows us to consider SIPC changes in nations experiencing *no* political change between 1928 and 1960 is the mean SIPC change per nation. Table 4 shows that fifty-six nations experienced a positive political change, forty-one had negative changes, and sixteen experienced no change. A net advantage of nearly twenty points per nation goes to countries with positive rather than negative changes, but a still larger SIPC increase of eighty-seven is found among nations that had no political change at all. These stable nations are nearly all at the highest level of economic and political development, a condition favorable to maximum SIPC changes.

TABLE 4

SIPC INDEX GAINS ASSOCIATED WITH POSITIVE, NEGATIVE, AND NO CHANGES IN PRI

Direction of PRI Change	Mean SIPC Change* per PRI Change	Mean SIPC Change per Nation
Positive	22 (98)	38 (56)
Negative	11 (70)	20 (41)
No change	Not applicable	87 (16)

* Numbers in parentheses are the number of PRI changes and the number of nations.

Table 5 controls for the level of economic development and shows the amount of social-insurance program-completion change associated in any economic level with either positive or negative political changes or with stable political systems.

At the highest level of energy consumption there were five nations that experienced positive political changes with an average social-insurance index increase of eighty, while seven nations that experienced

change. Since nations at high development levels will also be more likely to have had several programs by 1928, they will also receive "extra" credit when they introduce their next program. Whether it is more difficult for a nation to launch its first than its last program is not the issue here. Comparison of the results using this index as opposed to another is a matter for investigation rather than for debate. Although it might be expected that larger scores would be associated with the economically and politically more-developed nations, this does not justify assuming that high scores should be associated with positive rather than with negative political changes. Controls for economic-development levels will also be introduced to reduce the spurious association between large scores and high economic-development scores.

negative changes had an increase of only seventeen per nation. The thirteen nations with completely stable political structures had an increase of ninety-two. At the second level of energy consumption there is no difference in the amount of social-insurance program-coverage gains between countries with positive or negative change, but the two nations (Finland and Italy) with stable PRI experience have a gain of seventy-five. The third level of economic development again shows the expected pattern with a score of forty-seven associated with positive political change compared to a score of twenty-three associated with negative change. In like fashion, the fourth energy-consumption level reveals an increase of thirty-nine associated with positive change and only eleven with negative change, while the single nation with stable government (Saudi Arabia) had an increase of forty. At the lowest level of development, the ten nations with positive increases had an average of only seven in social-insurance program completion. There were very few cases of negative change and no cases of stable governments in this category. Part of the reason for the small number of negative changes is that nations at this level of economic development have minimal PRI scores to begin with and thus have little room to decrease.

TABLE 5

SIPC INDEX GAINS PER NATION ASSOCIATED WITH POSITIVE, NEGATIVE, AND NO CHANGES IN PRI*

Energy Consumption Level and Type of Political Change	Mean SIPC Index Change per Nation Following Each PRI Change			No. of Nations	Net No. of Nations
Level I:					
Positive	80	....	....	5	8
Negative	....	17	....	7	
No PRI change	....	....	92	13	13
Level II:					
Positive	35	....	....	13	13
Negative	....	35	....	9	
No PRI change	....	....	75	2	2
Level III:†					
Positive	47	....	....	10	11
Negative	....	23	....	8	
Level IV:					
Positive	39	....	....	18	18
Negative	....	11	....	17‡	
No PRI change	....	....	40	1	1
Level V:†					
Positive	7	....	....	10	10
Negative	....	....	....	(4)§	

* Level of energy consumption controlled.

† Levels III and V had no nations without at least one PRI change between 1928 and 1960.

‡ Includes four cases from lowest level.

§ Combined with negative group in Level IV.

If we look down the columns in Table 5, we see some irregularities in the pattern associated with positive and negative PRI changes—there is not a perfectly steady decline in the size of the score from one energy-consumption level to the next.[14]

[14] An alternative method of computing SIPC change per political change rather than per nation provides a somewhat more consistent table when broken down in the same way as Table 5. At each level of energy consumption the average SIPC index change following each PRI positive

Regardless of the method used, the conclusion seems clear enough: Nations at very high levels of economic development are able to take advantage of stable government or positive political change, and they are likely to move toward completing the normal pattern of social-insurance programs. Social security growth is less likely to follow a negative PRI change than a positive change at *any* level of economic development. Again it is seen that nations with the lowest level of economic development are not introducing social insurance programs even when they have a positive PRI change. On the one hand, this reinforces the earlier conclusion that for these nations a rise in the level of the economy must precede the introduction of social-insurance programs. On the other hand, a positive change in political representativeness will tend to induce economically more-developed nations to introduce new social security programs.

DISCUSSION

An index (SIPE) measuring the general level of social security protection legislated or otherwise directed by the national government was developed. The SIPE index was closely related (.90) to the level of economic development, literacy (.83), and to a lesser extent (.58) urbanization. The .74 correlation of the SIPE index with the Political Representativeness Index (PRI) was found, after further analysis, to be related to variation in the SIPE index when the level of economic development was controlled.

The effect of different levels of political representativeness on the development of national social security programs varied with the level of economic development enjoyed by the nation. In nations with very low economic development, the push for social security development has, in most cases, yet to begin, despite the presence in this group of several different levels of political representativeness. This finding was interpreted in terms of the necessity of certain technological and bureaucratic prerequisites for successful introduction of social-insurance schemes. However, nations that have this capability (Levels I through IV) do not always exercise it. Within the mid-range of economic development (III and IV), the level of PRI was not powerfully related to SIPE. Nations in the upper two economic-development strata not only had high SIPE scores but within each stratum SIPE was positively related to PRI. In general, the political condition that was most strongly related to low levels of social security development was colonial or quasicolonial status. With few exceptions (especially that of Iceland) little was done by occupying powers to institute social security programs. The difference between being politically dependent and being politically self-governing appears (Table 2) critical to the early development of social security programs. Once political independence is achieved, the degree to which the national government becomes more and more representative is also related to how rapidly the government acts to introduce national social security programs. This is most clearly demonstrated at the higher levels of economic development. In a separate analysis that controlled for the 1930 PRI level and ranked the 1930–60 PRI change against 1934–60 gains in SIPE, the size of PRI changes was positively related to SIPE gains.

A second index (SIPC) of government activity in the social security field was devised. A score was computed that measured the degree to which any new program instituted by a nation moved that nation closer to complete program coverage. Analysis of these scores revealed that social-insurance completion followed more upon

change is larger than for negative PRI changes. The pattern of decline in SIPC change associated with decreasing energy consumption is also consistent for both positive and negative PRI change.

positive political changes than upon negative political changes, whatever the level of economic development. Also, the few nations that enjoyed stable political structures had larger social security completion scores than did unstable nations.

If we are willing to speak of nations with the highest PRI scores as being democratic and of nations with lower scores as something less than democratic, we can engage the question of whether people living in a democracy enjoy levels of social security protection that are not provided to populations in other political systems. The analysis indicated that only a small difference could be found between nations at the very highest PRI level and a second group of nations above the mean on PRI. Further, at the upper levels of economic development even nations below the mean on PRI had SIPE scores close to those of the democratic nations.

The evidence presented in this paper supports the idea that national political, economic, and social systems are interdependent. Changes in the complexity of organization in one sphere are followed by changes in organization in other areas. The specific activities that engage the attention of national governments are not independent of the general level of development. Quite the contrary is true. In spite of very great differences among nations in ideological orientation as well as in type of political organization, we found that actual activities of government in the social security field were strongly related to the complexity of social organization in economic, social, and political institutions. Nations with high levels of economic development but with less than "perfect" (i.e., democratic) political systems had government activities highly similar to those undertaken by democratic governments. Further comparative studies of government activities in other areas of social life will aid in understanding this conclusion. One might see the activities of government as intimately related to the problem of maintaining motivation and order in societies as well as being a response to the democratically organized demands of the population. A government can act without being told what to do. The scholar operating within a democratic context (and especially that of the United States) may tend to view government activities as being dependent upon the demands of secondary groups. A major but tentative conclusion that can be drawn from this study of government activity is that it need not await the petition of secondary groups. The role (or even the existence) of politically relevant secondary groups in guiding government decisions in many of the nations included in this study is modest.

In many nations we would conclude that the introduction of social security measures is a response by government to changes in the economic and social order that is not strongly affected by some degree of departure from ideal democratic organizational forms. Similar levels of social security coverage are found in nations whose governments are thought to act in response to the popular will as occur in nations whose governments are thought to act with less regard to public demands. It appears that the level of social security in a nation is a response to deeper strains affecting the organization of society. Governments may ignore human needs, but there are rather tight limits on the extent to which they may ignore organizational requirements.

Social Security Administration
Washington, D.C.

APPENDIX A

YEARS OF SOCIAL-INSURANCE PROGRAM EXPERIENCE, 1934–60, BY NATION

Nation	Years	Nation	Years
Afghanistan	15	Japan	88
Albania	53	Jordan	6
Argentina	75	Laos	0
Australia	118	Lebanon	36
Austria	121	Luxembourg	122
Belgium	135	Malaya	37
Bolivia	54	Mexico	65
Brazil	101	Mongolia	0
Bulgaria	100	Nepal	0
Burma	34	Netherlands	130
Cambodia	27	New Zealand	131
Canada	106	Nicaragua	39
Ceylon	52	Norway	121
Chile	129	Pakistan	27
China	37	Panama	67
Colombia	46	Paraguay	63
Costa Rica	62	Peru	77
Cuba	81	Philippines	41
Czechoslovakia	97	Poland	95
Denmark	117	Portugal	100
Dominican Republic	55	Romania	98
Ecuador	86	Russia	98
Finland	96	Salvador	39
France	135	Saudi Arabia	28
Germany	115	South Korea	0
Great Britain	124	Spain	131
Greece	100	Sweden	122
Guatemala	30	Switzerland	104
Haiti	10	Syria	29
Honduras	9	Thailand	5
Hungary	104	Turkey	44
Iceland	99	United States	95
India	49	Uruguay	92
Indonesia	26	Venezuela	48
Iran	40	Vietnam	31
Iraq	40	Yemen	0
Ireland	125	Yugoslavia	115
Israel	45		
Italy	133		

APPENDIX B

The index used to measure differences among national political structures is computed according to a point system. Parliament and the executive branch of government are scored as follows.

A. Parliament scoring

2 points if the largest party has less than 70 per cent of the seats in the lower or only chamber and achieved the seats through elections.

1½ points if the largest party has 70 per cent or more but the second party has at least 20 per cent.

1 point if the largest party has 70 per cent or more and the second party has less than 20 per cent.

½ point if the largest grouping in parliament is over 70 per cent with the second less than 20 *and* they do not represent parties selected by election but are appointed to represent trade, commerce, ethnic, or other interests. Includes Fascist and Communist party systems.

No points if there is no parliament, if a former parliament is dissolved during the year by coup or revolt, if it is a "constituent assembly," or if the nation still has colonial status.

B. Executive scoring.

1½ points for each year under a chief executive who was selected by a party or parliament under conditions meeting the 70 and 20 per cent rule.

1½ points if the chief executive is elected directly by the people in a competitive election held at the usual time, regardless of the 70–20 rule.

1 point when the chief executive is selected by a party that is a sustaining force (has existed for 5 years as a party in parliament), but the party composition (is not a multiparty system) violates the 70–20 rule.

1 point when the executive is selected by a party in a system that fails to observe the usual election time or goes outside the rules for having an election or has a non-competitive election or fails the 70 per cent rule.

½ point for junta, clique, non-party selection of leaders, or when existing leaders remain in power beyond the regular time.

No points to independent nations with hereditary rulers having chief executive power.

No points to nations with dependent colonial status or occupied by a foreign power.

In order to maintain a distinction between dependent and independent nations, ½ point was added to the over-all raw PRI score of each independent nation, while colonial or dependent nations received a score of zero.

It was possible for a nation to achieve from 0 to 4 points each year. A mean yearly PRI score was computed for each of the following four time periods: 1928–34; 1935–44; 1945–54; 1955–61, and a total of 304 scores (four for each of the seventy-six nations) was amassed and *T*-scored. The four distributions were *T*-scored separately as well. (A simple technique for computing the *T*-score is given in Allen E. Edwards, *Statistical Methods for the Behavioral Sciences* [New York: Holt, Rinehart & Winston, 1954]. For a single variable, *T*-scoring the raw data will yield a distribution with a mean of 50 and a standard deviation of 10.)

Community of Residence and Occupational Choice[1]

William H. Sewell and Alan M. Orenstein

ABSTRACT

This paper tests the hypothesis that there are differences in the occupational choices of those raised in rural and urban communities. For our sample of 9,986 Wisconsin high-school seniors, the proportion choosing high-status occupations increases as the size of community of residence increases. Since sex, intelligence, and socioeconomic status are each associated with both occupational choice and residence, these factors are simultaneously controlled. Differences in occupational choice by residence are eliminated for girls but maintained for boys. These differences are greatest for boys of low intelligence and for boys from high-status families; the greatest differences are for boys of low intelligence from high-status families. Several suggestions are offered regarding the selective way in which community background may operate to produce the results noted.

Studies in Europe and the United States show that rural and small-town migrants to cities hold lower-status occupational positions than the urban-born residents with whom they compete.[2] Lipset explains these differences in occupational achievement, at least in part, by assuming that there are differences in the occupational aspirations of those raised in rural and urban communities. He argues that urban-reared youth have greater acquaintance with the broad spectrum of occupational possibilities that exist in the cities than do rural youth. It is the knowledge of these opportunities which stimulates urban youth to aspire to and work toward high-status occupations.[3]

Lazarsfeld's finding that German and Austrian school youth planned to enter an occupation in direct proportion to the number of persons in their community engaged in that occupation supports Lipset's formulation.[4] Research recently conducted in this country also supports Lipset's claim that there are rural-urban differences in the occupational aspirations of youth.

[1] This research program is currently financed by a grant (M-6275) from the National Institutes of Health, U.S. Public Health Service. The basic data were obtained by J. Kenneth Little in a survey conducted in 1957 under a contract with the U.S. Office of Education (J. Kenneth Little, *A Statewide Inquiry into Decisions of Youth about Education beyond High School* [Madison, Wis.: School of Education, University of Wisconsin, 1958]). For this paper, a new sample was drawn, data recoded, and new indexes constructed. The writers wish to acknowledge the assistance of J. Michael Armer, Vimal P. Shah, and Herschel S. Shosteck, the critical suggestions of Russell Middleton, Archie O. Haller, and Murray A. Straus, and the computational services of the Numerical Analysis Laboratory of the University of Wisconsin.

[2] Gunnar Boalt, "Social Mobility in Stockholm," *Transactions of the Second World Congress of Sociology* (London: International Sociological Association, 1954), pp. 67–73; G. Beijer, *Rural Migrants in Urban Areas* (The Hague: Martinus Nijhoff, 1963); Howard W. Beers and Catherine Heflin, *Rural People in the City* (Kentucky Agricultural Experiment Station Bull. 478 [Lexington, Ky., 1946]); Ronald Freedman and Deborah Freedman, "Farm-reared Elements in the Non-farm Population," *Rural Sociology*, XXII (March, 1956), 50–61; and Seymour M. Lipset and Reinhard Bendix, *Social Mobility in Industrial Society* (Berkeley: University of California Press, 1962), pp. 203–26.

[3] Seymour M. Lipset, "Social Mobility and Urbanization," *Rural Sociology*, XX (September–December, 1955), 220–28.

[4] Paul F. Lazarsfeld, *Jugend und Beruf* (Jena: C. Fischer, 1931), p. 13, cited in Lipset and Bendix, *op. cit.*, p. 221.

Studies in Florida,[5] Iowa,[6] Kentucky,[7] Washington,[8] Michigan,[9] and Utah[10] all show that youth reared on farms, in rural non-farm areas, or in smaller cities aspire to lower prestige and less well-paid occupations than those reared in larger communities.[11]

[5] Russell Middleton and Charles M. Grigg, "Rural-Urban Differences in Aspirations," *Rural Sociology*, XXIV (December, 1959), 347–54, and Charles M. Grigg and Russell Middleton, "Community of Orientation and Occupational Aspirations of Ninth Grade Students," *Social Forces*, XXXVIII (May, 1960), 303–8.

[6] Lee G. Burchinal, "Differences in Educational and Occupational Aspirations of Farm, Small-Town and City Boys," *Rural Sociology*, XXVI (June, 1961), 107–21; and Donald R. Kaldor, Eber Eldridge, Lee G. Burchinal, and I. W. Arthur, *Occupational Plans of Iowa Farm Boys* (Iowa Agricultural Experiment Station Research Bull. 508 [Ames, Iowa, 1962]).

[7] Harry K. Schwarzweller, *Socio-cultural Factors and the Career Aspirations and Plans of Rural Kentucky High School Seniors* (Kentucky Agricultural Experiment Stations Progress Report, No. 94 [Lexington, Ky., 1960]).

[8] Walter L. Slocum, *Occupational and Educational Plans of High School Seniors from Farm and Nonfarm Homes* (Washington Agricultural Experiment Station Bull. 564 [Pullman, Wash., 1956]); John B. Edlefsen and Martin J. Crowe, *Teen-agers' Occupational Aspirations* (Washington Agricultural Experiment Station Bull. 618 [Pullman, Wash., 1960]).

[9] E. Grant Youmans, "Occupational Expectations of Twelfth Grade Michigan Boys," *Journal of Experimental Education*, XXIV (June, 1956), 259–71; James D. Cowhig, Jay Artis, J. Allen Beegle, and Harold Goldsmith, *Orientations toward Occupation and Residence: A Study of High School Seniors in Four Rural Counties of Michigan* (Michigan Agricultural Experiment Station Spec. Bull. 428 [East Lansing, Mich., 1960]).

[10] John R. Christiansen, James D. Cowhig, and John W. Payne, *Educational and Occupational Aspirations of High School Seniors in Three Central Utah Counties* (Social Science Bull. No. 1 [Provo, Utah: Brigham Young University, 1962]).

[11] An exception to this general finding comes from a Wisconsin study (Archie O. Haller and William H. Sewell, "Farm Residence and Levels of Educational and Occupational Aspiration," *American Journal of Sociology*, LXII [January, 1957], 407–11).

The present paper provides no direct evidence to test Lipset's supposition that occupational aspirations are the product of familiarity with the occupational structure of the community—nor did Lipset, for that matter. However, we do have quite adequate data to test the relationship between community structure as reflected by size, and the occupational choices of youth. Our data also make it possible to control for sex, intelligence, and socioeconomic status, all of which have been shown to be related to occupational choice and differentially distributed by residence.[12] Thus, control of one or a combination of these variables might account for the association between occupational aspirations and community size without reference to community structure.

Only one study has simultaneously controlled these variables. Grigg and Middleton[13] found in their Florida study that the relation between residence and occupational aspiration held for boys when intelligence and socioeconomic status were controlled, but not for girls when either was controlled. This study, however, was based on ninth-grade students, so the lack of realism characteristic of the occupational choices of younger students may have affected their findings. There is some evidence that this may have been the case because their results indicated that the effect of controlling intelligence differed for ninth- and twelfth-grade boys. Among ninth-grade boys, residence differences in occupational choice held within each in-

[12] For residential distribution by sex, intelligence, and socioeconomic status see William H. Sewell, "Residential Background and College Plans," *American Sociological Review*, XXIX (February, 1964), 24–38; and for relation of these variables to occupational plans see William H. Sewell, Archie O. Haller, and Murray A. Straus, "Social Status and Educational and Occupational Aspirations," *American Sociological Review*, XXII (February, 1957), 67–73. Both papers contain numerous references to the relevant literature.

[13] *Op. cit.*

telligence group. Among high-school seniors, no statistically significant differences were found except for boys of low intelligence, though differences in the other intelligence groups were in the predicted direction.[14] Thus, the further control of socioeconomic status might eliminate the remaining differences among high-school seniors. This would limit any assumed effect of community structure to young boys with relatively unrealistic occupational choices.

The present research tests the hypothesis that among Wisconsin high-school seniors there is no relation between size of community of residence and occupational choice when sex, intelligence, and socioeconomic status are controlled. The analysis proceeds by the following steps: (1) the relation between community of residence (the independent variable) and occupational choice (the dependent variable) is established; (2) the relation between sex, intelligence, and socioeconomic status (the control variables) and both community of residence and occupational choice is determined; (3) because the three control variables are related to the independent and dependent variables, sex, intelligence, and socioeconomic status are simultaneously controlled.[15] The tables generated by these operations also provide data for testing empirical findings of Lipset and Bendix that rural-urban differences in occupational choice are greatest for boys from low-status families[16] and of Middleton and Grigg that these differences are greatest for less intelligent boys.[17]

THE DATA

The present study is based on a 1957 survey of graduating seniors in all public, private, and parochial schools in Wisconsin.[18] Information was obtained from the respondents and school authorities on the student's educational and occupational plans, measured intelligence, attitudes toward college, and socioeconomic status. The analysis employs a one-third random sample of all cases having the requisite data.[19] Because the major hypothesis of this study is based on Lipset's explanation for differential achievement in the urban labor market in terms of differences in occupational aspiration, students planning to farm were eliminated from the sample. This reduced the number of cases available for analysis from 10,321 to 9,986.

The independent variable, community of residence, is based on the size of the community in which the student attended high school, except that all students residing on farms, regardless of where they attended high school, are classified as farm residents. Data are presented for the following categories: farm, village (places under 2,500), small city (2,500 to 25,000), medium city 25,000 to 100,000), and large city (100,000 and over). A second set of categories is used in which the farm and

[14] Middleton and Grigg, *op. cit.* Other possible reasons for the apparent differences in the Florida ninth- and twelfth-grade studies are that the latter had a much smaller sample than the former, and the ninth-grade study reported aspirations to professional occupations while the twelfth-grade study was concerned with aspirations to white-collar occupations.

[15] This technique is called "elaboration" or "elaboration by partials" and is fully described in Herbert Hyman, *Survey Designs and Analysis* (Glencoe, Ill.: Free Press, 1955), chaps. vi and vii.

[16] *Op. cit.*, p. 222.

[17] *Op. cit.*, p. 352.

[18] For further details about how the original survey was conducted, see Little, *op. cit.*, pp. 2–6. The information upon which Little's report was based was derived from a one-sixth sample of all cases, while for the present analysis a new sample consisting of one-third of the cases was drawn.

[19] This requirement resulted in the loss of 426 cases. Practically all of the losses were due to failure to obtain intelligence-test scores. A comparison of those omitted by sex, residence, father's occupation, rank in high-school class, and college plans showed that they did not differ significantly from those included in the sample.

village students are combined into a rural category, the small and medium cities into a smaller urban category, with the large-city category left as it is, but for the sake of symmetry, now called larger urban.

The dependent variable, occupational choice, is based on a series of questions about the type of occupation the respondent eventually plans to enter and the type of schooling or work he will be involved in during the following year.[20] The answers to these questions, obtained late in the senior year after the students had given considerable thought to the matter, may be taken as fairly realistic occupational choices. These choices were coded according to major occupational groupings as specified in the 1950 detailed classification of the Bureau of the Census. "Professional, Technical and Kindred Workers," and "Managers, Officials, and Proprietors" are considered high-status occupational choices. Plans falling within the other major occupational groupings are considered low-status choices. Those choosing to be "Farmers" or "Farm Managers" are of course eliminated from the sample.

The control variables in the analysis are sex, intelligence, and socioeconomic status. The intelligence variable is based on scores on the Henmon-Nelson Test of Mental Maturity which is administered annually to all high-school seniors in Wisconsin.[21] Students were divided into approximately equal thirds in measured intelligence: high (I.Q.: 116 and above), middle (I.Q.: 105–115), and low (I.Q.: below 105). The socioeconomic status classification is based on a factor-weighted combination of father's educational level, mother's educational level, an estimation of the funds the family could provide if the student were to attend college, the degree of sacrifice this would entail for the family, and the approximate wealth and income status of the student's family.[22] The sample was

[20] The student's occupational choice was evoked by the following item: "I hope eventually to enter the type of occupation checked below." This was followed by a series of precoded socioeconomic categories, such as professional, executive, small business ownership or management, salesman, office worker, etc., and a blank in which unlisted occupations were to be entered. Students were also asked to specify their field or training program if they planned to enter a trade or vocational school, or their field of interest if they planned to enter a college or university. The primary information in determining occupational choice was obtained from answers to the first item. No one was classified as having a professional or executive choice unless he planned to continue with some kind of formal education beyond high school—with a few exceptions, such as entertainers, dancers, and musicians. Respondents who planned on college but whose occupational plans were not indicated were coded as having high-status occupational choices. For 7.3 per cent of the sample no information on occupational choice could be obtained from the responses. There were no large differences by community of residence or sex. An additional 1.1 per cent of the girls gave no information on occupational plans other than that they planned to marry after graduation, and 9.0 per cent of the male respondents indicated only that they planned to enter military service after graduation. Since none of these youths indicated that they planned to go to college at some later time, it was inferred that they were not choosing high-status occupations as defined in this study. There were no significant differences among the residence groups in the proportion of girls planning to marry or of boys planning to enter military service.

[21] V. A. C. Henmon and M. J. Nelson, *The Henmon-Nelson Test of Mental Ability* (Boston: Houghton Mifflin Co., 1942).

[22] The five indicators were factor-analyzed using the principal-components method, and were orthogonally rotated according to the verimax criterion. This produced a two-factor structure composed of a factor on which the three economic items were most heavily loaded, and a factor on which the two educational items were most heavily loaded. The composite socioeconomic status index was developed by squaring the loadings of the principal items of each factor as weights, then multiplying student scores on the items by the respective weights, and finally summing the weighted scores of the principal items on each factor. The two factors were combined into a composite socioeconomic status score after multiplying the factor scores of all students by certain constants which would produce approximately equal variances for both status dimensions. The resulting sum of the weighted scores was then multiplied by a constant to produce a theoretical range of scores between 0 and 99.

divided into three roughly equal-sized categories labeled high, middle, and low in socioeconomic status.

RESULTS AND ANALYSIS

The first data presented in the analysis demonstrate the association between the independent and dependent variables of the study. The "total" column of Table 1 indicates a monotonic relation between size of community and occupational choice. Rural students rank considerably below those from small- and medium-size cities, who in turn are less likely to choose professional and managerial positions than those from large cities; the range between the farm and large-city categories is from 29.8 per cent to 48.6 per cent.[23]

The next step is to examine the relation between the control variables and occupational choice and between the control variables and community size. From data presented in Tables 1 and 2 it is evident that each of the control variables is associated with occupational choice since boys, students with high intelligence, and those from high socioeconomic status backgrounds are more likely to choose high-status occupations than are girls, students of lower intelligence, and those of lower socioeconomic status.[24]

The data on the relation between the control variables and community size have been published elsewhere and need not be presented here, but they indicate a significant positive association between

TABLE 1

PERCENTAGE WITH HIGH OCCUPATIONAL CHOICES, BY PLACE OF RESIDENCE, FOR MALE AND FEMALE HIGH-SCHOOL SENIORS*

Place of Residence	Males	Females	Total
(1) Farm	32.9 (678)†	27.6 (935)	29.8 (1,613)
(2) Village (under 2,500)	35.7 (905)	31.1 (930)	33.3 (1,835)
(3) Small city (2,500–25,000)	45.4 (1,217)	38.1 (1,219)	41.7 (2,436)
(4) Medium city (25,000–100,000)	45.7 (1,085)	40.8 (1,226)	43.1 (2,311)
(5) Large city (100,000 and more)‡	57.2 (802)	41.7 (989)	48.6 (1,791)
Rural (1 and 2)	34.5 (1,583)	29.3 (1,865)	31.7 (3,448)
Smaller urban (3 and 4)	45.5 (2,302)	39.4 (2,445)	42.4 (4,747)
Larger urban (5)	57.2 (802)	41.7 (989)	48.6 (1,791)
Total (1–5)	43.8 (4,687)	36.3 (5,299)	39.9 (9,986)

* All χ^2's for each column and any set of residence categories in this table are significant beyond the 0.05 level.

† In this and all other tables the number in parentheses is the denominator on which the percentage preceding it is based.

‡ Only two cities are included in this category—Madison (population 126,706) and Milwaukee (population 741,324).

[23] If the cutting point between high- and low-status choices had been white-collar occupations rather than professional and executive positions, the percentages in Table 1 would have been as follows: *For males:* farm, 47.3; village, 50.3; small city, 59.6; medium city, 60.5; large city, 70.0; rural, 49.0; smaller urban, 60.0; larger urban, 70.0. *For females:* farm, 79.5; village, 76.7; small city, 81.0; medium city, 83.4; large city, 87.9; rural, 78.0; smaller urban, 82.2; larger urban, 87.9. *For the total sample:* farm, 65.9; village, 64.6; small city, 70.3; medium city, 72.6; large city, 79.9; rural, 64.7; smaller urban, 71.4; larger urban, 79.9. Thus, no important change in the pattern of occupational choice by residence occurs when all white-collar positions are included as high-status occupational choices.

[24] Both intelligence and socioeconomic status appear to contribute more to differences in occupational choice than does community of residence. Rough evidence is that the three-category break for residence shows a percentage difference in high occupational choice between the rural and larger urban categories of only 16.9 for the whole sample (Table 1), while the percentage difference between the low- and high-intelligence thirds is 42.9; and the percentage difference between the low- and high-socioeconomic status thirds is 43.7 (Table 2).

community size and intelligence and community size and socioeconomic status.[25] The association between community size and sex is less regular in that the proportion of males is lower in the farm and larger urban categories than in other categories.

Since the control variables are each related to occupational choice and differentially distributed by community size, the next step is to partial out sex, then sex and intelligence, then sex and socioeconomic status, and finally sex, intelligence, and socioeconomic status simultaneously to see if any of these operations will cause community of residence differences in occupational choice to vanish.

Table 1 gives the results of controlling by sex. The monotonic relation between community of residence and occupational choice remains for both sexes, but partialling increases the residential differences for boys and reduces them for girls. When both sex and intelligence are controlled, as in Table 3, the community of residence differences in occupational choice (compared with the distribution shown in the "total" column) is reduced but not eliminated. For boys the association remains for each intelligence third. Though there are two reversals in ordering in the high-intelligence third, clear community size differences are evident. For girls, residence differences in occupational choice are practically eliminated in the low-intelligence third, reduced in the middle third, but increased in the high-intelligence third.[26]

The results of controlling socioeconomic status for male and female students are shown in Table 4. For boys, there are clear residence differences at each status level—the differences being greatest among the high-status boys and least among the low-status boys.[27] In two instances there are reversals in ordering in the low-status group and in one instance in the middle status category, whereas there are no exceptions to the monotonic ordering in the high-status group. For girls, controlling on socioeconomic status eliminates residence differences for the low-status group, re-

TABLE 2

PERCENTAGE WITH HIGH OCCUPATIONAL CHOICES, BY INTELLIGENCE AND SOCIOECONOMIC STATUS, FOR MALE AND FEMALE HIGH-SCHOOL SENIORS*

	Males	Females	Total
Intelligence:			
Low	20.8 (1,510)	17.0 (1,795)	18.7 (3,305)
Middle	42 5 (1,549)	35 8 (1,790)	38.9 (3,339)
High	66.4 (1,628)	57.1 (1,714)	61.6 (3,342)
Total	43.8 (4,687)	36.3 (5,299)	39.8 (9,986)
Socioeconomic status:			
Low	23 9 (1,521)	14.7 (1,848)	18.8 (3,369)
Middle	41.9 (1,540)	34.8 (1,724)	38.1 (3,264)
High	64.3 (1,626)	60 9 (1,727)	62 5 (3,353)
Total	43.8 (4,687)	36.3 (5,299)	39 8 (9,986)

* All χ^2's for each column in this table are significant beyond the 0.05 level.

[25] Sewell, *op. cit.*, Table 3, p. 29. The data presented in this table are based on the total sample, while the present study excludes 335 farm boys who planned to farm. Consequently, the sex distribution in the farm category is much more unbalanced for the present sample (males 42 per cent, females 58 per cent) than is shown in the above-mentioned table. Otherwise, the distributions are closely similar.

[26] Middleton and Grigg, *op. cit.*, p. 351, and Grigg and Middleton, *op. cit.*, p. 306, find no differences between rural and urban girls when they control for intelligence or socioeconomic status.

[27] This is opposite to the findings of Lipset and Bendix, *op. cit.*, p. 213.

PERCENTAGE WITH HIGH OCCUPATIONAL CHOICES, BY PLACE OF RESIDENCE AND INTELLIGENCE, FOR MALE AND FEMALE HIGH-SCHOOL SENIORS*

PLACE OF RESIDENCE	INTELLIGENCE			
	Low Third	Middle Third	High Third	Total
Males:				
Farm	12.6 (247)	32.5 (234)	58.9 (197)	32.9 (678)
Village	18.2 (351)	38.5 (283)	55.4 (271)	35.7 (905)
Small city	20.7 (376)	41.6 (401)	69.8 (440)	45.4 (1,217)
Medium city	21.7 (350)	46.2 (351)	67.2 (384)	45.7 (1,085)
Large city	34.9 (186)	51.4 (280)	74.4 (336)	57.2 (802)
Rural	15.9 (598)	35.8 (517)	56.8 (468)	34 5 (1,583)
Smaller urban	21.2 (726)	43.8 (752)	68.6 (824)	45.5 (2,302)
Larger urban	34.9 (186)	51.4 (280)	74.4 (336)	57.2 (802)
Total	20.8 (1,510)	42.5 (1,549)	66.4 (1,628)	43.8 (4,687)
Females:				
Farm	15.6 (333)	27.2 (345)	43.6 (257)	27.6 (935)
Village	15.0 (353)	35.8 (307)	46.7 (270)	31.1 (930)
Small city	19.6 (424)	36.5 (411)	60.2 (384)	38.1 (1,219)
Medium city	16.5 (387)	40.7 (386)	61.6 (453)	40.8 (1,226)
Large city	17.8 (298)	37.8 (341)	65.7 (350)	41.7 (989)
Rural	15.3 (686)	31.3 (652)	45.2 (527)	29.3 (1,865)
Smaller urban	18.1 (811)	38.5 (797)	60.9 (837)	39.4 (2,445)
Larger urban	17.8 (298)	37.8 (341)	65.7 (350)	41.7 (989)
Total	17.0 (1,795)	35.7 (1,790)	57.1 (1,714)	36.3 (5,299)

* All χ^2's for each column in this table for any set of five and three residence categories, except for females in the low-intelligence third (N.S.), are significant beyond the 0.05 level.

TABLE 4

PERCENTAGE WITH HIGH OCCUPATIONAL CHOICES, BY PLACE OF RESIDENCE AND SOCIOECONOMIC STATUS, FOR MALE AND FEMALE HIGH-SCHOOL SENIORS*

PLACE OF RESIDENCE	SOCIOECONOMIC STATUS			
	Low	Middle	High	Total
Males:				
Farm	22.0 (305)	37.4 (238)	49.6 (135)	32.9 (678)
Village	19.2 (338)	37.5 (315)	55.6 (252)	35.7 (905)
Small city	24.3 (374)	42.4 (363)	64.0 (480)	45.4 (1,217)
Medium city	23.8 (311)	40.3 (360)	66.9 (414)	45.7 (1,085)
Large city	34.2 (193)	52.7 (264)	73.6 (345)	57.2 (802)
Rural	20.5 (643)	37.4 (553)	53 5 (387)	34.5 (1,583)
Smaller urban	24.1 (685)	41.4 (723)	65.3 (894)	45.5 (2,302)
Larger urban	34.2 (193)	52.7 (264)	73.6 (345)	57.2 (802)
Total	23.9 (1,521)	41.9 (1,540)	64.3 (1,626)	43.8 (4,687)
Females:				
Farm	17.1 (439)	30.7 (313)	47.5 (183)	27.6 (935)
Village	14.5 (359)	33.1 (296)	50.5 (275)	31.1 (930)
Small city	11.2 (402)	34.7 (400)	67.1 (417)	38.1 (1,219)
Medium city	16.8 (375)	36.7 (390)	63.8 (461)	40.8 (1,226)
Large city	13.5 (273)	38.1 (325)	64.2 (391)	41.7 (989)
Rural	15.9 (798)	31.8 (609)	49.3 (458)	29.3 (1,865)
Smaller urban	13.9 (777)	35.7 (790)	65.4 (878)	39.4 (2,445)
Larger urban	13.5 (273)	38 1 (325)	64.2 (391)	41.7 (989)
Total	14.7 (1,848)	34.8 (1,724)	60.9 (1,727)	36.3 (5,299)

* All χ^2's for each column in this table for any set of five residence categories, except for females in the low- and middle-intelligence third (N.S.), are significant beyond the 0.05 level.

duces differences in the middle-status group to a non-significant level, and increases the differences in the high-status category.

The above analysis indicates that control of sex and intelligence or sex and socioeconomic status does not eliminate community-of-residence differences in occupational choice. For boys, significant differences are found for each subpopulation examined. For girls, statistically significant differences are eliminated for the low-intelligence third and for those in the low- and middle-socioeconomic status categories. Within the other subpopulations, however, the originally observed differences are maintained or increased as a result of partialling.

Partialling on all three variables produces nine tables for each sex in which each of the three socioeconomic status categories is combined with each of the three intelligence categories (see Table 5). For example, the first column of the table shows that 12.4 per cent of farm boys in the lowest socioeconomic third who are also in the lowest intelligence third choose a high-status occupation, while 9.2 per cent of the village, 10.4 per cent of the small-city, 11.9 per cent of the medium-city, and 18.8 per cent of the large-city boys with the same characteristics choose high-status occupations. These differences are small and not statistically significant. For the low-status–middle-intelligence boys and the low-status–high-intelligence boys the differences, in comparison with the original differences (shown in the "total" column at the far right of the table), are also reduced and not statistically significant.

Among boys in the middle-socioeconomic status third, residence differences are considerably altered for all but the low-intelligence third, where high-status choices may be unrealistic in any case.

Among boys from high-socioeconomic-status families the community-of-residence differences in occupational choice remain for each intelligence third. The greatest differences, however, are now found among the high-status boys of low intelligence. There are still fairly large differences between the farm, village, and city boys in the high-status–middle-intelligence third, whereas the differences in the high-status–high-intelligence group are greatly reduced but still favor the city boys.

If only the three-level residence categories are considered, the monotonic relationship between size of community and occupational choice of boys is never altered by the control of socioeconomic status and intelligence: the percentage of rural boys choosing high-status occupations is always lower than for those from the smaller urban places, and they in turn are always less likely to choose high-status occupations than are boys from larger urban places.[28]

Data for girls are presented in the bottom half of Table 5. There is no significant association between residential background and occupational choice for girls in any of the subpopulations shown in the first seven columns of the table. In each of these groups the differences between communities are small and the ordering is irregular. For the two remaining subpopulations, high-status–middle-intelligence and high-status–high-intelligence groups, the association is significant, but the ordering is irregular and the differences are not great. Consequently, we conclude that the control of socioeconomic status and intelligence largely eliminates the relation of residence and occupational choice for the girls but not for the boys in our sample.

To obtain a more precise test of our conclusions, direct standardization was applied to the original data. This demographic technique provides a summary measure of what population rates would be if specified population characteristics were held constant. In this application,

[28] This confirms Grigg and Middleton's general conclusion for ninth-grade Florida boys (*op. cit.*, pp. 307–8).

TABLE 5

PERCENTAGE WITH HIGH OCCUPATIONAL ASPIRATIONS, BY PLACE OF RESIDENCE, SOCIOECONOMIC STATUS, AND INTELLIGENCE, FOR MALE AND FEMALE HIGH-SCHOOL SENIORS

Place of Residence	Low Socioeconomic Status			Middle Socioeconomic Status			High Socioeconomic Status			Total†
	Low Intelligence	Middle Intelligence	High Intelligence	Low Intelligence*	Middle Intelligence	High Intelligence*	Low Intelligence*	Middle Intelligence†	High Intelligence†	
Males:										
Farm	12.4 (129)	20 0 (100)	40.8 (76)	9.4 (85)	40.9 (88)	69.2 (65)	21.2 (33)	43.5 (46)	71.4 (56)	32.9 (678)
Village	9.2 (164)	24.8 (109)	35.4 (65)	23.9 (113)	39.4 (104)	51 0 (98)	29.7 (74)	58.6 (70)	71.3 (108)	35.7 (905)
Small city	10.4 (163)	27.1 (129)	47.6 (82)	23.3 (116)	40.3 (134)	64.6 (113)	35.1 (97)	56.5 (138)	79.6 (245)	45.4 (1,217)
Medium city	11.9 (135)	26.2 (103)	42.5 (73)	19.0 (121)	44 3 (113)	57.1 (126)	39.4 (94)	63.0 (135)	83.8 (185)	45.7 (1,085)
Large city	18.8 (64)	34.2 (79)	54.0 (50)	35.4 (65)	46.3 (95)	69.2 (104)	52.6 (57)	68.9 (106)	83.0 (182)	57.2 (802)
Rural	10.6 (293)	22.5 (209)	38.3 (141)	11.7 (198)	40 1 (192)	58.3 (163)	27.1 (107)	52.6 (116)	71.3 (164)	34.5 (1,583)
Smaller urban	11.1 (298)	26.7 (232)	45.2 (155)	21.1 (237)	42.1 (247)	60 7 (239)	37.2 (191)	59.7 (273)	81.4 (430)	45.5 (2,302)
Larger urban	18 8 (64)	34.2 (79)	54.0 (50)	35.4 (65)	46.3 (95)	69.2 (104)	52.6 (57)	68.9 (106)	83.0 (182)	57.2 (802)
Total	11.6 (655)	26.2 (520)	43.7 (346)	21.6 (500)	42.1 (534)	61.7 (506)	36 6 (355)	60.0 (495)	79.6 (776)	43.8 (4,687)
Females:										
Farm	8.9 (190)	19.0 (142)	29.0 (107)	23.5 (98)	28.1 (121)	41.5 (94)	26.7 (45)	40.2 (82)	75.0 (56)	27.6 (935)
Village	5.9 (187)	18.5 (108)	32.8 (64)	20.7 (106)	31.3 (99)	49.4 (91)	33.3 (60)	59.0 (100)	52.0 (115)	31.1 (930)
Small city	4.8 (187)	12.8 (141)	24.3 (74)	21.1 (142)	37.2 (137)	47.9 (121)	46.3 (95)	60.9 (133)	82 0 (189)	38.1 (1,219)
Medium city	7.3 (179)	18.9 (127)	37.7 (69)	15.8 (120)	41.2 (131)	50.4 (139)	36.4 (88)	61.7 (128)	74.7 (245)	40.8 (1,226)
Large city	4.9 (122)	14.7 (95)	30.4 (56)	24.1 (108)	31.3 (106)	58.6 (111)	30.9 (68)	58.6 (140)	80.9 (183)	41.7 (989)
Rural	7.4 (377)	18.8 (250)	30.4 (171)	22.0 (204)	29 5 (220)	45.4 (185)	30.5 (105)	50.5 (182)	59.6 (171)	29.3 (1,865)
Smaller urban	6.0 (366)	15.7 (268)	30.8 (143)	18.7 (262)	39.2 (268)	49.2 (260)	41.5 (183)	61.3 (261)	77.9 (434)	39.4 (2,445)
Larger urban	4.9 (122)	14 7 (95)	30.4 (56)	24.1 (108)	31.3 (106)	58.6 (111)	30.9 (68)	58.6 (140)	80.9 (183)	41.7 (989)
Total	6.5 (865)	16.8 (613)	30.5 (370)	20.9 (574)	34.2 (594)	49.9 (556)	36.2 (356)	57.3 (583)	74.6 (788)	36.3 (5,299)

* χ^2 significant beyond 0.05 level for males only.

† χ^2 significant beyond 0.05 level for males and females.

standardization provides a simple and precise summary measure of the effect on size of community differences in occupational choice of controlling sex, intelligence, and socioeconomic status for the sample as a whole and of controlling intelligence and socioeconomic status for each sex.[29] The results are shown in Table 6 along with the original or unstandardized data.

Again, controlling on sex, intelligence, and socioeconomic status is shown to reduce the relation between residential background and occupational choice for the sample as a whole, but the combined effect of these variables does not eliminate the original association. The original 18.6 percentage-point difference between the two extreme categories, farm and large city, is reduced by more than one-half. For females, standardization on intelligence and socioeconomic status largely eliminates the community-of-residence differences in occupational choice. For males the differences are reduced in magnitude but the pattern remains. These findings constitute the most impressive evidence yet available in support of Lipset's hypothesis that the occupational structure of a community, to the extent that it is inferable from community size, is related to the occupational aspirations of youth. The evidence, however, is still far from complete. There are doubtless other factors associated with community size and occupational choice which would help to account for the association. This possibility should be explored in future research.

TABLE 6

ORIGINAL AND STANDARDIZED RELATIONS BETWEEN PLACE OF RESIDENCE AND HIGH OCCUPATIONAL CHOICE

Place of Residence	Males		Females		Total	
	Original	Standardized*	Original	Standardized*	Original	Standardized†
Farm	32.9	38.1	27.6	33.1	30.0	35.5
Village	35.7	39.4	31.1	32.8	33.3	35.9
Small city	45.4	43.8	38.1	37.5	41.7	40.5
Medium city	45.7	44.4	40.8	37.9	43.1	40.0
Large city	57 2	51.9	41.7	35.7	48.6	44.2
Rural	34.5	39.3	29.3	32.4	31.7	34.1
Smaller urban	45.5	44.0	39.4	36.5	42.6	39.7
Larger urban	57.2	51.9	41.7	37.4	48.6	44.2

* Direct standardization of intelligence and socioeconomic status.

† Direct standardization of sex, intelligence, and socioeconomic status.

DISCUSSION

The foregoing analysis shows that boys, but not girls, from rural areas and smaller communities have lower occupational aspirations than those from larger urban places—independent of intelligence and socioeconomic differences. This finding agrees with the general conclusion of Grigg and Middleton for Florida boys at the ninth-grade level, but is more conclusive because it is based on more extensive community-of-residence breakdowns and is for high-school seniors whose occupational aspirations are likely to be realistic and meaningful.[30] Our findings are also of par-

[29] See Morris Rosenberg, "Test Factor Standardization as a Method of Interpretation," *Social Forces*, XL (October, 1962), 53–61.

[30] *Op. cit.*, pp. 307–8.

ticular interest because the only past study failing to demonstrate a relation between residence and occupational plans used a sample of Wisconsin students. In an earlier study, Haller and Sewell found no differences in the occupational plans of farm boys who did not plan to enter farming and other non-farm high-school seniors.[31] At that time, however, the high schools of Wisconsin were retaining a much smaller proportion of farm youth than at present. Thus differential dropout rates probably affected the earlier findings in that many farm youth with relatively low occupational aspirations dropped out of school. By the time the present data were gathered, changes in Wisconsin compulsory school laws had resulted in retention in high school of many more low-aspiring farm youth with the consequence that rural-urban differences in occupational choice may now appear greater.[32]

Whatever may account for the differences between the two Wisconsin studies, clearly other factors than those tested are currently operating to produce the residence differences in the present study. Much literature suggests that certain aspects of community structure may be influential. One of these is the limited educational opportunities found in most rural communities. In Wisconsin all publicly supported colleges and most private colleges are in urban communities. Thus, the greater cost and difficulty of attending college away from home may prevent youth in rural places from continuing their education beyond high school. This limits the range of occupational positions available to them and might result in their aspiring to lower-status positions. If this were a major factor, however, we would expect the greatest residence differences among boys from low-status families, those families least able to send their children away to college. In fact we find that community-of-residence differences in occupational choice are least among lower-status youth.

As previously noted, Lipset argues that the occupational structure of the community influences occupational aspirations. Lipset and Bendix further suggest that residence differences should be greatest for youths from lower-status families: "Middle-class youth, even those living in smaller communities, will receive the stimulus to obtain a high-status occupation from their families and from other aspects of their environment which are related to middle-class status. It is among the working class youth that size of community makes a major difference. Those living in smaller communities will not be as stimulated by their environment to aspire to higher goals."[33] Our results contradict this point. Size-of-community differences in occupational plans are greatest for high-socio-economic-status youth whether status is controlled separately or controlled with intelligence, while there are only small differences among low-status youth.

This finding calls for further specification of the type of information derived from the local occupational structure which could influence occupational plans. Certainly high-school seniors in the smaller community are not completely ignorant of the major professional, managerial, and technical positions available in larger urban communities. Nor do we assume a marked deficiency in their knowledge of the entrance requirements of most of these positions. We doubt that such differences as exist are sufficient to explain the residence differences in occupational choice. More likely, there are differences in the

[31] *Op. cit.*, p. 410.

[32] In 1950 the proportions of sixteen- and seventeen-year-old Wisconsin farm, rural non-farm, and urban youth attending school were 70, 82, and 91 per cent, respectively, while in 1960 the corresponding figures were 88, 87, and 89 per cent (see Douglas G. Marshall, *Wisconsin's Population, Changes and Prospects, 1900–1963* (Wisconsin Agricultural Experiment Station Research Bull. 241 [Madison, Wis., 1963], p. 29).

[33] *Op. cit.*, p. 222.

direct personal knowledge youth have about high-status positions, and these differences are related to both residence and status.

This paper deals with fairly realistic occupational choices, based in part on the youth's belief that he will be able to achieve his goals. A youth in immediate contact with persons holding high-status positions, or receiving a more or less continuous flow of information concerning their daily activities, will perceive these persons as occupational role models and will feel that their occupational positions are reasonable personal goals. This belief will be reinforced when parents, teachers, and friends encourage high goals. All of this is less likely to be the experience of youth from smaller communities.

Higher-status youth in small communities are exposed to a more restricted range of occupations than are higher-status youth in larger places. They personally know fewer of the occupants of high-status positions and hear little about their activities from adult acquaintances. Moreover, the relevant adults in their lives probably recommend lower-prestige occupations than would urban adults of similar status who have better acquaintance with a complex occupational structure.

Also working against the higher-status youth in the small community is the restricted interactional situation in the school and community in which he lives. This forces him to associate with many low-aspiring peers who may have a depressing influence on his aspirations.[34] On the other hand, the higher-status youth in the larger community is likely to interact mainly with high-status peers who reinforce his high occupational aspirations.

The case of the lower-status boy, whether rural or urban, is of course quite different. His intimate adult contacts are restricted to those in lower-status occupational positions. Lower-status adults lack intimate and detailed knowledge of the activities of high-status persons to pass on to the lower-status youth. They also have lower economic aspirations for themelves and recommend lower-status occupations to others.[35] Thus, the lower-status boy tends to aspire to low-status positions regardless of the occupational structure of his community.

Another, perhaps more crass, interpretation of our findings is that higher-status urban families manage through friends, relatives, and acquaintances to give their children advantages in the urban labor market. Higher-status rural parents whose knowledge and influence are limited to the local community can provide only limited assistance in obtaining high-status urban positions. Higher-status rural youth, knowing this, may direct their aspirations toward the more limited opportunity structure of the local community or toward the less prestigeful positions they perceive as open to them in the larger communities.

Our findings regarding the influence of controlling intelligence generally fail to support the finding of Middleton and Grigg that the rural-urban differences are primarily for boys of low intelligence. We find significant differences in each intelligence third, and even though they are somewhat greater for the lower third, the differences are still quite sizable in the highest intelligence group. It may be, as they reason,

[34] James S. Coleman, *The Adolescent Society* (New York: Free Press of Glencoe, 1961); Coleman, "The Adolescent Subculture and Academic Achievement," *American Journal of Sociology*, LXV (January, 1960), 337–47; and Archie O. Haller and C. E. Butterworth, "Peer Influences on Levels of Occupational and Educational Aspiration," *Social Forces*, XXXVIII (May, 1960), 289–95.

[35] Genevieve Knupfer, "Portrait of the Underdog," in Reinhard Bendix and Seymour M. Lipset (eds.) *Class, Status and Power* (Glencoe, Ill.: Free Press, 1953), pp. 255–63; and Herbert Hyman, "The Values Systems of Different Classes: A Social Psychological Contribution to the Analysis of Stratification," in Bendix and Lipset, *ibid.*, pp. 426–42.

that the boys of lower intelligence are most influenced by firsthand impressions of the local labor market, while the highly intelligent rural boys become aware of the variety of opportunities beyond the local community as a result of reading, counseling, and other secondary sources of information.[36] But if this is true, it has not produced any very marked effect on the boys in our sample.

A more sociologically significant finding is that when both intelligence and socioeconomic status are controlled, community-of-residence differences in occupational choice are greatest among those of low intelligence and high socioeconomic status and are minimal among those of low intelligence and low socioeconomic status. Perhaps direct contact with a highly differentiated occupational structure overcomes the tendency to low occupational aspiration of those of low intelligence if their status makes them optimistic about their life chances. Also, the proportion of high-status openings for those of limited ability may be greater in urban than rural communities. These "sinecure" positions are entered primarily through family influence and financial aid. If this type of family inheritance is more common in urban society, it may help to account for the particularly marked rural-urban differences in the occupational plans of the high-status–low-intelligence boys in our sample.

Finally, our finding that the original association between community of residence and occupational choices of girls can be largely accounted for by intelligence and socioeconomic status differences agrees with previous studies. The occupational alternatives for girls in rural communities are so severely limited that those who wish to work, and most of the girls in our sample plan on some period of employment before marriage, must look to the urban labor market for desirable employment. There the job restrictions generally encountered by women force them to consider essentially the same limited set of occupational alternatives as urban girls—mainly teaching school, nursing, social work, and a few other lower-status professions and white-collar jobs. These occupations are widely known, and rural girls, lacking satisfactory rural occupational opportunities, are as likely to aspire to them as urban girls of similar intelligence and socioeconomic status.[37]

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[36] *Op. cit.*, p. 352.

[37] Many non-occupational features of the urban community are also attractive to rural girls, including the wider range of marital opportunities, more varied social life, and higher standard of living.

On the Statistical Analysis of Mobility Tables[1]

Leo A. Goodman

ABSTRACT

This article presents (*a*) mathematical models for analyzing patterns of mobility and (*b*) statistical methods for testing whether observed patterns are congruent with patterns predicted by the models. To illustrate the application of these rather general models and methods, we reanalyze data on intergeneration social mobility in Britain and in Denmark which were studied earlier by Glass and his co-workers, by Svalastoga, and by White. We find, for example, that White's main results require modification. His predictions differ from the observed data in statistically significant ways, whereas the models introduced herein lead to predictions that fit the data well. Using the models and methods developed in the present article, we shall propose and test the theory that "status inheritance" from father to son is operative within certain limits: on the one hand there is "status inheritance" in every social stratum; but on the other hand for an individual who actually moves out of his father's stratum—either up or down—his own social status is, in a certain sense, independent of his father's status. When this theory is properly qualified, we find that it is supported by the British and Danish data.

The mathematical models and statistical methods which we shall present in this article can be applied to test a variety of substantive theories concerning mobility. In order to illustrate this application we shall propose a particular substantive theory and a mathematical model to describe this theory, and we shall use our statistical methods to test whether data on British and Danish social mobility patterns are congruent with the theory.[2] The theory tested here is a rather simple one which contradicts in some respects what might be regarded as "common sense," but we shall see that the observed data support this theory when it is properly qualified. In brief, the theory states that there is some "status inheritance" from father to son in every social stratum; *but*, once a son has moved out of his father's stratum, his father's social status exerts no further influence (in a certain sense) on his own status. If our theory (as explained more fully below) is correct, then it would contradict the more "reasonable" view that the father's status exerts a continuing influence on his son's status (even after the son has moved out of his father's stratum), making it increasingly difficult for the son to move further and further away. At first glance, the observed patterns of mobility for the mobile people might actually suggest the more "reasonable" view to some readers, but we shall see that with our theory it will be possible to "explain" or "predict" all of these patterns using only the reduced number of estimated parameters in the model corresponding to our theory. If our theory were correct, there would be no need to introduce, for example, the "distance" between a mobile person's status and his father's status (or other related factors) in order to explain or predict these patterns. With our theory, these patterns are explainable merely in terms of factors which affect (*a*) the (marginal) distribution of mobile people in the different strata and (*b*) the corresponding distribution for their fathers. In the present article, we shall see what the par-

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[2] These data were obtained from sample surveys conducted by D. V. Glass and his co-workers, *Social Mobility in Britain* (Glencoe, Ill.: Free Press, 1954), and by Kaare Svalastoga, *Prestige, Class and Mobility* (London: William Heinemann, Ltd., 1959). They studied social mobility in terms of intergeneration changes in occupational status.

ticular qualifications of this theory are and in which respects it is supported by the observed data.

In earlier studies of social mobility, the concept of "perfect mobility" was introduced to describe the idealized situation where an individual's social status is independent of his father's status; that is, where the father's status exerts no influence at all on his son's status.[3] Survey data describing the observed cross-classification of each respondent's social status and his father's status actually differed from what would have been expected in the case of "perfect mobility," but these expectations derived for the "perfect-mobility" model were used as a standard for measuring the extent to which observed mobility between the social strata was imperfect. With respect to the observed cross-classification table, the expected pattern of mobility (between the strata) derived for the perfect-mobility model could be estimated simply from the observed (marginal) distribution of respondents in the different strata and the corresponding observed distribution for their fathers. If the observed and expected patterns of mobility had been congruent, this would have indicated that the perfect-mobility hypothesis had been supported by the data, that an individual's status had appeared to be independent of his father's status, that the observed pattern of mobility had been "predicted" simply by taking account of the observed (marginal) distribution of the respondents' social strata and the corresponding observed distribution for their fathers, and that these two marginal distributions had been sufficient to "explain" the observed mobility patterns. The observed and expected patterns were, in fact, not congruent, but we shall see in the present article that the observed patterns of British and Danish mobility are congruent with what would be expected in the case where there is, in a certain sense, both "quasi-perfect mobility" and "status inheritance" in all strata.

The concept of "status inheritance" is introduced here to describe the situation where an individual's social status is more likely to be the same as his father's status than would be the case if there had been perfect mobility.[4] When there is status inheritance in all strata, we shall introduce the concept of "quasi-perfect mobility" to describe the idealized situation in which, aside from those individuals who have the same status as their fathers (i.e., considering only those individuals who have moved out of their fathers' strata), an individual's own status is, to a certain extent, independent of his father's status.[5] An individual whose status is different from his father's status we shall call a "mover" or a "mobile person." In the quasi-perfect-mobility model for the case where there is status inheritance in all strata, the probability that an individual's status will be the same as his father's status is disproportionately large (so that an individual's status is not independent of his father's status), but this dependence is limited to the extent that a mobile person's chances of moving to the various possible

[3] See, e.g., D. V. Glass, *op. cit.* and Natalie Rogoff, *Recent Trends in Occupational Mobility* (Glencoe, Ill.: Free Press, 1953).

[4] An individual's "social status" refers here to the particular stratum in which his occupation is classified (e.g., the upper, middle, or lower stratum), and "status inheritance" refers to the probability that his stratum will be exactly the same as his father's stratum. Other terms for this concept of "status inheritance" may in some respects be preferable and in other respects not. In any case, the concept we are defining is clear. Status inheritance does not necessarily refer to the inheritance of genetic characteristics, nor does it refer to an individual's literal inheritance of his father's specific occupation, except insofar as these phenomena, in conjunction with other phenomena, may lead to an increase in the probability that an individual's stratum is the same as his father's. Of course, if the class boundaries that are used to define the strata are changed, then the strata will pertain to a different classification of occupations, and the magnitude of the status inheritance will depend upon the particular classification under consideration.

[5] A more precise definition of this model will be given later herein. This model, as defined later, is a rather general one which is not necessarily restricted to the study of situations where there is status inheritance in all strata.

strata are, in a certain sense, unaffected by his father's status.[6]

With respect to the cross-classification tables describing each respondent's social status and his father's status, in the case where there is status inheritance in all strata, the expected pattern of mobility for the movers can be derived for the quasi-perfect-mobility model, and these expectations can be estimated from the observed (marginal) distribution of movers in the different social strata and the corresponding observed distribution for their fathers.[7] We shall see that the observed patterns of mobility for the movers are congruent with the expected patterns assuming quasi-perfect-mobility, and thus the data support the quasi-perfect-mobility hypothesis; a mover's social status appears to be independent (in a certain sense) of his father's status, the observed pattern of mobility for the movers can be "predicted" by taking account of the observed (marginal) distribution of the movers' social strata and the corresponding observed distribution for their fathers, and these two marginal distributions are sufficient to "explain" the observed mobility pattern.[8] Since these two marginal distributions can be calculated from the observed distribution of the respondents in the different social strata, the corresponding distribution for their fathers, and the number of respondents in each stratum who have the same status as their fathers, we shall see that the observed pattern can be "predicted" taking account of the status inheritance in each stratum (and the marginal distributions for the respondents and for their fathers).

In this paper, we shall first reconsider the perfect-mobility model; second, introduce a model for quasi-perfect mobility for the case where there is status inheritance in the upper and lower strata but not in the middle stratum; third, present a model for quasi-perfect mobility for the case where there is status inheritance in all strata; and then introduce other more general quasi-perfect-mobility models. In his earlier paper, White[9] attempted to predict mobility patterns taking into account status inheritance in the upper and lower strata (assuming implicitly that there was no status inheritance in the middle stratum), but the methods he used in his numerical calculations are actually quite different from those presented here. The latter methods are simpler to apply, and

[6] The precise meaning of this sentence will be made clear later. A mover's chances of moving to the various possible strata cannot be, strictly speaking, unaffected by his father's status, since the set of possible strata to which the mover might move is limited at least to the extent that his father's stratum is excluded from the set. In this model, aside from the effects caused directly by the fact that the set of possible strata to which the mover might move is limited in this way, his chances of moving to the various possible strata available to him are affected in no other way by his father's stratum.

[7] In this case the expected frequencies in all of the non-diagonal cells of the contingency table can be estimated. The frequencies in the diagonal cells of the table can be used to measure the magnitude of the status inheritance, but they cannot be "predicted" from the model unless some supplementary assumptions regarding the nature of status inheritance are introduced. The magnitude of the status inheritance will depend upon various factors including, of course, the number of strata used to classify the population. Models that could predict the observed magnitude of the status inheritance would be useful and could supplement the models introduced herein, but we shall not discuss such models here.

[8] This empirical finding holds true for the British and Danish data when three social strata (i.e., upper, middle, and lower) are used to classify the population. It also holds in a certain modified sense, described later, when a larger number of strata are used. Since the classification used is a rather broad one, a variety of occupations will be included in a single stratum; and there will, of course, be some mobility within each stratum, but this will not be considered here. The interpretation of our finding must also be qualified by the limitations of the surveys from which the data were obtained. For a full discussion of these surveys and their limitations, see Glass, *op. cit.*, and Svalastoga, *op. cit.* For further study of related matters, see Gösta Carlsson, *Social Mobility and Class Structure* (Lund, Sweden: CWK Gleerup, 1958); Saburo Yasudo, "A Methodological Inquiry into Social Mobility," *American Sociological Review*, XXIX (1964), 16–23; Otis Dudley Duncan, "Methodological Issues in the Analysis of Social Mobility." (Dittoed manuscrupt.)

[9] Harrison C. White, "Cause and Effect in Social Mobility Tables," *Behavioral Science*, VII (1963), 14–27.

they are more adaptable in the sense that they can be applied more readily to a wider variety of models. We shall also introduce in the present article several statistical measures and tests of the goodness-of-fit between the observed mobility patterns and the expected patterns estimated for the various models presented here; and we shall show that White's predictions differ from the observed data in statistically significant ways, whereas the models proposed here lead to predictions that fit the data well.

THE PERFECT-MOBILITY MODEL

Readers who are familiar with the concept of independence in a contingency table (i.e., a cross-classification table) will see that the perfect-mobility model corresponds to this concept.[10] In this section, we shall describe the perfect-mobility model in a way that will facilitate the modification of this model in later sections in order to obtain the various quasi-perfect-mobility models.[11]

Let us first consider the simple situation where individuals are drawn at random from a (large) population, and where each individual drawn is classified according to his social status (say, upper, middle, or lower) and also according to his father's status (upper, middle, or lower). In this case, the observed data can be summarized in a 3×3 cross-classification table such as Table 1. The upper, middle, and lower strata are denoted in Table 1 by 1, 2, and 3, respectively; and f_{ij} denotes the number of individuals drawn whose fathers were in stratum i ($i = 1, 2, 3$) and who were themselves in stratum j ($j = 1, 2, 3$). Denoting by n the total number of individuals drawn in the sample, we have

$$\sum_{i=1}^{3} \sum_{j=1}^{3} f_{ij} = n .$$

Denoting by $f_{i\cdot}$ the number of individuals drawn whose fathers were in stratum i, and by $f_{\cdot j}$ the number of individuals drawn who were themselves in stratum j, we have

$$\sum_{j=1}^{3} f_{ij} = f_{i\cdot}$$

and

$$\sum_{i=1}^{3} f_{ij} = f_{\cdot j} .$$

TABLE 1

		Subject's Status 1	2	3
Father's Status	1	f_{11}	f_{12}	f_{13}
	2	f_{21}	f_{22}	f_{23}
	3	f_{31}	f_{32}	f_{33}

Denoting by P_{ij} the probability that an individual in the sample will be in stratum j and that his father will be in stratum i, we have

$$\sum_{i=1}^{3} \sum_{j=1}^{3} P_{ij} = 1 , \qquad (1)$$

since an individual in the sample will be in one of the three strata and so will his father.

In the idealized situation where there is "perfect mobility" between the strata (i.e., where an individual's own status is independent of his father's status in the population under study), then the P_{ij} are of the special form

$$P_{ij} = P_i R_j , \qquad (2)$$

where P_i and R_j are positive constants such that[12]

[10] Almost all elementary statistics textbooks discuss the concept of independence in a contingency table and the χ^2 test of the null hypothesis of independence. This is a test of the congruency (i.e., the goodness-of-fit) between the observed pattern of frequencies in the table and the expected pattern derived for the null hypothesis.

[11] Some readers may find the presentation in this section more difficult at first sight than the usual textbook discussions of contingency-table independence, but this kind of presentation should facilitate the later understanding of the modified models.

[12] In the perfect-mobility model, the parameter R_j can be interpreted as the probability that an indi-

$$\sum_{i=1}^{3} P_i = 1$$

and

$$\sum_{j=1}^{3} R_j = 1 \, .$$

The model described by equation (2) states that the probabilities P_{ij} $(i, j = 1, 2, 3)$ can be completely determined by the parameters P_i and R_j associated with the ith row and jth column of the table. In this case, the expected value of the frequency f_{ij} in cell (i, j) can be written as

$$E\{f_{ij}\} = nP_iR_j \, , \qquad (3)$$

and so in order to estimate $E\{f_{ij}\}$ we must first estimate P_i and R_j. The maximum likelihood estimators of P_i and R_j are

$$P'_i = f_{i.}/n \quad \text{and} \quad R'_j = f_{.j}/n \, ,$$

respectively, and from equation (3) we see tnat the maximum likelihood estimator of $E\{f_{ij}\}$ is

$$F_{ij} = nP'_iR'_j = \frac{f_{i.}f_{.j}}{n} \, . \qquad (4)$$

To test the hypothesis of perfect mobility (i.e., to test eq. [2]), we calculate the goodness-of-fit statistic

$$X^2 = \sum_{i=1}^{3}\sum_{j=1}^{3}\frac{(f_{ij} - F_{ij})^2}{F_{ij}} \, . \qquad (5)$$

Statistical theory tells us that when equation (2) holds true, the asymptotic (large-sample) distribution of the statistic (5) will be the χ^2 distribution with $2 \times 2 = 4$ degrees of freedom.

Note that the F_{ij} are based upon the P'_i and R'_j, which are in turn based upon the marginal frequencies $f_{i.}$ and $f_{.j}$. If the F_{ij} are sufficiently close to the corresponding f_{ij}, we would then say that the perfect-mobility model given by equation (2) fits the observed pattern of mobility, and that this pattern can be explained or predicted solely in terms of the marginal frequencies.

Modifications of the statistical methods described above will usually be in order, since the perfect-mobility model will usually not fit the data observed in mobility tables. In other words, if the "predicted values" (i.e., the F_{ij}) based upon the model are not sufficiently close to the corresponding observed values f_{ij}, and this will often be the case, then this model and the methods of analysis suited to it should be modified accordingly. We shall describe below some of the kinds of modifications that seem to be suited to the analysis of mobility tables.

Before closing this section, we take note of a model of perfect mobility which is different from, but closely related to, the one given above. In the situation where equation (2) holds true, the conditional distribution of the f_{ij}, given the marginal frequencies $f_{i.}$ and $f_{.j}$, can be written as follows:[13]

$$\frac{\prod_{i=1}^{3}(f_{i.}!)\prod_{j=1}^{3}(f_{.j}!)}{\left\{(n!)\left[\prod_{i=1}^{3}\prod_{j=1}^{3}(f_{ij}!)\right]\right\}} \, . \qquad (6)$$

This formula also gives us the probability that exactly f_{ij} individuals $(i, j = 1, 2, 3)$ are assigned to stratum j and have fathers who are assigned to stratum i, in the situation where $f_{.j}$ individuals are to be assigned to stratum j, $f_{i.}$ individuals have fathers who are to be assigned to stratum i, and the different ways of assigning the n different individuals (and their fathers) are all equally likely. The model given by formula (6) is for the case where the marginal frequencies are

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vidual will be in stratum j, and the parameter P_i can be interpreted as the probability that his father will be in stratum i. These parameters will also appear later in the quasi-perfect-mobility models and their interpretation there is somewhat similar to, although different in detail from, their interpretation here. The estimators of these parameters, which are presented below for the perfect-mobility models, are actually calculated in a somewhat different way from the estimators of the corresponding parameters in the quasi-perfect-mobility models discussed later.

[13] See, e.g., Alexander M. Mood and Frank A. Graybill, *Introduction to the Theory of Statistics* (2d ed.; New York: McGraw-Hill Book Co., 1963), pp. 315–16.

fixed, whereas the model given by equation (2) is for the case where they are not. The close relationship between expressions (2) and (6) follows from the fact that (6) can be derived from (2) by considering the conditional distribution as noted above. Furthermore, formulas (4) and (5) which were derived for the model given by equation (2) can also be applied directly to the model given by formula (6). When formula (6) holds true, formula (4) gives the expected frequency in cell (i, j), and formula (5) can be used to test the model given by formula (6).

Denoting by $g_{..}$ the total frequency in Table 2, we have

$$g_{..} = \sum_{i=1}^{3} g_{i.} = \Big[\sum_{i=1}^{3} f_{i.}\Big] - [f_{11} + f_{33}] = n - f_{11} - f_{33} . \quad (8)$$

To reflect the fact that the entries g_{11} and g_{33} are blank, we modify the perfect-mobility model given by equation (2) by setting P_{11} and P_{33} equal to zero, rather than to P_1R_1 and P_3R_3 as in equation (2). Denoting by $P^{\dagger}_{ij}$ the modified P_{ij}'s pertaining to Table 2, in order that they continue to sum to one (as in eq. [1] above), we replace equation (2) by

$$P^{\dagger}_{ij} = \begin{cases} 0, & \text{for} \quad i = j = 1 \quad \text{and for } i = j = 3 \\ P_iR_j/\,[1 - P_1R_1 - P_3R_3]\,, & \text{otherwise} . \end{cases} \quad (9)$$

A MODEL THAT TAKES INTO ACCOUNT STATUS INHERITANCE IN THE UPPER AND LOWER STRATA

With respect to the 3 × 3 cross-classification table (Table 1), the perfect-mobility model attempts to predict the cell entries f_{ij} $(i, j = 1, 2, 3)$ for all $3 \times 3 = 9$ cells of the table. We shall now present a model, which is a modification of the perfect-mobility model, that attempts to predict the cell entries for seven out of the nine cells in the table. We shall, for the time being, blank out the cell entries f_{11} and f_{33} and focus our attention upon the remaining seven cell entries in the table.

We replace Table 1 by Table 2. The cell entries g_{11} and g_{33} are blanked out, and we denote this by assigning zero to these cells, while the entries in the remaining seven cells are equal to the corresponding entries in Table 1; that is, $g_{11} = 0$, $g_{33} = 0$, and for the remaining seven cells $g_{ij} = f_{ij}$. Thus, the row and column marginal frequencies in Table 2 can be written as

$$g_{i.} = \sum_{j=1}^{3} g_{ij} = \begin{cases} f_{1.} - f_{11} & \text{for } i = 1 \\ f_{2.} & \text{for } i = 2 \\ f_{3.} - f_{33} & \text{for } i = 3 \end{cases}$$
$$g_{.j} = \sum_{i=1}^{3} g_{ij} = \begin{cases} f_{.1} - f_{11} & \text{for } j = 1 \\ f_{.2} & \text{for } j = 2 \\ f_{.3} - f_{33} & \text{for } j = 3 . \end{cases} \quad (7)$$

The model described by equation (9) states that the $P^{\dagger}_{ij}$ $(i, j = 1, 2, 3)$ are of a special form, and that they can be completely determined by the parameters P_i and R_j pertaining to the model. Note that

$$\sum_{i=1}^{3}\sum_{j=1}^{3} P^{\dagger}_{ij} = \Big\{\Big[\sum_{i=1}^{3}\sum_{j=1}^{3} P_iR_j\Big] - P_1R_1 - P_3R_3\Big\}/[1 - P_1R_1 - P_3R_3] = 1 . \quad (10)$$

A comparison of the cell entries in the seven cells of Table 2 that are not blank with the corresponding predictions based upon the model given by equation (9) will tell us whether or not the data in these seven cells can be fitted by this model of "quasi-perfect" mobility which takes into account the possibility of "status inheritance" in the first and third strata.

TABLE 2

Father's Status	Subject's Status 1	2	3
1	0	g_{12}	g_{13}
2	g_{21}	g_{22}	g_{23}
3	g_{31}	g_{32}	0

Formula (9) can be rewritten as

$$P^{\dagger}_{ij} = \begin{cases} 0, & \text{for} \quad i = j = 1 \quad \text{and for } i = j = 3 \\ S_i T_j, & \text{otherwise}, \end{cases} \tag{11}$$

where S_i and T_j are positive constants which are such that[14]

$$\sum_{i=1}^{3} \sum_{j=1}^{3} P^{\dagger}_{ij} = \left[\sum_{i=1}^{3} \sum_{j=1}^{3} S_i T_j \right] - S_1 T_1 - S_3 T_3 = 1 \,. \tag{12}$$

When the model given by equation (2) is replaced by the model given by equation (9), then the formula for the expected frequency in each cell given by equation (3) must be replaced by

$$E\{g_{ij}\} = \begin{cases} 0, & \text{for} \quad i = j = 1 \quad \text{and for } i = j = 3 \\ g_{..} S_i T_j, & \text{otherwise}, \end{cases} \tag{13}$$

and formula (4) for the maximum likelihood estimator of the expected frequency must be replaced by

$$G_{ij} = \begin{cases} 0, & \text{for} \quad i = j = 1 \quad \text{and for } i = j = 3 \\ g_{..} S'_i T'_j, & \text{otherwise}, \end{cases} \tag{14}$$

where S'_i and T'_j are the appropriate estimators of S_i and T_j, respectively.[15] To test the hypothesis that the model given by equation (9) or (11) holds true, we calculate the goodness-of-fit statistic

$$X^2 = \sum_{i=1}^{3} \sum_{j=1}^{3} \frac{(g_{ij} - G_{ij})^2}{G_{ij}}, \tag{15}$$

where the terms summed in equation (15) are zero for $i = j = 1$ and for $i = j = 3$. Statistical theory tells us that when equation (9) or (11) holds true the large-sample distribution of the statistic (15) will be the χ^2 distribution with $(2 \times 2) - 2 = 2$ degrees of freedom.

The G_{ij} are based upon the S'_i and T'_j, which are in turn based upon the $g_{i.}$ and $g_{.j}$. If the G_{ij} are sufficiently close to the corresponding g_{ij}, we would then say that the model given by equation (9) or (11) fits the observed pattern of mobility in the seven non-zero cells of Table 2, and that this pattern can be explained or predicted solely in terms of the $g_{i.}$ and $g_{.j}$. Since the $g_{i.}$ and $g_{.j}$ can be calculated from $f_{i.}$, $f_{.j}$, and the cell entries f_{11} and f_{33} (see eq. [7]), if the G_{ij} and g_{ij} are congruent we would also say that the observed pattern can be explained solely in terms of the status inheritance in the first and third strata (together with the marginal frequencies $f_{i.}$ and $f_{.j}$).

Before closing this section, we take note of the fact that the problem considered here was studied earlier by White,[16] but the model presented in his paper was different from

[14] The S_i and T_j are somewhat similar to, although different in detail from, the P_i and R_j appearing in equation (2).

[15] The calculation of G_{ij} is a special case of a more general problem discussed in my "A Short Computer Program for the Analysis of Transaction Flows," *Behavioral Science*, IX (1964), 176–86. An efficient method for calculating G_{ij} was described in that article, and it can be applied in the present context. The general method of estimation presented in that article can be applied not only to calculate G_{ij} and the estimators S'_i and T'_j for the particular quasi-perfect-mobility model presented in this section, but it also can be applied to calculate the estimators of the corresponding parameters for the other models discussed in the present article. The actual calculations will be different for the different models, but the general approach is the same.

[16] *Ibid.*

the one given above and the formulas he used in his numerical calculations were also quite different from those given here. His model can, however, be derived from the one given here by considering the conditional distribution of the g_{ij}, when the marginal frequencies $g_{i.}$ and $g_{.j}$ are given, in the case where equation (9) holds true. The formula for this conditional distribution also gives the probability that exactly g_{ij} individuals ($i, j = 1, 2, 3$) are assigned to stratum j and have fathers who are assigned to stratum i; in the situation where no individuals who are to be assigned to stratum 1 have fathers who are to be assigned to stratum 1 and no individuals who are to be assigned to stratum 3 have fathers who are to be assigned to stratum 3, and where $g_{.j}$ individuals are to be assigned to stratum j, $g_{i.}$ individuals have fathers who are to be assigned to stratum i, and the different ways of assigning the $g_{..}$ different individuals (and their fathers) are all equally likely. The relationship between White's model and the one given here by equation (9) corresponds to the relationship described in the preceding section between the models given by equations (6) and (2). As we noted in the preceding section, the methods presented here for the case where the marginal frequencies are not fixed (i.e., for the model given by eq. [9]) can also be applied to the case where the marginal frequencies are fixed. In particular, the statistic (15) can be used as a large-sample test of the models both where the marginal frequencies are fixed and where they are not. Furthermore, the methods presented here have the advantage that they are simpler to apply than the methods used by White in his calculations, and they are also more adaptable in the sense that they can be applied more readily to a wider variety of models.

TABLE 3

CROSS-CLASSIFICATION OF BRITISH AND DANISH MALE SAMPLES ACCORDING TO EACH SUBJECT'S STATUS CATEGORY AND HIS FATHER'S STATUS CATEGORY

		British Subject's Status		
		U	M	L
Father's Status	U	588	395	159
	M	349	714	447
	L	114	320	411

		Danish Subject's Status		
		U	M	L
Father's Status	U	685	280	118
	M	232	348	198
	L	83	201	246

TWO EXAMPLES: SOCIAL MOBILITY IN BRITAIN AND IN DENMARK

Table 3 provides cross-classifications of male samples in Britain and in Denmark according to each subject's occupational status category and his father's occupational status category. These data were reported for 3,497 males in the British study, and for 2,391 males in the Danish study. The upper-, middle-, and lower-status categories used for the British data in Table 3 correspond to the occupational status categories 1–4, 5, and 6–7, respectively, as defined by Glass and his co-workers; for the Danish data they correspond to the occupational status categories 1–6, 7, 8–9, respectively, as defined by Svalastoga.[17]

We can apply the methods given in the preceding section to try to "predict" the expected frequencies in seven of the cells in each part of Table 3. These expected frequencies are given in Table 4.

The model described in the preceding section corresponds to the hypothesis that, except for the possibility of status inheritance in the upper and lower strata, there is perfect mobility between the strata. To test this hypothesis with respect to the British and Danish data, we compare the expected fre-

[17] The status categories in Table 3 were used also in part of White's analysis (*op. cit.*). For further details about these categories, see the earlier literature.

quencies in the seven non-asterisked cells of each part of Table 4 with the corresponding observed frequencies in Table 3. The χ^2 goodness-of-fit statistic (15) will give $X^2 = 19.2$ for the British data and $X^2 = 12.6$ for the Danish data. We noted earlier that the asymptotic distribution of this statistic is the χ^2 distribution with 2 degrees of freedom, when the hypothesis under consideration is true. Since the ninety-ninth percentile for this χ^2 distribution is 9.21, values of $X^2 = 19.2$ and $X^2 = 12.6$ calculated for the data in Table 3 should lead to the rejection of this hypothesis as tested by these data.

Since the cross-classification data in Table 3 summarize the observations made on samples of males in Britain and Denmark, the χ^2 test tells us that, even if we take into account the sampling variability, the observed patterns of mobility differ so much from the patterns predicted by the hypothesis under consideration (i.e., by the model described by eqs. [9] or [11]) that we should reject this hypothesis as tested by these data.[18] If, on the other hand, these cross-classified data are thought of as having fixed marginal frequencies, and if the model suggested by White is under consideration, then the χ^2 test tells us that, even if we take into account the full range of possible mobility patterns which might occur with this probability model, the observed patterns differ so much from the patterns predicted by this model that we should also reject this model as tested by these data.[19] Thus, in the present context, our conclusion is the same when the sampling variability of the marginal frequencies is ignored (i.e., when the marginal frequencies are considered fixed) as when sampling variability is given full consideration: we reject the hypothesis that, aside from the possibility of status inheritance in the upper and lower strata, there is perfect mobility between the two strata. We shall show in a later section that the goodness-of-fit statistics can be reduced from the values $X^2 = 19.2$ and $X^2 = 12.6$ that we have just obtained to $X^2 = 0.6$ and $X^2 = 0.8$, respectively, when the hypothesis under consideration is modified to take into ac-

TABLE 4

PREDICTED MOBILITY PATTERNS FOR BRITISH AND DANISH SAMPLES WHEN STATUS INHERITANCE IN UPPER- AND LOWER-STATUS CATEGORIES IS TAKEN INTO ACCOUNT

		British Subject's Status		
		U	M	L
Father's Status	U	588*	360.2	193.8
	M	331.8	766.0	412.2
	L	131.2	302.8	411*

		Danish Subject's Status		
		U	M	L
Father's Status	U	685*	267.4	130.6
	M	212.9	379.6	185.4
	L	102.1	181.9	246*

* Used in the calculation of the predicted entries in the remaining seven cells.

[18] The data in Table 3 were obtained by a kind of stratified random sampling described by Glass and his co-workers and by Svalastoga. Strictly speaking, the χ^2 test should be applied to data obtained by simple random sampling, but we use it here as an approximate gauge. When this test is replaced by a test of the hypothesis under consideration based on the corresponding likelihood-ratio statistic, the results obtained are in fact very similar to those presented here. (See related results in the section below in which a likelihood-ratio statistic is used.) Other methods of analysis, which take more fully into account the particular kind of sampling used, could be developed, but this we leave to future research.

[19] Note that if the marginal frequencies are considered to be fixed, then statistical inferences based upon these data pertain, strictly speaking, only to the situation where these particular marginal frequencies hold true. In order to make inferences about the whole population, White assumed in his paper that the marginal proportions calculated for each table were equal to the corresponding proportions for the whole population of the country. This assumption ignores the possible distortions in the marginal frequencies that might occur as a result of non-random errors (non-response errors, coding errors, etc.) as well as the possible distortions that might occur as a result of random variation.

count status inheritance in the middle stratum.

Considering only those individuals in the British part of Table 3 who fall into the seven cells with entries which we have tried to predict, if we calculate the proportion of these individuals who actually fall into each of these seven cells and then compare these seven proportions with the corresponding predicted proportions calculated from the seven cell entries in Table 4, we find that 4.2 per cent of these individuals would have to be moved from the cell in which they have been classified to some other cell in the table in order to make the observed proportions equal to the predicted proportions.[20] For those individuals in the Danish part of Table 3 who fall into the seven cells whose entries we have tried to predict, the corresponding percentage is 4.3. These percentages measure, in a sense, the over-all magnitude of the discrepancies between the observed and predicted proportions, and they will be considered small or large depending upon the standards to which they are to be compared. We shall show in a later section that these measures can be reduced from 4.2 and 4.3 per cent to 0.8 and 1.3 per cent, respectively, when the hypothesis under consideration is modified to take into account status inheritance in the middle stratum.

In interpreting the findings presented here and those presented earlier by White, one should keep in mind that the data analyzed, like all survey data, may be subject to non-random errors (non-response errors, coding errors, etc.)[21] as well as to random variation. In the present article, we take into account the possible effects of random variation with our introduction of the χ^2 goodness-of-fit statistic (in this and the preceding section) and the likelihood-ratio statistic (in a later section), but we do not explicitly take into account non-random error. (Neither the effects of non-random errors nor the effects of random variation were taken into account in the calculations presented by White.) The possible effects of non-random error are difficult to assess. If it were possible to remove completely the various sources of non-random error, would there be a reduction or an increase in the discrepancies between the observed and predicted proportions presented herein? Unfortunately, at the present time not enough is known about non-random error in these kinds of surveys to answer this question.[22]

A MODEL THAT TAKES INTO ACCOUNT STATUS INHERITANCE IN ALL STRATA

We shall now present a model, which is a modification of the perfect-mobility model, that attempts to predict the cell entries for six out of nine cells in the mobility table. We shall, for the time being, blank out the diagonal cell entries, f_{11}, f_{22}, and f_{33} and shall focus our attention on the remaining six cell entries in the table. We replace Table 1 by Table 5, in which the cell entries h_{11}, h_{22}, and h_{33} are blanked out. We denote this by assigning zero to these cells, while the entries in the remaining six cells are equal to the corresponding entries in Table 1, that is, $h_{ij} = 0$ for $i = j$, and $h_{ij} = f_{ij}$ for $i \neq j$. Thus, the row and column marginal frequencies in Table 5 can be written as

[20] White states in his paper (p. 25) that 8 per cent of the individuals in the seven cells of the British part of Table 3 herein would have to be moved from the cell in which they have been classified to some other cell in the table in order to make the observed proportions equal to his predicted proportions. Further calculation indicates that White's result should have been similar in this case to ours, and that a factor of one-half was overlooked in his computations. The value of 4.2 per cent is obtained by calculating the difference between the observed and predicted proportions in the seven cells, noting which of these differences are positive, and then taking the sum of the positive differences; it can also be obtained by taking one-half the sum of the absolute values of the seven differences.

[21] For some discussion of the magnitude of these kinds of errors, see Glass, *op. cit.*, and Svalastoga, *op. cit.*

[22] This question is relevant both where the marginal frequencies are not considered fixed and where they are considered fixed. The possible effects of non-random error on the observed marginal frequencies, when these frequencies are considered fixed, together with the possible effects of random errors in these frequencies, may lead to erroneous inferences concerning the total population under consideration.

$$h_{i\cdot} = \sum_{j=1}^{3} h_{ij} = f_{i\cdot} - f_{ii}, \quad \text{for } i = 1, 2, 3, \tag{16}$$

$$h_{\cdot j} = \sum_{i=1}^{3} h_{ij} = f_{\cdot j} - f_{jj}, \quad \text{for } j = 1, 2, 3.$$

Denoting by $h_{\cdot\cdot}$ the total frequency in Table 5, we have

$$h_{\cdot\cdot} = \sum_{i=1}^{3} h_{i\cdot} = n - \sum_{i=1}^{3} f_{ii}. \tag{17}$$

To reflect the fact that the entries f_{ii} ($i = 1, 2, 3$) are blank, we modify the perfect-mobility model (given by eq. [2]) by setting P_{ii} equal to zero, rather than to

TABLE 5

		Subject's Status 1	2	3
Father's Status	1	0	h_{12}	h_{13}
	2	h_{21}	0	h_{23}
	3	h_{31}	h_{32}	0

$P_i R_i$. Denoting by $P^{\ddagger}_{ij}$ the modified P_{ij}'s pertaining to Table 5, in order that they continue to sum to one, we replace equation (2) by

$$P^{\ddagger}_{ij} = \begin{cases} 0, & \text{for } i = j \\ P_i R_j \Big/ \left[1 - \sum_{k=1}^{3} P_k R_k\right], & \text{for } i \neq j. \end{cases} \tag{18}$$

The model described by equation (18) states that the $P^{\ddagger}_{ij}$ ($i, j = 1, 2, 3$) are of a special form and that they can be completely determined by the parameters P_i and R_j pertaining to the model. Formula (18) will be used to determine whether or not the observed pattern of mobility can be fitted by this model of "quasi-perfect" mobility which takes into account the possibility of "status inheritance" in the three strata.

Formula 18 can be rewritten as

$$P^{\ddagger}_{ij} = \begin{cases} 0, & \text{for } i = j \\ U_i V_j, & \text{for } i \neq j, \end{cases} \tag{19}$$

where U_i and V_j are positive constants which are such that

$$\sum_{i=1}^{3} \sum_{j=1}^{3} P^{\ddagger}_{ij} = \left[\sum_{i=1}^{3} \sum_{j=1}^{3} U_i V_j\right] - \sum_{i=1}^{3} U_i V_i = 1. \tag{20}$$

For the model given by equation (18) or (19), the formula for the expected frequency in each cell is

$$E\{h_{ij}\} = \begin{cases} 0, & \text{for } i = j \\ h_{\cdot\cdot} U_i V_j, & \text{for } i \neq j, \end{cases} \tag{21}$$

and the maximum likelihood estimator of $E\{h_{ij}\}$ is

$$H_{ij} = \begin{cases} 0, & \text{for } i = j \\ h_{\cdot\cdot} U'_i V'_j, & \text{for } i \neq j, \end{cases} \tag{22}$$

where U'_i and V'_j are the appropriate estimators of U_i and V_j, respectively.[23] To test the hypothesis that the model given by equation (18) or (19) holds true, we calculate the goodness-of-fit statistic

$$X^2 = \sum_{i=1}^{3} \sum_{j=1}^{3} \frac{(h_{ij} - H_{ij})^2}{H_{ij}}, \tag{23}$$

[23] A method for determining the U'_i and V'_j is given in Leo A. Goodman, *op. cit.* For closely related results, see I. Richard Savage and Karl W. Deutsch, "A Statistical Model for the Gross Analysis of Transaction Flows," *Econometrica*, XXVIII (1960), 551–72. The iterative procedure given by Savage and Deutsch for calculating H_{ij} will, however, not lead to the desired estimates; corrections and modifications of this procedure are given in my "Statistical Methods for the Preliminary Analysis of Transaction Flows," *Econometrica*, XXXI (1963), 197–208. When compared with the Savage-Deutsch iterative procedure, the corresponding procedure introduced in my "A Short Computer Program for the Analysis of Transaction Flows," *op. cit.*, leads to a reduction of approximately 62 per cent in the total number of arithmetic operations (additions, divisions, etc.) required in the iterative steps.

where the terms summed in equation (23) are zero for $i = j$. When equation (18) or (19) holds true, then the large-sample distribution of the statistic (23) will be the χ^2 distribution with $(2 \times 2) - 3 = 1$ degree of freedom.

The H_{ij} as given by equation (22) are based upon the U'_i and V'_j, which are in turn based upon the $h_{i.}$ and $h_{.j}$. The $h_{i.}$ and $h_{.j}$ can be calculated from $f_{i.}$, $f_{.j}$, and the cell entries f_{ii} ($i = 1, 2, 3$), which pertain to status inheritance in the three strata (see eq. [16]). Thus, if the H_{ij} are sufficiently close to the h_{ij}, we would then say that the observed pattern of mobility can be predicted solely in terms of the status inheritance in the three strata (together with the marginal frequencies $f_{i.}$ and $f_{.j}$).

The techniques presented in this section can be used to analyze the situation where there is both quasi-perfect mobility and status inheritance (and/or status disinheritance) in the different strata. Applying the definition of status inheritance introduced earlier in this article, we shall see in the following section that the data analyzed here provide examples of the situation where there is both quasi-perfect mobility and status inheritance in all strata. Since three strata are used to describe the classification of occupations in the data analyzed in the following section (and five strata are used in a later section), these data provide examples of status inheritance within and quasi-perfect mobility across broad class boundaries. If the class boundaries are changed, different results may be obtained. For the sake of completeness, we also note here that if a somewhat different definition of status inheritance and disinheritance were to be introduced, the data would provide examples of both status inheritance and status disinheritance in a certain special sense (even when the class boundaries are not changed). But this is beyond the scope of the present article. It will be discussed in a separate report.

THE TWO EXAMPLES RECONSIDERED

Applying the methods described in the preceding section to the data given in Table 3, we obtain Table 6.

The model described by equation (18) or (19) corresponds to the hypothesis that, except for the possibility of status inheritance in the three strata, there is perfect mobility between the strata. To test this hypothesis with respect to the British and Danish data, we compare the expected frequencies in the six non-asterisked cells of each part of Table 6 with the corresponding observed frequencies in Table 3. The χ^2 goodness-of-fit statistic (23) applied to these data will give $X^2 = 0.6$ for the British data and $X^2 = 0.8$ for the Danish data. We noted earlier that the large-sample distribution of this statistic is the χ^2 distribution with one degree of freedom, when the hypothesis under consideration is true. Since the expected value of this χ^2 distribution is one, values of $X^2 = 0.6$ and $X^2 = 0.8$ calculated for the data in Table 3 should lead to the acceptance of this hypothesis as tested by these data.

TABLE 6

PREDICTED MOBILITY PATTERNS FOR BRITISH AND DANISH SAMPLES WHEN STATUS INHERITANCE IN THREE STATUS CATEGORIES IS TAKEN INTO ACCOUNT

		British Subject's Status					Danish Subject's Status		
		U	M	L			U	M	L
Father's Status	U	588*	390.2	163.8	Father's Status	U	685*	284.7	113.3
	M	353.8	714*	442.2		M	227.3	348*	202.7
	L	109.2	324.8	411*		L	87.7	196.3	246*

* Used in the calculation of the predicted entries in the remaining six cells.

Considering only those individuals in the British part of Table 3 who fall into the six cells with entries which we have now tried to predict, if we calculate the proportion of these individuals who actually fall into each of these six cells and then compare these six proportions with the corresponding predicted proportions calculated from the six cell entries in Table 6, we find that 0.8 per cent of these individuals would have to be moved from the cell in which they have been classified to some other cell in the table in order to make the observed proportions equal to the predicted proportions. The corresponding percentage for the Danes is 1.3.

In this section, we have attempted to predict the entries in six of the cells in each table, whereas in the earlier section we had attempted to predict the entries in seven of the cells in each table. Comparing the percentages 0.8 and 1.3, which measure the over-all magnitude of the discrepancies between the observed and predicted proportions in the six cells in each part of Table 3, with the corresponding percentages 4.2 and 4.3 obtained in the earlier section for the seven cells in each part, we see that the predictions made in the present section are more accurate than those made earlier. For each table, the predictions made earlier had two degrees of freedom, whereas the predictions made in the present section have one degree of freedom. Comparing the chi-square values $X^2 = 0.6$ and $X^2 = 0.8$ (each with one degree of freedom) obtained in the present section for the six cells of each table with corresponding values $X^2 = 19.2$ and $X^2 = 12.6$ (each with two degrees of freedom) obtained in the earlier section for the seven cells of each table, we see that the model presented in the present section actually fits the data well, whereas the model presented earlier does not.[24]

If the model tested in the earlier section had been correct (i.e., if mobility between the strata had been perfect, except for status inheritance in the upper and lower strata), then this would have implied that there was independence between the row and column classifications in both the 2×2 table formed from Table 2 by considering only the four cell entries g_{12}, g_{13}, g_{22}, g_{23}, and in the 2×2 table formed by considering only the four cell entries g_{21}, g_{22}, g_{31}, g_{32}. Thus, as a test of that model we could have analyzed separately these 2×2 tables and applied the usual contingency-table χ^2 test of independence to each of the 2×2 tables. (A method for combining the separate χ^2 tests calculated in this way could be devised, but we shall not go into these details here.) This is a much easier procedure to use than the test of that model suggested in the earlier section (i.e., the test based on eqs. [14] and [15]), and it would serve as a good first step in the analysis. (The method based on eqs. [14] and [15] has the advantage that the estimated expected frequencies G_{ij} for the seven non-zero entries in Table 2 are the maximum likelihood estimates, whereas the method based upon the separate 2×2 table χ^2 tests does not use all of the information available in calculating estimates of the corresponding expected frequencies for these seven entries; and each 2×2 table χ^2 test will yield a different estimate of the expected value of g_{22}.) Applying the separate 2×2 table χ^2 tests to both the British and Danish data, we find, for example, highly significant χ^2 values for the 2×2 table based on g_{12}, g_{13}, g_{22}, g_{23} for the British data, and for the 2×2 table based on g_{21}, g_{22}, g_{31}, g_{32} for the Danish data. Contrary to what would be expected on the basis of the model tested in the earlier section, if we compare those individuals in the British sample whose fathers were in the upper stratum but who are themselves in either the middle or lower stratum (call these individuals group I) with those individuals whose fathers were in the middle stratum and who are themselves in either the middle or lower stratum (call these individuals group II), the probability that an individual in group I will be in the middle stratum is greater than the corresponding probability for group II. (On the basis of the model tested in the earlier section, these

[24] In a later section we shall introduce somewhat different methods for comparing the fit obtained with these two models. Applying these additional methods to these data, we shall see that the results presented here are corroborated.

probabilities should have been equal.) Similarly, if we compare those individuals in the Danish sample whose fathers were in the lower statum but who are themselves in either the middle or upper stratum (call these individuals group III) with those individuals whose fathers were in the middle stratum and who are themselves in either the middle or upper stratum (call these individuals group IV), the probability that an individual in group III will be in the middle stratum is greater than the corresponding probability for group IV. (On the basis of the model tested in the earlier section, these probabilities should also have been equal.) Though the British and Danish data contradict the model tested in the earlier section in these ways, *they do not contradict the model* which we have tested in the present section.

A GENERAL METHOD OF ANALYSIS

The results discussed in the preceding sections were concerned with the situation where there are exactly three different status categories. We shall now consider the more general situation where there are K different status categories ($K \geq 3$). In this case, the observed data can be summarized in Table 7, where f_{ij} denotes the number of individuals whose fathers were in stratum i ($i = 1, 2, \ldots, K$) and who were themselves in stratum j ($j = 1, 2, \ldots, K$).

Suppose that we focus attention upon a particular subset of the $K \times K$ cells in Table 7, a subset consisting of, say, L cells; the entries in the remaining $K \times K - L$ cells are to be blanked out. We shall assume that $L \geq 2K$. (Earlier we studied cases where L was 7 and 6, and where K was 3.) We replace the entries in the $K \times K -$ L cells which are to be blanked out by zeros. Let us assume that the entries in the L cells upon which we focus attention are all positive. We thus replace Table 7 by a modified $K \times K$ table where the entry g_{ij} in cell (i, j) in the ith row and the jth column is zero if cell (i, j) is among the $K \times K - L$ cells which are to be blanked out; and it is equal to f_{ij} if cell (i, j) is among the L cells upon which attention is to be focused.

Let Σ^* denote summation over all i, j where $g_{ij} \neq 0$. Denoting by $P^\dagger_{ij}$ the probability associated with cell (i, j) in the (modified) table of the g_{ij}'s, for those values of i, j where $g_{ij} = 0$ we set

$$P^\dagger_{ij} = 0\,, \tag{24}$$

and for those values of i, j where $g_{ij} \neq 0$ we set

$$P^\dagger_{ij} = P_i R_j / \sigma\,, \tag{25}$$

where

$$\sigma = \Sigma^* P_i R_j\,.$$

Formulas (9) and (18) are special cases of formulas (24) and (25). The model described by equations (24) and (25) states that the $P^\dagger_{ij}$ are of a special form, and that they can be completely determined by the parameters P_i and R_j of the model. We can rewrite equations (24) and (25) as

$$P^\dagger_{ij} = \begin{cases} 0, & \text{for } (i, j) \quad \text{where } g_{ij} = 0 \\ S_i T_j, & \text{otherwise}\,, \end{cases} \tag{26}$$

where S_i and T_j are positive constants which are such that

$$\Sigma^* S_i T_j = 1\,.$$

Formulas (11) and (19) are special cases of equation (26).

TABLE 7

		Subject's Status						
		1	2	3				K
	1	f_{11}	f_{12}	f_{13}	.	.	.	f_{1K}
	2	f_{21}	f_{22}	f_{23}	.		.	f_{2K}
Father's Status	3	f_{31}	f_{32}	f_{33}	.	.	.	f_{3K}
	.	.	.	.			.	.
	.	.	.	.			.	.
	.	.	.	.			.	.
	K	f_{K1}	f_{K2}	f_{K3}	.	.	.	f_{KK}

For the model given by equations (24) and (25) or equation (26), the formula for the expected frequency in each cell of the table is

$$E\{g_{ij}\} = \begin{cases} 0, & \text{for } (i,j) \quad \text{where } g_{ij} = 0 \\ g_{..}S_iT_j, & \text{otherwise}, \end{cases} \tag{27}$$

where $g_{..}$ is defined here as

$$\sum_{i=1}^{K}\sum_{j=1}^{K} g_{ij} = \Sigma^* g_{ij} .$$

The maximum likelihood estimator of $E\{g_{ij}\}$ is

$$G_{ij} = \begin{cases} 0, & \text{for} \quad (i,j) \quad \text{where } g_{ij} = 0 \\ g_{..}S'_iT'_j, & \text{otherwise}, \end{cases} \tag{28}$$

where S'_i and T'_j are the appropriate estimators of S_i and T_j, respectively. Procedures for calculating G_{ij} are described in an earlier article by the present author.[25]

To test the hypothesis that the model given by equations (24) and (25) or equation (26) holds true, we calculate the goodness-of-fit statistic

$$X^2 = \Sigma^* \frac{(g_{ij} - G_{ij})^2}{G_{ij}}. \tag{29}$$

When this hypothesis holds true, the large-sample distribution of the statistic (29) will be the χ^2 distribution with $L - 2K + 1$ degrees of freedom.[26]

MULTISTRATA MODELS: FURTHER ANALYSIS OF BRITISH SOCIAL-MOBILITY DATA

The British survey referred to earlier actually used seven different occupational status categories.[27] One part of White's analysis was based upon a grouping of these seven status categories into three broader categories. (This same grouping was used earlier herein.) A second part of White's analysis was based upon the seven ungrouped categories. To illustrate the application of the methods presented in the preceding section, we shall reanalyze these data grouping the seven categories into five somewhat broader status categories. We do not use the seven ungrouped categories, since the number of males studied in the survey was too small to place much reliance upon the entries obtained in some of the cells of the 7 × 7 cross-classification table (with its forty-nine cells).[28] We do not use a grouping of the seven categories into six categories for similar reasons. The status categories A, B, C, D, and E used in Table 8 correspond to the categories 1–3, 4, 5, 6, and 7, respectively, as defined earlier by Glass and his co-workers.

The results presented by White would suggest that the pattern of mobility in this table can be explained or predicted if we take into account the possibility of status inheritance in the upper two strata (i.e., if we blank out the entries in the four cells

[25] "A Short Computer Program for the Analysis of Transaction Flows," *op. cit.*

[26] The significance levels determined from the tabulated χ^2 distribution will indicate (for large samples) the probability of errors of the first type. When the sample size is large enough so that the probability of errors of the second type, as well as errors of the first, can be small, then the model under study would be confirmed by the data if the observed X^2 is sufficiently small that it does not lead to rejection of the hypothesis. The risk of errors of the first type can be determined from the tabulated χ^2 distribution if the particular model to be tested is specified before the actual data are studied. If the model is chosen on the basis of the data, this will, of course, affect the risks associated with the test.

[27] These status categories are discussed in detail by Glass and his co-workers, *op. cit.*

[28] The frequency in two of the cells was 3; the frequency in one of the cells was 2; and the frequency in two of the cells was zero.

[A, A], [A, B], [B, A], [B, B]) and in the lower two strata (i.e., if we blank out the entries in the four cells [D, D], [D, E], [E, D], [E, E]). If this were the case, it would imply that, aside from the entries in the eight cells referred to above, there is perfect mobility between the strata. We shall now re-examine this thesis.

Applying the methods given in the preceding section in order to study this thesis, we blank out the entries in the eight cells (A, A), (A, B), (B, A), (B, B), (D, D), (D, E), (E, D), (E, E) of Table 8, and we focus attention upon the remaining seventeen cells of the 5 × 5 table. The methods in the preceding section can be applied directly to the modified 5 × 5 table, or equivalently to a modified 3 × 3 table to which the 5 × 5 table can be reduced since the eight cells which are to be ignored form two square blocks of cells. (The expected frequencies in the modified 3 × 3 table formed by grouping category A with B and category D with E can be used to determine the expected frequencies in the cells of the modified 5 × 5 table, by allocating the modified 3 × 3 table expected frequencies to the cells of the corresponding block in the 5 × 5 table in proportion to the product of the corresponding row and column marginal frequencies [of the modified 5 × 5 table] associated with each cell in the block.)

We shall compare the expected frequencies in the seventeen non-asterisked cells of Table 9 with the corresponding observed frequencies in Table 8, in order to test the hypothesis that, aside from the possibility of status inheritance in the two upper strata and in the two low strata, there is perfect mobility between the strata. The χ^2 goodness-of-fit statistic (29) applied to these tables gives a value of $X^2 = 26.7$. We noted earlier that the asymptotic distribution of this statistic is the χ^2 distribution with $17 - 10 + 1 = 8$ degrees of freedom, when the hypothesis under consideration is true. Since the ninety-ninth percentile for this chi-square distribution is 20.1, a value of

TABLE 8

CROSS-CLASSIFICATION OF BRITISH MALE SAMPLE ACCORDING TO EACH SUBJECT'S STATUS CATEGORY AND HIS FATHER'S STATUS CATEGORY

		Subject's Status				
		A	B	C	D	E
Father's Status	A	297	92	172	37	26
	B	89	110	223	64	32
	C	164	185	714	258	189
	D	25	40	179	143	71
	E	17	32	141	91	106

TABLE 9

PREDICTED MOBILITY PATTERN FOR BRITISH SAMPLE WHEN STATUS INHERITANCE IN TWO UPPER AND TWO LOWER STRATA IS TAKEN INTO ACCOUNT

		Subject's Status				
		A	B	C	D	E
Father's Status	A	297*	92*	152.8	48.7	33.5
	B	89*	110*	207.4	66.1	45.5
	C	147 6	184.2	766 0	244.2	168.0
	D	32.8	40.9	170.2	143*	71*
	E	25.6	31.9	132.6	91*	106*

* Used in the calculation of the predicted entries in the remaining seventeen cells of the table. Strictly speaking, the sum of the four asterisked entries in the upper left corner and the sum of the four asterisked entries in the lower left corner of the table were the quantities used (together with the marginal frequencies) in the calculation of the predicted entries in the remaining cells.

$X^2 = 26.7$ calculated for the data in Table 8 should lead to the rejection of this hypothesis as tested by these data.

We shall now modify the hypothesis just considered by taking into account the possibility of status inheritance in the middle stratum, in addition to the status inheritance in the two upper and the two lower strata. Applying the methods given in the preceding section in order to study this modified hypothesis, we blank out the entry in cell (C, C) of Table 8, in addition to the entries in the eight cells blanked out above, and focus attention upon the remaining sixteen cells of the 5 × 5 table. (Here, too, the methods in the preceding section can be applied to the modified 5 × 5 table, or equivalently to a modified 3 × 3 table to which the 5 × 5 table can be reduced.) We obtain the predictions presented in Table 10.

TABLE 10

PREDICTED MOBILITY PATTERN FOR BRITISH SAMPLE WHEN STATUS INHERITANCE IN TWO UPPER STRATA, TWO LOWER STRATA, AND MIDDLE STRATUM IS TAKEN INTO ACCOUNT

		Subject's Status				
		A	B	C	D	E
Father's Status	A	297*	92*	165.5	41.2	28.3
	B	89*	110*	224.7	55.9	38.5
	C	157.4	196.4	714*	261.9	180.2
	D	27.3	34.1	182.6	143*	71*
	E	21.3	26.5	142.2	91*	106*

* Used in the calculation of the predicted entries in the remaining sixteen cells of the table. Strictly speaking, the sum of the four asterisked entries in the upper left corner, the sum of the four asterisked entries in the lower right corner, and the asterisked entry in the center of the table were the quantities used (together with the marginal frequencies) in the calculation of the predicted entries in the remaining cells.

We shall compare the expected frequencies in the sixteen non-asterisked cells of Table 10 with the corresponding observed frequencies in Table 8 in order to test the hypothesis that, aside from status inheritance in the two upper strata, in the two lower strata, and in the middle stratum, there is perfect mobility between the strata.[29] The χ^2 goodness-of-fit statistic (29) applied to these tables gives a value of $X^2 = 7.9$. The asymptotic distribution of this statistic is the χ^2 distribution with $16 - 10 + 1 = 7$ degrees of freedom, when the hypothesis under consideration is true. Since the expected value of this χ^2 distribution is seven and the ninety-ninth percentile is 18.5, a value of $X^2 = 7.9$ calculated for the data in Table 8 should lead to acceptance of this hypothesis as tested by these data.

In this section, we have attempted to predict the entries in a subset of the cells in Table 8, first taking account of status inheritance in the two upper strata and in the two lower strata (ignoring status inheritance in the middle stratum), and then taking account of status inheritance in all strata (i.e.,

[29] The term "status inheritance in the two upper (lower) strata," as used here, refers to those individuals who were in one of the two upper (lower) strata and whose fathers were also in one of these two strata. Thus the frequencies in the cells (A, B), (B, A), (D, E), and (E, D), together with the five diagonal cells, refer to "status inheritance in the two upper strata, in the two lower strata, and in the middle stratum"; and for short we use the term "status inheritance in all strata" to describe this. The quasi-perfect-mobility model introduced here in order to take into account "status inheritance in all strata" (i.e., status inheritance in the two upper strata, the two lower strata, and the middle stratum) is quite different from the model which takes into account status inheritance only in the five diagonal cells; the latter model does not fit the data as well as the former. The precise meaning of the former model should be kept in mind when interpreting the empirical findings presented here.

the middle stratum in addition to the two upper and the two lower strata). Comparing the χ^2 value $X^2 = 26.7$ (with 8 degrees of freedom) obtained in the first case with the value $X^2 = 7.9$ (with 7 degrees of freedom) obtained in the second case, we conclude that status inheritance in the middle stratum should not be ignored.[30]

If the model tested in the first case had been correct (i.e., if mobility between the strata had been perfect, except for status inheritance in the two upper strata and in the two lower strata), this would have led to the implication that the row and column classifications were independent of each other in the following two tables formed from the population tableau corresponding to Table 8: (*a*) the 3×3 table obtained by considering only the nine cell entries in rows A, B, C, which are also in columns C, D, E; and (*b*) the 3×3 table obtained by considering only the nine cell entries in rows C, D, E, which are also in columns A, B, C. A straightforward analysis of the two 3×3 tables (corresponding to [*a*] and [*b*]) formed from Table 8 would have indicated, however, that this implication was not confirmed by the data. This analysis could have been based on the usual contingency-table χ^2 test of independence applied to each of these 3×3 tables. (See remarks above about the application of the contingency-table χ^2 test to separate subtables formed from the larger table.) Applying the separate 3×3 table χ^2 tests to these data, we find, for example, a highly significant χ^2 value for the 3×3 table corresponding to (*a*). Contrary to what would be expected on the basis of this model (where status inheritance in the middle stratum is ignored), if we consider those individuals in the British sample whose fathers were in stratum A but who are themselves in either C, D, or E (call these individuals group α), those individuals whose fathers were in stratum B but who are themselves in either C, D, or E (call these individuals group β), and those individuals whose fathers were in stratum C and who are themselves in either C, D, or E (call these individuals group γ), then the probability that an individual in group γ will be in stratum C is less than the corresponding probabilities for group α and for group β. (On the basis of this model, these probabilities should have been equal.) Thus the data contradict this model. On the other hand, the data do not contradict the model in which status inheritance in the middle stratum is not ignored. This model leads to the implication that the row and column classifications are independent of each other in the following four tables formed from the population tableau corresponding to Table 8: (*a*) the 2×3 table obtained by considering only the six cell entries in rows A and B which are also in columns C, D, E; (*b*) the 3×2 table obtained by considering the six cell entries in rows A, B, C, which are also in columns D and E; (*c*) the 2×3 table obtained by considering the six cell entries in rows D and E which are also in columns A, B, C; (*d*) the 3×2 table obtained by considering the six cell entries in rows C, D, E, which are also in columns A and B. An analysis of the corresponding four tables formed from Table 8 would indicate that this implication is supported by the data.

We have seen that the data presented herein are congruent with the expected values derived from the quasi-perfect-mobility model which takes into account status inheritance in every stratum (in the two upper strata, the two lower strata, and the middle stratum). The use of this model, when applicable, would lead to various conclusions among which we list the following: (*a*) If we compare those individuals in the British sample whose fathers were in stratum A but who are themselves in either C,

[30] The empirical findings presented here apply only to the particular mobility tables which we have analyzed. Would similar results have been obtained if quite different definitions of the status categories had been used, if the sample had been increased in size to the extent necessary to permit an analysis of the full 7×7 cross-classification table, or to permit an analysis of the still larger table that would be obtained when more than seven categories are employed? Would similar results have been obtained if different mobility data (e.g., data pertaining to U.S. mobility) had been used? These questions we leave for future research.

D, or E (call these individuals group α) with those individuals whose fathers were in stratum B but who are themselves in either C, D, or E (call these individuals group β), then the probability that an individual in group α will be in stratum C, the probability that he will be in stratum D, and the probability that he will be in stratum E, are equal to the corresponding probabilities for group β. (*b*) If we consider those individuals whose fathers were in stratum A but who are themselves in either D or E (call these individuals group α^*), those individuals whose fathers were in stratum B but who are themselves in either D or E (call these individuals group β^*), and those individuals whose fathers were in stratum C but who are themselves in either D or E (call these individuals group γ^*), then the probability that an individual in group α^* will be in stratum D is equal to the corresponding probability for group β^* which in turn is equal to the corresponding probability for group γ^*. (*c*) If we compare those individuals whose fathers were in stratum D but who are themselves in either A, B, or C (call these individuals group $\delta^\dagger$) with those individuals whose fathers were in stratum E but who are themselves in either A, B, or C (call these individuals group $\epsilon^\dagger$), then the probability that an individual in group $\delta^\dagger$ will be in stratum A, the probability that he will be in stratum B, and the probability that he will be in stratum C are equal to the corresponding probabilities for group $\epsilon^\dagger$. (*d*) If we consider those individuals whose fathers were in stratum C but who are themselves in either A or B (call these individuals group $\gamma^\ddagger$), those individuals whose fathers were in stratum D but who are themselves in either A or B (call these individuals group $\delta^\ddagger$), and those individuals whose fathers were in stratum E but who are themselves in either A or B (call these individuals group $\epsilon^\ddagger$), then the probability that an individual in group $\gamma^\ddagger$ will be in stratum A is equal to the corresponding probability for group $\delta^\ddagger$, which in turn is equal to the corresponding probability for group $\epsilon^\ddagger$.

SOME ADDITIONAL METHODS OF COMPARISON

Applying our methods of analysis to observed patterns of mobility (Tables 3 and 8), we have compared the conformity of these data to the two hypotheses H_1 and H_2: the hypothesis H_1 states that, aside from status inheritance in the upper strata and in the lower strata, there is perfect mobility between the strata; H_2 states that, aside from status inheritance in the upper strata, in the lower strata, and in the middle stratum, there is perfect mobility between the strata. We shall now present additional methods for comparing the conformity of these data to the two hypotheses.

For the 3 × 3 table (e.g., Table 1), the hypothesis of perfect mobility given by equation (2) specifies that the nine probabilities P_{ij} ($i = 1, 2, 3; j = 1, 2, 3$), associated with the nine cells in the table, can be determined in terms of the parameters P_i and R_j associated with the ith row and the jth column of the table. The hypothesis H_1 applied to this table ignores the question of whether the two probabilities, P_{11} and P_{33}, can be determined in terms of the parameters P_i and R_j, but it specifies that the remaining seven probabilities in the table can be determined by these parameters. The hypothesis H_2 applied to this table ignores the question of whether the three probabilities, P_{11}, P_{22}, P_{33}, can be determined in terms of the parameters P_i and R_j, but it specifies that the remaining six probabilities associated with the six non-diagonal cells can be determined by these parameters. To compare the conformity of the observed data to H_1 and H_2, we compare the likelihood of obtaining these data when H_1 is true with the likelihood of obtaining these data when H_2 is true. Since these likelihoods will depend upon the parameter values, we use the maximum likelihood estimators of these parameters in each case. Thus we compare the maximum likelihood of obtaining the observed data when H_1 is true with the maximum likelihood of obtaining these data when H_2 is true. The ratio of these two likelihoods we denote by λ, the likelihood-ratio

statistic. If λ is close to one, then the data conform to the hypothesis H_1 almost as well as they do to the hypothesis H_2. If λ is very small, the data do not conform to the hypothesis H_1.

Calculating λ for the data given in Table 3, we obtain $\lambda = 0.00005$ for the British data and $\lambda = 0.003$ for the Danish data. A similar calculation applied to the data in Table 8, to measure their conformity to the hypothesis corresponding to H_1, would yield the same value as that obtained for the British data in Table 3, namely, $\lambda = 0.00005$. (Recall that the British data in Table 3 are a condensation of Table 8.) Thus the likelihood of obtaining the observed data when H_1 is true is very small indeed when compared with the likelihood of obtaining these data when H_2 is true.

Could such small values of λ be obtained as a result of sampling variability, in the case where H_1 is in fact true? To answer this question, we note that the large-sample distribution of the statistic

$$Y^2 = -2 \log \lambda \qquad (30)$$

is the χ^2 distribution with one degree of freedom, when H_1 is true.[31] Calculating Y^2 for the data in Table 3, we obtain $Y^2 = 20$ for the British data and $Y^2 = 12$ for the Danish data. Since the ninety-ninth percentile for the χ^2 distribution is 6.63, the observed values of Y^2 should lead to the rejection of hypothesis H_1.

A test of the hypothesis H_1 that pertains to the relative conformity of H_1 and H_2 to the observed data can also be based upon the χ^2 goodness-of-fit statistics presented earlier. We compare the X^2 statistic pertaining to the goodness-of-fit of the expected values estimated when H_1 is assumed true with the X^2 statistic pertaining to the goodness-of-fit of the expected values estimated when H_2 is assumed true. Let Z^2 denote the difference between these two statistics. When H_1 is true, the large-sample distribution of the statistic Z^2 is the χ^2 distribution with one degree of freedom. Calculating Z^2 for the data given in Table 3, we obtain $Z^2 \approx 19$ (i.e., $19.2 - 0.6$) for the British data and $Z^2 \approx 12$ (i.e., $12.6 - 0.8$) for the Danish data. A similar calculation applied to the data in Table 8 yields $Z^2 \approx 19$ (i.e., $26.7 - 7.9$). Since the ninety-ninth percentile for the χ^2 distribution is 6.63, the observed values of Z^2 should lead to the rejection of H_1. By this method, too, we see that the data do not support the hypothesis H_1, and again we note that status inheritance in the middle stratum cannot be ignored.

[31] This result can be shown to follow from, e.g., Theorem 13.8.1 in Samuel S. Wilks, *Mathematical Statistics* (New York: John Wiley & Sons, 1962).

MOBILITY AS A PROBABILITY PROCESS

The methods presented here can be applied to situations where mobility is viewed as a probability process.[32] To illustrate this, let us consider the situation where individuals are observed at two time points (say, 1 and 2) and their status categories are noted at each time. For simplicity, let us suppose that three status categories are used. Let π_{ij} denote the probability that an individual who is in the ith category ($i = 1, 2, 3$) at time 1 will be in the jth category ($j = 1, 2, 3$) at time 2. Thus

$$\sum_{j=1}^{3} \pi_{ij} = 1 \qquad \text{for } i = 1, 2, 3 .$$

Let f_{ij} denote the number of observed individuals who were in category i at time 1 and in category j at time 2. In this context,

[32] These methods are quite different from those presented in earlier studies that use probability processes as models for mobility and for other processes of change. See, e.g., Isadore Blumen, Marvin Kogan, and Philip T. McCarthy, *The Industrial Mobility of Labor as a Probability Process* (Ithaca, N.Y.: Cornell University Press, 1955); Leo A. Goodman, "Statistical Methods for the Mover-Stayer Model," *Journal of the American Statistical Association*, LVI (1961), 841–68; Leo A. Goodman, "Statistical Methods for Analyzing Processes of Change," *American Journal of Sociology*, LXVIII (1962), 57–78; Judah Matras, "Differential Fertility, Intergenerational Occupational Mobility, and Change in the Occupational Distribution: Some Elementary Interrelationships," *Population Studies*, XV (1961), 187–97.

"perfect mobility" would be described by the condition that $\pi_{1j} = \pi_{2j} = \pi_{3j}$ for $j = 1, 2, 3$. This condition is equivalent to the condition that

$$\pi_{ij} = R_j \quad \text{for} \quad i = 1, 2, 3 \quad \text{and } j = 1, 2, 3, \tag{31}$$

where the R_j are positive constants such that

$$\sum_{j=1}^{3} R_j = 1 .$$

When condition (31) is not met, mobility is imperfect. If $\pi_{jj} > \pi_{ij}$ for all $i \neq j$ ($j = 1, 2, 3$) then there is "status inheritance" in all three strata.[33] The concept of "quasi-perfect-mobility" in the case where there is "status inheritance" in all three strata would be described by the condition that

$$\pi_{ij} = \begin{cases} A_j , & \text{for } i = j \\ (1 - A_i)R_j/(1 - R_i) , & \text{for } i \neq j , \end{cases} \tag{32}$$

where the R_j are positive constants such that

$$\sum_{j=1}^{3} R_j = 1 ,$$

and the A_j are also positive constants. The parameter A_j in this model is estimated by

$$A'_j = f_{jj} \Big/ \sum_{k=1}^{3} f_{jk} ;$$

the parameter R_j is estimated by

$$R'_j = V'_j \Big/ \sum_{i=1}^{3} V'_i ,$$

where V'_j is determined exactly as before.[34] To test whether the quasi-perfect-mobility model described by equation (32) actually fits an observed set of mobility data (i.e., an observed set of f_{ij} or h_{ij}), we calculate χ^2 given by equation (23) and carry out the test exactly as before.[35]

The model described by equation (31) states that the nine transition probabilities π_{ij} ($i = 1, 2, 3; j = 1, 2, 3$) pertaining to the mobility process are of a special form which can be completely summarized by the three parameters R_j ($j = 1, 2, 3$). The model described by equation (32) states that the π_{ij} are of a special form which can be completely summarized by the six parameters R_j ($j = 1, 2, 3$) and A_j ($j = 1, 2, 3$).[36] Each of the other models presented earlier in this article can be shown to provide different special forms for the transition probabilities pertaining to the mobility process, and the various tests provided herein can be used to test which of these special forms actually fit an observed set of mobility data.

STATUS CATEGORIES OR STATUS SCORES

The methods presented above were developed for the analysis of categorized data, but some of the general concepts which we have introduced can also be applied to situations where meaningful quantitative status scores might be available. Instead of cross-classification tables we would have the corresponding scatter diagrams describing the relationship between, say, each individual's

[33] The term "status inheritance" might be replaced here by "status persistence" or "status stability" to describe this condition.

[34] See n. 23. In the present context, h_{ij} is the number of observed individuals who were in category i at time 1 and in category j ($i \neq j$) at time 2.

[35] To test whether the perfect-mobility model described by eq. (31) actually fits an observed set of mobility data, we calculate X^2 given by eq. (5) exactly as earlier herein.

[36] Since

$$\sum_{j=1}^{3} R_j = 1 ,$$

the model (31) can actually be summarized by two parameters, and the model (32) can be summarized by five parameters.

status score X and his father's score Y. Instead of a model which implies that the row and column classifications were independent of each other in various subtables of the larger cross-classification table, we would have the hypothesis that the X and Y scores are independent of each other in various subsections of the larger scatter diagram. For example, the larger scatter diagram could be divided into four quadrants, and the two quadrants containing the "movers" (i.e., those individuals whose scores were above [below] the cutting point and whose fathers were below [above] the cutting point) could be analyzed separately to test the hypothesis that X and Y scores were independent of each other in both of these quadrants.[37] In some contexts, rather than focus attention on these two particular quadrants, other square or rectangular subsections of the larger scatter diagram might be of special interest.

In closing, we take note of the fact that the particular models and methods developed here can be applied in studying phenomena other than mobility. The techniques presented in, say, the section on the analysis of Table 7 above can be extended to provide a wide range of models and methods for studying other phenomena for which the relevant data can be summarized in contingency tables. (The number of rows and columns in a table need not be equal.) These techniques are different from the usual methods applied to the analysis of contingency tables, and they may in some cases shed additional light on such data. In turn, contingency-table methods other than those developed here may shed additional light on mobility data.[38]

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[37] For each of the two quadrants, the rank-correlation coefficient or some other non-parametric method might be used. Although a zero value for the Pearsonian correlation coefficient does not necessarily imply independence between X and Y (if there is a bivariate normal distribution then it does imply this), we may nevertheless be interested to test the hypothesis that the correlation was zero in each of the two quadrants, and to study in more detail the relationship between X and Y in each quadrant.

[38] See, e.g., my "Simultaneous Confidence Limits for Cross-Product Ratios in Contingency Tables," *Journal of the Royal Statistical Society*, Series B, XXVI (1964), 86–102, where quite different methods are developed, esp. pp. 96–97, where mobility data are used to illustrate the application of these methods.

Norms: The Problem of Definition and Classification

Jack P. Gibbs

ABSTRACT

Following a critical appraisal of the conceptual treatment of norms in sociological literature, a typology of norms is presented. The typology treats collective evaluations of behavior, collective expectations of behavior, and reactions to behavior as the basic normative dimensions. These dimensions generate a total of nineteen types of norms, four of which are possible null classes.

Sociological literature reveals three shortcomings in the conceptual treatment of norms: (1) a lack of agreement in generic definitions, (2) no adequate classificatory scheme for distinguishing types of norms, and (3) no consistent distinction between attributes of norms that are true by definition and those that are contingent (i.e., attributes which vary from one norm to the next and therefore are not relevant for a generic definition of norms). This paper considers the three problems and offers a solution for each.

LACK OF AGREEMENT IN DEFINITION

The following definitions of norms illustrate differences and certain points of agreement among sociologists.

Bierstedt: "A norm, then, is a rule or a standard that governs our conduct in the social situations in which we participate. It is a societal expectation. It is a standard *to which we are expected to conform whether we actually do so or not.*"[1]

Broom and Selznick: "All societies have rules or *norms* specifying appropriate and inappropriate behavior, and individuals are rewarded or punished as they conform to or deviate from the rules. The norms are blueprints for behavior, setting limits within which individuals may seek alternate ways to achieve their goals. Norms are based on cultural *values,* which are justified by moral standards, reasoning, or aesthetic judgment."[2]

Homans: "A *norm* is a statement made by a number of members of a group, not necessarily by all of them, that the members ought to behave in a certain way in certain circumstances."[3]

Johnson: "A norm is an abstract *pattern,* held in the mind, that sets certain limits for behavior. An 'operative' norm is one that is not merely entertained in the mind but is considered worthy of following in actual behavior; thus one feels that one *ought* to conform to it. This feeling means that one 'accepts' the norm."[4]

Morris: "Norms are generally accepted, sanctioned prescriptions for, or prohibitions against, others' behavior, belief, or feeling, i.e. what others *ought* to do, believe, feel—*or else.* Norms must be shared prescriptions. . . . Norms always include sanctions."[5]

Newcomb: "The term 'norm,' unfortunately, has several meanings. We shall use it, however, only in the sense of 'more or

[1] Robert Bierstedt, *The Social Order* (2d ed.; New York: McGraw-Hill Book Co., 1963), p. 222. (Italics mine.)

[2] Leonard Broom and Philip Selznick, *Sociology* (3d ed.; New York: Harper & Row, 1963), p. 68.

[3] George C. Homans, *Social Behavior: Its Elementary Forms* (New York: Harcourt, Brace & Co., 1961), p. 46.

[4] Harry M. Johnson, *Sociology* (New York: Harcourt, Brace & Co., 1960), p. 8.

[5] Richard T. Morris, "A Typology of Norms," *American Sociological Review,* XXI (October, 1956), 610.

less fixed frame of reference,' whether of quantitive or qualitative nature."[6]

Williams: "*Norms* . . . are rules of conduct; they specify what should and should not be done by various kinds of social actors in various kinds of situations. . . . The term 'cultural norm' refers to a specific prescription of the course that action *should* (is supposed to) follow in a given situation. . . . For a whole group or society, probably the best index to an institutional norm is the occurrence of severe penalties for violation. Such penalties are truly institutional, however, only if supported by an effective consensus of the society."[7]

In a survey of the conceptual treatment of norms some ten years ago, Rommetveit observed that there are three distinct uses of the term: (1) to indicate uniformities in behavior, (2) to designate a particular shared "frame of reference," and (3) to express the existence of social obligation or pressure.[8] Inspection of the above definitions reveals only two points of agreement. First, none of them suggests that a norm is simply a uniformity in behavior. Second, with the exception of the definition offered by Newcomb, none of the definitions identifies a norm as a shared frame of reference in a strictly psychological sense. However, beyond these two points there is very little agreement among the definitions. Further, most of the definitions are ambiguous in that they leave various questions concerning the character of norms unanswered. For example, must a norm in fact govern conduct; or, stated otherwise, how much deviation is allowed before the standard is no longer a norm? Also, must norms be supported by, or otherwise consistent with, collective values, in the sense that most persons find them just and acceptable? If so, are the military laws which govern an occupied country to be excluded from the category of norms?

The reference to military laws leads to a consideration of sanctions as a normative element. But observe that some of the above definitions mention sanctions, while others do not. Moreover, the definitions which do refer to sanctions leave three important questions unanswered: (1) Does the content of the sanction enter into the definition of norms? (2) Must the sanction be administered in a particular way? (3) Must the sanction actually be administered in a large proportion of cases or is it only necessary that an attempt be made to administer it?

Difference in the above definitions and the ambiguous quality of some of them stem primarily from the fact that there are several distinct types of norms.[9] One treatment of the concept may differ from another because the two are actually concerned with different types of norms; and, when the definition is truly generic, it is typically ambiguous because it is not set forth in the context of a typology which makes all of the attributes of norms explicit.

The latest survey of the conceptual treatment of norms, that conducted by Dohrenwend, implicitly recognizes various types in setting forth a generic definition: "A social norm is a rule which, over a period of time, proves binding on the overt behavior of each individual in an aggregate of two or more individuals. It is marked by the following characteristics: (1) Being a rule, it has content known to at least one mem-

[6] Theodore M. Newcomb, *Social Psychology* (New York: Henry Holt & Co., 1958), p. 266.

[7] Robin M. Williams, Jr., *American Society* (2d ed.; New York: Alfred A. Knopf, 1960), pp. 24, 25, 30.

[8] Ragnar Rommetveit, *Social Norms and Roles* (Minneapolis: University of Minnesota Press, 1955), pp. 18–26.

[9] The heterogeneous character of norms evidently prompts some observers to focus on types of norms and to eschew a generic definition. Kingsley Davis, for example, offers a systematic analysis of several commonly recognized types of norms (folkways, mores, customary law, enacted law, custom, morality, etc.), but does not set forth a formal and explicit definition which applies to norms generally (*Human Society* [New York: Macmillan Co., 1949], chap. iii).

ber of the social aggregate. (2) Being a binding rule, it regulates the behavior of any given individual in the social aggregate by virtue of (*a*) his having internalized the rule; (*b*) external sanction in support of the rule applied to him by one or more of the other individuals in the social aggregate; (*c*) external sanctions in support of the rule applied to him by an authority outside the social aggregate; or any combination of these circumstances."[10]

The above definition of norms is commendable, in that it is precise but at the same time so general that other definitions can be subsumed under it. However, the definition suggests that the members of a social aggregate must actually conform to a certain standard of conduct before that standard is a norm. Some other conceptual treatments (see, e.g., Bierstedt's definition) do not agree. Furthermore, if we identify norms in terms of behavior, it is tautological to speak of the former as influencing or controlling the latter. The position is taken here that the degree of conformity to norms is a contingent but not a definitional attribute. Finally, in ascribing multiple attributes to norms, Dohrenwend's treatment suggests but does not produce a typology of norms.

TYPES OF NORMS

Sociologists are fully aware that there are different types of norms, but an exhaustive and mutually exclusive set of types is still wanting. Distinctions commonly are drawn among folkways, mores, and laws; but such distinctions by no means exhaust all possibilities. Moreover, they are *ad hoc* and lack a generalized rationale in that they do not explicitly identify the dimensions to which their components might be referable. To identify such dimensions, it is necessary to have recourse to the procedure of substruction, which, by making the various attributes of norms explicit, may lead to a typology that includes more than the commonly recognized types.[11]

Regardless of the way a typology is constructed, it must not confuse those attributes of norms which are true by definition with those that are contingent, that is, the ways in which norms may differ, even norms of the same type. Thus, using Richard T. Morris' analysis as an example, norms may differ with regard to such attributes as: (1) the extent to which they are known or recognized, (2) the extent to which they are accepted as being just, (3) the degree to which they are uniformity applied to all groups or categories, (4) whether they are severely or lightly sanctioned, (5) the mode and consistency of enforcement, (6) source of authority, (7) the degree to which they are internalized, (8) the mode of their transmission, and (9) the amount and kind of conformity to them.[12]

These attributes do serve to characterize norms, but norms of the same type may differ with regard to them. As a case in point, laws differ with regard to some of

[10] Bruce P. Dohrenwend, "Egoism, Altruism, Anomie, and Fatalism: A Conceptual Analysis of Durkheim's Types," *American Sociological Review*, XXIV (August, 1959), 470.

[11] See, e.g., Allen H. Barton, "The Concept of Property-Space in Social Research," in Paul F Lazarsfeld and Morris Rosenberg (eds.), *The Language of Social Research* (Glencoe, Ill.: Free Press, 1955), pp. 50–52. Despite its methodological sophistication, Barton's analysis of types of norms is not definitive, because it does not distinguish laws from rules. Further, Barton's typology treats the origin of norms (tradition versus enactment) as a definitional attribute. The origin of a norm is of historical interest only (i.e., it has no necessary relation to any other characteristics of the norm in the present); therefore, the origin of a norm should be treated as a contingent attribute.

Anderson and Moore's analysis of norms is not relevant because their concern is with relations among particular norms (e.g., their logical consistency) and not a typology of norms. Further, they do not focus exclusively on definitional attributes. As a case in point, some norms prescribe or make certain acts "obligatory," while others proscribe or designate certain acts as "forbidden"; but such distinctions are not central to a definition of norms generally. See Alan R. Anderson and Omar K. Moore, "The Formal Analysis of Normative Concepts," *American Sociological Review*, XXII (February, 1957), 9–17.

[12] Morris, *op. cit.*

the attributes identified by Morris (e.g., some laws are severly sanctioned but others are not, some laws are widely "accepted" but others are not). Moreover, some of the attributes (e.g., the degree of internalization) do not enter into the conventional generic definition of norms. The point is that the typology proposed by Morris contains a mixture of definitional and contingent attributes and thereby confuses the purely conceptual treatment with the empirical study of norms.

The distinction between definitional and contingent attributes is not intended to suggest that the latter are secondary for research and theory. On the contrary, there are at least two major questions concerning contingent attributes. First, what is the relation between each of the various definitional attributes and each of the various contingent attributes? As an illustration, is there a relationship between the administration of sanctions by persons in particular statuses (treated here as one of the definitional attributes of norms) and the severity of the sanctions? Second, what is the relationship among the various contingent attributes of norms? As an example, is there a relation between the specificity of a sanction and the severity of the sanction? These are only illustrative of the numerous questions that can be asked about the contingent attributes of norms, yet such questions assume that the definitional attributes and related types of norms have been established.

DEFINITIONAL ATTRIBUTES

A norm in the generic sense (i.e., encompassing all the various types of norms) involves: (1) a collective evaluation of behavior in terms of what it *ought* to be; (2) a collective expectation as to what behavior *will be;* and/or (3) particular *reactions* to behavior, including attempts to apply sanctions or otherwise induce a particular kind of conduct. Virtually all conceptions of norms can be subsumed under this generic definition, and it further has the virtue of encompassing all of the attributes which distinguish types of norms.

Collective evaluations.—Perhaps the most commonly recognized characteristic of a norm is a shared belief that persons *ought* or *ought not* to act in a certain way. An emphasis on such an evaluation as an attribute of norms is in some respects closely associated with a particular sociological perspective (Durkheim and Parsons in particular), because it implies the existence of shared values. However, while a collective evaluation of an act may be the attribute of some types of norms (e.g., mores), this is not so for all types. Rules imposed on a group by an external authority (e.g., some prison regulations as they apply to convicts) or simply "unpopular" laws do not have the support of shared values; but, nonetheless, it would be most questionable to label them as non-normative. Further, collective evaluations do not characterize what are often regarded as customs. Thus, as an example, the typical American probably expects his fellow countrymen to drink coffee; but in most cases it is a matter of extreme indifference to him whether or not they do so, certainly in the sense of *ought* or *should.*

Collective expectations.—Whereas collective evaluations relate to how one ought to behave, collective expectations refer to predictions as to what persons will do; and the two are distinct attributes of norms. One illustration of the difference has already been given in the above reference to the custom of drinking coffee. But the contrast may go the other way. Most Americans probably believe that drivers ought to obey traffic regulations, but at the same time they perhaps would refrain from predicting that all of them or even a majority of them do so. For that matter, we fully expect all drivers to violate traffic regulations at one time or another, and to do it consciously and deliberately; but we persist in the belief that they ought not do so.

Reactions to behavior.—The above reference to traffic regulations leads to a con-

sideration of laws as a type of norm, and the realization that authorities on jurisprudence do not agree on the definition of a law, even positive law.[13] While a definition which suits one field may not suit all others, for the sociology of law Weber's definition is a classic: "An order will be called *law* if it is externally guaranteed by the probability that coercion (physical or psychological), to bring about conformity or avenge violation, will be applied by a *staff* of people holding themselves specially ready for that purpose."[14]

Several elements in Weber's definition are central to the present analysis. First and foremost, whether or not a norm is a law depends on *reactions* to behavior and not on collective evaluations or expectations. Although the content of the reaction to its violation does not characterize a law, the reaction nonetheless must be of a particular kind. It must be made by a *staff* of people who hold themselves in readiness to respond or, put in terms of contemporary sociology, by a person or persons in a particular status or statuses.

Other observations by Weber make it abundantly clear that the reaction does not necessarily involve the application of penalties—any kind of an attempt (even the friendly admonition) to secure conformity is sufficient as long as it is made by persons in particular statuses.[15] Thus, while we may speak of laws as "sanctioned," it must be understood that the term encompasses any attempt in the way of enforcement and not just the administration of penalties.[16] However, while the content of the sanction itself may vary, the persons who administer it may make use of physical force with a low probability of retaliation by a third party. The use of force should not be identified exclusively with the administration of physical punishment (e.g., whipping, branding) as a prescribed penalty. On the contrary, the penalty may be nothing more than a fine, but the state may use force to impose the penalty (laying hands on the person and bringing him into court, incarceration until the fine is paid, etc.). For that matter, laws may be enforced even though there is no statutory provision for a specific penalty, or even any penalty. However, the power of the state does not lie entirely in its use of force, because anyone may attempt to resist and use force in doing so. It is, rather, that the recalcitrant individual typically cannot expect other persons to rally to his cause or retaliate on his behalf when the state takes action. Indeed, to the extent that an individual can use force to resist the state and depend on the support of his fellow citizens, the society is not politically organized.

The attribute of unlimited force is crucial in distinguishing laws from another type of norm. Associations often have norms that are enforced in one way or another by persons in particular statuses. Such norms are identified commonly as *rules*. They differ from laws in that the persons responsible for their enforcement may *not* make use of force without a high probability of retaliation by a third party (the state in particular).

Since laws and rules are distinguished from other norms in terms of reaction to behavior, there is a conceptual problem in determining what the persons are react-

[13] See H. L. A. Hart, *The Concept of Law* (Oxford: Clarendon Press, 1961); Edgar Bodenheimer, *Jurisprudence* (Cambridge, Mass.: Harvard University Press, 1962); and E. Adamson Hoebel, *The Law of Primitive Man* (Cambridge, Mass.: Harvard University Press, 1954), esp. chap. ii.

[14] Max Rheinstein (ed.), *Max Weber on Law in Economy and Society* (Cambridge, Mass.: Harvard University Press, 1954), p. 5. It is significant that Hoebel, after grappling with the problem of formulating a definition which is applicable cross-culturally, set forth a definition of law much like Weber's (see Hoebel, *op. cit.*, pp. 26–28).

[15] Rheinstein, *op. cit.*, p. 6.

[16] Most of the conceptual treatments of norms focus on sanctions in the negative sense of the word (i.e., penalties for lack of conformity). This practice is continued in the present analysis, but it must be recognized that positive sanctions (i.e., rewards for overconformity or simply conformity) may be attached to some norms.

ing to when they react. Weber circumvented the problem by using the term "order" (i.e., the reaction is to the violation of an order), but he did not further define the term. A far better approach is to recognize that, like all norms, laws and rules are beliefs or expectations that persons should or will act in a certain way; but in the case of laws and rules the beliefs or expectations need not be collective (i.e., shared). It is necessary only that one person hold the expectation or belief, as long as he reacts to behavior in a certain way or controls those who do so react, with a low probability of retaliation by a third party.

A TYPOLOGY OF NORMS

When the above definitional attributes are organized in the way of a property-space arrangement, as shown in Table 1, the result is a typology of norms.

One may question the terminology employed in Table 1 to identify the types of norms, but it should be observed that there is no consensus on the matter. In any event, the configurations of attributes are

TABLE 1

A TYPOLOGY OF NORMS

		Low probability that an attempt will be made to apply a sanction* when the act occurs†	High probability that an attempt will be made to apply a sanction* when the act occurs†			
			By anyone (i.e., without regard to status)		Only by a person or persons in a particular status or statuses	
			By means that exclude the use of force	By means that may include the use of force	By means that exclude the use of force	By means that may include the use of force
Collective evaluation of the act‡	Collective expectation concerning the act§	Type A: Collective conventions	Type D: Collective morals	Type H: Collective mores	Type L: Collective rules	Type P: Collective laws
	No collective expectation concerning the act	Type B: Problematic conventions	Type E: Problematic morals	Type I: Problematic mores	Type M: Problematic rules	Type Q: Problematic laws
No collective evaluation of the act	Collective expectation concerning the act§	Type C: Customs	Type F: Possible empirical null class	Type J: Possible empirical null class	Type N: Exogenous rules	Type R: Exogenous laws
	No collective expectation concerning the act	Logical null class, i.e., nonnormative	Type G: Possible empirical null class	Type K: Possible empirical null class	Type O: Coercive rules	Type S: Coercive laws

* Including any kind of an attempt to secure conformity.
† Or when the act does not occur, as in the case of prescriptive norms.
‡ Convictions that persons should act in a certain way.
§ *Predictions as to how persons will act.*

the crucial identifying factors and not the class terms.

Several general features of the typology call for comment. First of all, combinations of the definitional attributes yield a total of nineteen types of norms, far more than even the most inclusive list of commonly recognized types. Four types of norms in Table 1 (F, G, J, and K) are left unnamed. Some appropriate designations may be realized eventually, but far more important is the possibility that these four types are, empirically speaking, null classes. The rationale for this anticipation can be put best in the way of a question: Why would persons in a social aggregate frequently attempt to apply sanctions in response to a certain act when they have no collectively shared evaluations and/or expectations concerning the act? This could occur in a case where one or more of the divisions of a social aggregate has a distinctive subculture,[17] but the typology is not concerned with the obvious fact that norms are relative.

Not all of the types of norms in the sociological literature are identified in Table 1, but this does not mean that the typology excludes all types which are not explicitly identified in the table. Various combinations of the nineteen types (i.e., reduction of attributes) may be related to more inclusive concepts. For example, it may be that Sorokin's "law-norms"[18] encompass two or more of the types in Table 1. Space limitations preclude the linking of the norms in the table with other and perhaps more inclusive types.

Comments on particular types of norms.—The terms "convention" and "custom" are frequently employed in discussions of norms, but there does not appear to be any consensus in definitions of them beyond the point that they may not be sanctioned. In keeping with Weber's distinction,[19] a convention is a collective evaluation of behavior (i.e., one *ought not* or, in the case of prescribed norms, one *should* behave in a particular way). While all conventions are collective evaluations, some of them may not be collective expectations. When the collective evaluation corresponds to collective expectations, we may speak of a *collective* convention; otherwise, it is a *problematic* convention, a label chosen to indicate that persons have doubts about others actually conforming to the norm.[20] Finally, customs[21] are collective expectations as to what persons actually do and not beliefs as to what they should do.

Laws and rules are peculiar in that there are four types of norms that are laws and four types that are rules. Thus, in the context of the typology, the term law encompasses all types P, Q, R, and S norms, while types L, M, N, and O all qualify as rules.

Rather than leave the four types of laws and rules unlabeled, tentative class terms are suggested in Table 1. A law or rule that corresponds to shared evaluations and expectations is designated as *collective* to signify that it has popular support. In contrast, when persons support a law or a rule (e.g., traffic regulations) but profess doubts as to others actually conforming to it, the term *problematic* appears to be an appropriate designation. On the other hand, persons may not have a high regard for a law or a rule but at the same time expect

[17] See J. Milton Yinger, "Contraculture and Subculture," *American Sociological Review*, XXV (October, 1960), 625–35.

[18] Pitirim A. Sorokin, *Society, Culture, and Personality* (New York: Harper & Bros., 1947), pp. 71–85.

[19] Rheinstein, *op. cit.*, p. 5.

[20] Note, however, that collective expectations may be in error, i.e., they may overestimate or underestimate the actual degree of conformity; but, again, the actual amount of conformity is treated as a contingent and not a definitional attribute. Furthermore, what persons think the degree of conformity is may be far more important as a determinant of their behavior than is the actual degree of conformity.

[21] The term "custom" is preferred over "folkways" because the latter suggests norms which are peculiar to non-literate or "simple" societies.

conformity (e.g., payment of a poll tax to qualify as a voter in some states). Such a law or rule suggests the designation *exogenous*. Finally, some laws or norms do not command either popular support or a shared expectation of conformity; and they can best be designated as *coercive*, in the sense that coercion is the only discernible basis for them.

Mores resemble conventions in that they are collective beliefs as to how persons ought to behave. However, in keeping with Sumner, they relate to certain kinds of conduct which are deemed so important to social welfare that they are defended overtly (i.e., there is a high probability that *someone* will attempt to apply sanctions to those persons who violate them).

Attempts to administer sanctions in support of a *mos* may include the use of force. This characteristic virtually excludes mores from urban societies, where the state supposedly has a virtual monopoly on the legitimate use of force. The present treatment of mores may be contrary to current usage, but it does offer a solution to a baffling problem in comparative law. One perennial debate in jurisprudence pertains to the nature of social control in non-literate societies, particularly as to whether or not such societies have laws.[22] In some non-literate societies the violation of norms may result in the administration of sanctions through means that include the use of force.[23] While this in itself may not be indicative of a legal system (because the administration of sanctions is not organized), some concept should be employed to classify the norms even if they are not laws. The appropriate designation, in the context of the present typology, is mores.

While mores are virtually excluded from urban societies, such is not the case for morals. In the present typology, morals are sanctioned norms; but the sanctions may be administered by anyone rather than persons in a particular status, and the means of administering the sanction or the sanction itself exclude the use of force. However, as in the case of mores, while morals represent collective beliefs as to what conduct should be, they may or may not correspond to what people actually expect conduct to be.

When mores or morals are supported by collective beliefs *and* are consistent with expectations concerning behavior, the term *collective* is an appropriate designation. On the other hand, if they are supported by collective beliefs but are not consistent with expectations as to actual conduct, the appropriate term is *problematic*.

CONCEPTUAL AND EMPIRICAL PROBLEMS

Given the lack of consensus in the conceptual treatment of norms and the absence of a large body of reliable cross-cultural data on the subject, no typology can lay claim to being definitive. The present typology is no exception and, accordingly, some possible deficiencies should be noted.

The most obvious problem on the empirical side is that the typology cannot be applied without extensive research. Thus, to illustrate, the typology does not enable us to classify the alleged proscription of premarital sexual relations in the United States. It is simply a matter of not knowing what the collective evaluations, collective expectations, and reactions concerning premarital sexual relations really are. However, this is a problem for any typology and, for that matter, it is indicative of the fact that observations on norms in a particular society rarely are based on "hard" data.

There are several major problems on the conceptual side. Above all, no specific and meaningful criterion can be stated at present for a collective evaluation, a col-

[22] See, e.g., Hoebel, *op. cit.*, esp. chap. i.

[23] The Nuer, Eskimo, and certain tribes in New Guinea (prior to subjugation by Europeans) are cases in point. See E. E. Evans-Pritchard, *The Nuer* (Oxford: Clarendon Press, 1940); Hoebel, *op. cit.*, chap. v; and Ronald M. Berndt, *Excess and Restraint* (Chicago: University of Chicago Press, 1962).

lective expectation, or the probability of reaction. Supposedly, survey methods can reveal what 50 per cent or more of the members of a population think persons ought to do and what they expect them actually to do, but the cutting point is obviously arbitrary and therefore questionable. The situation is much the same for reactions to behavior. Again supposedly, field studies can reveal what the reaction has been in cases where the deviant was detected, but the criterion of a "low" or a "high" probability of a certain kind of reaction is no more certain now than it was when Weber employed the concept in his analysis of normative phenomena.

Still another conceptual problem is the treatment of reactions in terms of "attempts" to apply sanctions or otherwise enforce the norm. One could argue that only successful attempts should be considered, or that the success of the attempts should be treated as another definitional attribute of norms. One could argue further that the correspondence between attempted or actual reactions and collective evaluations or expectations concerning reactions is a definitional attribute of norms.[24] There are merits in both arguments, but the inclusion of these attributes would make a very complicated typology. For this reason and because most definitions of norms do not include these additional variables, it is perhaps best, at least for the present, to regard them as contingent, but not definitional, attributes.

SUMMARY AND CONCLUSION

Although the concept is central to the social sciences, norms have not been treated in a truly systematic manner, and particularly so with regard to the construction of typologies. The present analysis suggests three definitional attributes of norms: collective evaluations, collective expectations, and reactions to behavior.

An analysis of the three normative attributes in terms of substruction and their property-space arrangement reveals nineteen distinct types of norms. The typology enables us to identify most of the commonly recognized kinds of norms and, at the same time, to designate certain kinds which have not been treated conceptually or empirically.

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[24] E.g., if what actually happens to deviants does not correspond to what persons think will or should happen to them, the related standard of conduct is not a norm, or at least it is a special type of norm.

The Measurement of Empathy

Charles W. Hobart and Nancy Fahlberg

ABSTRACT

In this paper we examine the recent literature on the predictive test of empathy, propose another procedure for scoring predictive empathy tests, and present some data suggestive of the relationship between various predictive measures which have been described in the literature. We attempt to demonstrate that a significant source of confusion in the literature, and particularly in the published critiques of the predictive test, results from important conceptual and methodological differences in the approaches of psychologically and sociologically oriented social psychologists to the study of empathy. The measure we propose permits precise segregation of "likeness bias" or projection but does not permit simultaneous segregation of "unlikeness bias" or reverse projection.

The predictive test of empathy involves having a "judge" (J) predict the responses of an other (O) to a set of items, and then comparing J's predictions with the responses that O actually makes to these same items. This technique was first used by Dymond in 1945.[1] Criticisms began to appear in 1952 with doubts concerning the "raw empathy score"[2] and culminated in the publication in 1955 of three papers which were very critical of the predictive test technique.[3] The consequence appears to have been the virtual abandonment of this procedure, since no important studies making use of the predictive test of empathy have been published since that date.[4] The purpose of this paper is to evaluate some of the major criticisms that have been made and to propose a defensible predictive test of empathy.

A central point in our argument is that there are significant differences between the approaches of the social psychologist and the psychologist to the study of empathy. The lack of comparability of research findings from various studies may result from the fact that psychologists and social psychologists have been (1) exploring different aspects of interpersonal behavior in their studies of empathy, (2) employing different types of research designs, and (3) using a variety of incomparable raw or crudely "refined" empathy scores in their different studies. Some recent criticisms of the predictive test may be appropriate in evaluating it as a measure of the social perception in which psychologists are interested, but a refined form of the predictive test may yet be well suited to the kind of study of empathy and role-taking ability that is of interest to social psychologists. Three areas will be taken up in turn: (1)

[1] Rosalind F. Dymond, "A Preliminary Investigation of the Relationship of Insight and Empathy," *Journal of Consulting Psychology*, XII (1945), 228–33.

[2] A. H. Hastorf and I. E. Bender, "A Caution Respecting the Measurement of Empathic Ability," *Journal of Abnormal and Social Psychology*, XLVII (1952), 574–76.

[3] L. J. Cronbach, "Processes Affecting Scores on 'Understanding of Others' and 'Assumed Similarity,'" *Psychological Bulletin*, LII (1955), 177–94. N. L. Gage, and L. J. Cronbach, "Conceptual and Methodological Problems in Interpersonal Perception," *Psychological Review*, LXII (1955), 411–23. A. H. Hastorf, I. E. Bender, and D. J. Weintraub, "The Influence of Response Patterns on the 'Refined Empathy Score,'" *Journal of Abnormal and Social Psychology*, LI (1955), 341–43.

[4] The data for Bronfenbrenner, Harding, and Gallwey's study of "skill in social perception" were gathered in 1954, though their work was not comprehensively reported until 1958 (Urie Bronfenbrenner, John Harding, and Mary Gallwey, "The Measurement of Skill in Social Perception," in David McClelland *et al.* [eds.], *Talent and Society* [Princeton, N.J.: D. Van Nostrand Co., 1958]).

the contrasting approaches of psychologists and social psychologists to the study of empathy, (2) the techniques used to measure empathy, and (3) the criticisms of these techniques.

PRIOR APPROACHES

Contrasting approaches.—The differences between the two groups of researchers are symbolized by the preference of social psychologists for the term "empathy" and the preference of psychologists for the term "social perception" or "person perception." These terminologies seem to imply differences in the underlying processes involved. The social psychological approach based on the work of George Herbert Mead conceives of empathy as "taking the role of the other," a process basic to socialization and the acquisition of a "self." The term "empathy" implies that this is a feelingful, perhaps intuitive, process whereby one "identifies" with another. One feels with and for that person whom he knows well enough to be able to "feel his situation."

The psychological approach stems from the psychology of perception, an area of study foreign to most sociologists. Here the involvement of feeling is given little attention. Emphasis tends to be on the accuracy or inaccuracy of perception and on sources of error in perception. How J is to infer responses to feeling items on the testing instrument from the perception opportunities he has had is unclear. It is apparently assumed that this is a matter of perceiving manifested feeling. The possible role of intuition—of identifying with another person and thus knowing his feelings, not by *seeing* them in *him*, but by *feeling* them in *oneself*—is ignored.

The differences in these two approaches are reflected in differences in research design. A typical psychological study involves having two strangers interact in a highly structured way, such as discussing movies or television for five minutes and then having them predict each other's responses on a paper-and-pencil test.[5] At the other extreme social psychologists have asked well-acquainted couples—parents and offspring or marital partners—to predict each other's responses.[6] The former studies make sense in the context of perception theory. The latter studies, from the perspective of this same theory, are unacceptably imprecise because of the impossibility of controlling the profusion of variables that may be conditioning the perceptual process.

The differences between "social perception" and "empathy" studies may be clarified by drawing distinctions among the knowing, feeling, and doing modalities,[7] and between spontaneous empathy and contrived or artificial social perception. In studies of husbands and wives, subjects empathize with each other spontaneously and in all three modalities, feeling, knowing, and doing. But two strangers who are asked to interact very briefly in a stereotyped way and then to respond to a predictive test exemplify an artificial and contrived social perception process. Only the knowing modality will be very much involved because (1) strangers tend to be guarded in their communication of feeling, and (2) they have virtually no opportunity to see each other "doing."

Perception, we suspect, may be a necessary but not sufficient condition for em-

[5] N. L. Gage, "Judging Interests from Expressive Behavior," *Psychological Monographs*, LXVI (1952), Whole No. 350. Likewise, Bronfenbrenner *et al.* (*op. cit.*) had their J's make predictions for O's after very brief acquaintanceship.

[6] Clifford Kirkpatrick and Charles Hobart, "Disagreement, Disagreement Estimate and Non-Empathic Imputations for Intimacy Groups Varying from Favorite Date to Married," *American Sociological Review*, XIX (1954), 10–19.

[7] B. Notcutt and A. L. M. Silva, "Knowledge of Other People," *Journal of Abnormal and Social Psychology*, XLVI (1951), 30–37. Stuart Carter Dodd, "A Predictive Theory of Opinion—Using Nine 'Mode' and 'Tense' Factors," *Public Opinion Quarterly*, XX (1956), 571–85.

pathy. We define empathy, following Dymond, as the "imaginative transporting of oneself into the thinking, feeling and acting of another and so structuring the world as he does"[8]—in other words, as taking the role of the other. The scant involvement of perception is seen in our ability to empathize with a disaster victim known only through a news story, and our ability not to empathize with people we meet briefly who are physically present.

Measurement procedures.—A major methodological problem is that of distinguishing between empathy, correctly taking the role of the other, and projection. Projection, "the attribution to another of one's own needs, interests, and attitudes,"[9] is a process antithetical to empathy. Whether correct prediction of O's response to a questionnaire item is indicative of empathy or of projection becomes problematic when the *own* responses of J and O are identical.

The approaches of Dymond and of Hastorf and Bender exemplify the two major solutions to this discrimination problem. Dymond defines empathy operationally as the closeness with which J's response predictions for O correspond with O's responses. Dymond's deviation score is the sum of J's predictive errors when J's predictions and O's responses are recorded on a five-point scale.[10] Dymond found that subjects were able to predict correctly significantly more often than if chance alone were operating.[11]

Hastorf and Bender, in a critique of Dymond's approach,[12] considered the possibility that projection was compounded with empathy in her deviation score. They demonstrated that the predictions made by her J's were more closely related to their own responses than to the responses of O, which suggests projective rather than empathic predictions by J. Contrary to Dymond's presumption, Hastorf and Bender concluded that correct prediction on similar own-response items was probably due to projection.

These considerations led them to devise a refined empathy score, correcting for the projection component.[13] Dymond's deviation score was subtracted from a projection score (the sum of the deviations between J's own responses and his predictions),[14] the remainder being the refined empathy score. When the refined empathy score is positive, J is empathizing more than he is projecting; when it is negative J is projecting more than he is empathizing.[15] Note that interpreting the deviations between J's own responses and his predictions as "pure" projection is arbitrary. Kirkpatrick and Hobart interpreted this same response differential as a disagreement estimate score, reflective of the differences between J and O.[16]

Hastorf and Bender found a rank order correlation between the raw empathy score and the refined empathy score of .30.[17] They concluded that "such a low relationship indicates that the two scoring methods

[8] Rosalind F. Dymond, "Personality and Empathy," *Journal of Consulting Psychology,* XIV (1950), 343–50; "A Scale for Measurement of Empathic Ability," *Journal of Consulting Psychology,* XIV (1949), 127.

[9] Hastorf and Bender, *op. cit.*, p. 574.

[10] Her score is thus identical with the *n* 2 "incorrect prediction"—cell of Table 1, except that Table 1 relates only to the counting of items correctly or incorrectly predicted, whereas Dymond's method involves the measurement of predictive error on a five-point scale.

[11] Dymond, "A Scale . . . ," *op. cit.*

[12] Hastorf and Bender, *op. cit.*

[13] *Ibid.*

[14] Note that this is in fact a *non*-projection score.

[15] Note that these are lack-of-empathy and lack-of-projection scores.

[16] Kirkpatrick and Hobart, *op. cit.*

[17] The authors do not mention inverting the raw (non-) empathy score but it is clear that they must have done so since their next sentence begins, "Although there is some degree of commonality between the two measures . . ." (Hastorf and Bender, *op. cit.* p. 573).

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[12] Hastorf and Bender, *op. cit.*

[13] *Ibid.*

[14] Note that this is in fact a *non*-projection score.

[15] Note that these are lack-of-empathy and lack-of-projection scores.

[16] Kirkpatrick and Hobart, *op. cit.*

[17] The authors do not mention inverting the raw (non-) empathy score but it is clear that they must have done so since their next sentence begins, "Although there is some degree of commonality between the two measures . . ." (Hastorf and Bender, *op. cit.* p. 573).

are not really measuring the same ability."[18]

The refined empathy score may be criticized because highly similar J-O couple members are penalized in this determination of empathy. Two types of response patterns contribute to raising the projection score: (1) all predictions on items where J failed to perceive that he differed from O and (clearly) projected his own responses onto O, and (2) all predictions for items on which J and O responded with identical own responses and J *predicted* (or *possibly* projected) correctly. By interpreting all similarity as *projection* Hastorf and Bender commit the same error, but in the opposite direction, that Dymond committed when she interpreted as *empathy* all instances where couple members correctly predicted their similar responses. An adequate measure of empathy should achieve a better solution to the problem of differentiating empathy from "accurate projection."

Criticisms.—A variety of major objections to empathy or social perception has been raised by L. J. Cronbach, N. L. Gage, and Hastorf, Bender, and Weintraub. We shall consider here only those which could be met by an adequate measure of empathy.

Hastorf *et al.* raise two objections.[19] The first is that a low error in prediction score may be an artifact of the tendency for some subjects to make mid-scale responses under certain conditions and end-scale responses at other times. This objection may be met by using items on the empathy test that have only two response alternatives.

Their second objection is that subjects' responses to an empathy test often reflect a cultural norm, and thus apparent empathy is merely reflective of conformity to the norm by both J and O. This difficulty may be overcome by using items that have more or less equally acceptable response alternatives.

Gage and Cronbach specify three areas of confusion that make research on social perception "virtually valueless."[20] Two, dealing with J's acquaintanceship with O and with a "classification of objects of perception," do not relate to the measurement of empathy as role-taking ability. The third, having to do with a distinction between stereotype and differential accuracy, is in effect the same problem of cultural norms as that raised by Hastorf and associates.

Cronbach has developed a mathematical model of social perception based on analysis of covariance which permits factoring out the various sources of error in J's predictions of O's responses.[21] A heavy price is paid, however, since his analysis of variance technique necessitates (1) requiring J's to predict for many O's and thus (2) restricting the number of items J's can be asked to respond to. If one is interested only in measuring empathic ability and not in sources of inaccuracies, a less involved approach permits the use of more J's predicting for single O's on a longer list of items. Moreover, Cronbach's analysis technique presumes a normal distribution of responses on an equal-interval response scale having a determinable zero point. The scale that he used—"very much," "a good bit," "only slightly," "not at all"—does not meet these assumptions; thus his mathematical model has rather shaky foundations.

In a synthetic critique of "social perception scores" Cronbach dicerentiates between the faults of dyadic analysis and the faults of a global index.[22] He notes five objections to dyadic analysis, three of which are relevant here. His first, that

[18] *Ibid.*

[19] Hastorf, Bender, and Weintraub, *op. cit.*

[20] Gage and Cronbach, *op. cit.*

[21] Cronbach, "Processes Affecting . . . ," *op. cit.*

[22] L. J. Cronbach, "Proposals Leading to Analytic Treatment of Social Perception Scores," in Renato Tagiuri and Luigi Petrullo (eds.), *Person Perception and Inter-personal Behavior*, Stanford, Calif.: Stanford University Press, 1958.

"dyadic analysis is a breeding ground for artifacts," has been noted above. His second is that discrepancy scores treat regression effects as real changes. This difficulty disappears if items having two response alternatives are used. His third objection is that calculation of discrepancy scores presumes an interval scale. We have seen that this is a serious objection to Cronbach's own procedures for differentiating sources of error in social perception, but that it may be met by use of items with two response alternatives.

Two important criticisms of the predictive test of empathy emerge from this review. (1) Several critics have noted the possible influence of cultural norms and of group subcultures that would give rise to "stereotype accuracy." It is possible to get around this problem, however, by devising items not having clear cultural definition. (2) Empathy test items having three or more response categories raise problems involving both equality of scale intervals and tendencies of respondents to make mid-scale or end-scale responses. Use of empathy test items having only two response alternatives per item seems to solve these problems.[23]

PROPOSED MEASURES

We now turn to a description of our attempt to devise a measure of empathy that provides a solution to problems noted above and that does not introduce the distortions inherent in Dymond's and in Hastorf and Bender's scoring procedures. The crux of the problem of differentiating empathy from accurate projection hinges on the interpretation of items where J's and O's own responses are identical, and where prediction is correct. Items having this response pattern will be called compounded score items, since either projection or empathy, or perhaps both, may be reflected in the response pattern.

The basic data consist of the own responses and the predicted responses of both members of the responding couple. From these responses four scores are derived: the compounded, unperceived similarity, projection, and empathy scores. The relationships among these raw scores are shown in Table 1. The scores themselves become differentiated as a result of distinguishing (1) between similarity and dissimilarity of J's and O's opinions, and (2) between correct and incorrect prediction by J of O's responses. The following are operational definitions of the score types found in the fourfold table.

Similarity score *n* 1: number of items to which the responding pair gave identical own responses.

Dissimilarity score *n* 2: number of items to which the pair gave non-identical own responses.

Correct prediction score *n* .1: number of items to which J correctly predicted O's own responses.

Incorrect prediction score *n* .2: number of items to which J incorrectly predicted O's own responses.

Compounded score *n* 11: number of items to which the pair gave identical own responses and J correctly predicted O's own responses.

Empathy score *n* 21: number of items to which the pair gave non-identical own responses and J correctly predicted O's own responses.

Projection score *n* 22: number of items to which the pair gave non-identical own responses and J incorrectly predicted O's own

[23] It might be questioned whether the use of two categories resolves the equal-interval controversy. Is there any reason to assume that the interval between n and $n+1$ is equal to the interval between $n+1$ and $n+2$, except that $n+1$ had one more error than n, and $n+2$ had more error than $n+1$? But such a question presumes reconceptualizing what is in fact a nominal (yes-no) scale into an interval scale. That is, it assumes that one category involves more of something than the other, an entirely arbitrary assumption, and thus that one is dealing here with a dichotomized interval scale. Such an assumption is unnecessary, and appears unjustified, since it is more parsimonious to assume that the data, as collected, are nominal data than that they are "really" interval scale data which have been artifactually dichotomized.

responses, thus (where two response alternatives are used) making the prediction identical with J's own response.

Unperceived similarity score *n 12*: number of items to which the pair gave identical own responses and J incorrectly predicted O's own responses.

This analysis presumes the use of a test having only two response alternatives per item. Each score is computed for each subject in the sample. These operational definitions of empathy and projection insure that responses are scored as empathic only where J could not possibly be projecting and as projective only where he could not be empathizing.

Note that the "base line" of the Empathy Score, the size of the pool of items for which J might make a correct prediction of O's own responses, will differ for different pairs. That is, the number of dissimilarity items will differ for different pairs of subjects. Thus a couple having a low empathy score and a very low dissimilarity score might be more empathic than a couple with a higher empathy score but also a very high dissimilarity score. To make the raw empathy scores comparable between different subjects they were converted into ratios by dividing a couple's raw empathy score by its dissimilarity score. This score, expressed as a decimal, is here called the empathy ratio score, or ERS.

In like manner the raw projection score may be expressed as a ratio of the dissimilarity score, and the raw unperceived similarity score can be expressed as a ratio of the similarity score.

Our method of measuring empathy turns out to be identical with one suggested by Gage and Cronbach.[24] While we take as our classifying dimensions real similarity-dissimilarity and correct-incorrect prediction, Gage and Cronbach take real similarity-dissimilarity and assumed similarity-dissimilarity. Where a is J's self-description, b is O's self-description, and c is J's prediction of O's response, the real similarity-dissimilarity dimension is $a = b$, $a \neq b$. The assumed similarity-dissimilarity dimension is $a = c$, $a \neq c$, and the correct-incorrect prediction dimension is $b = c$, $b \neq c$. The combinations possible with these basic data are just four in number: (1) $a = b = c$, which we term the compounded score and Gage and Cronbach call the warranted assumed similarity score; (2) $a = b \neq c$, which we call the unperceived similarity score and they call the unwarranted assumed dissimilarity score; (3) $a \neq b = c$, which we call the empathy score and they call the warranted assumed dissimilarity

TABLE 1

RELATIONSHIPS BETWEEN RAW SCORES, SIMILARITY SCORES, AND PREDICTION SCORES

Compounded Score	Unperceived Similarity Score	Similarity Score
n 11	*n* 12	*n* 1
Empathy Score	Projection Score	Dissimilarity Score
n 21	*n* 22	*n* 2
Correct Prediction	Incorrect Prediction	
n .1	*n* .2	

[24] Gage and Cronbach, *op. cit.*

score; and (4) $a \neq b \neq c$, which we call the projection score and they call the unwarranted assumed similarity score.

METHOD

Our data were obtained from thirty-two pairs of sophomore female roommates at a residential college. The test used was the Myers-Briggs Type Indicator, which was designed to characterize people in terms of Jung's theory of psychological types. This test was chosen because there appear to be no norms specifying appropriate responses to it. It consists of ninety-four items, such as "Does following a schedule: 1-appeal to you, 2-cramp you?"; "Are you: 1-easy to get to know, 2-hard to get to know?"; "Which word in each pair appeals to you more: 1-firm-minded, 2-warm-hearted?; 1-systematic 2-spontaneous?" All of the scores noted in Table 1 were derived for each pair of subjects. In addition, Dymond's deviation scores and Hastorf and Bender's refined empathy scores were calculated for each subject pair. These various empathy scores were correlated with each other using Spearman's r_s, corrected for tie scores. In all cases the scores were ranked in such a way that the low-rank position signified accurate empathic prediction.

The results of the correlational analysis are seen in Table 2. The correlation between the deviation score and the refined empathy score is .55, in contrast to the .30 which Hastorf and Bender report.[25] The correlation of .46 between the ERS and the deviation score is of comparable magnitude. The ERS does correlate highly, .74, with the refined empathy score. Thus, if we assume that the ERS is a more valid criterion of empathy, since it requires subjects to predict *differences* between them correctly and thus is projection-free, it would appear that Hastorf and Bender were right in assuming that correct prediction on items to which own responses were identical was indicative more of projection than of empathy.

To test their position further we correlated the compounded score, expressed as a ratio of the similarity score, with the projection ratio score,[26] obtaining an r_s or .27, which is significant at the 5 per cent level. This is compatible with Hastorf and Bender's contention that compounded items (perceived similarity items) are more reflective of projection than of empathy. By the same token, however, a tendency toward correct perception of dissimilarity (empathy) is associated with a tendency toward non-perception of similarity. The r_s of the ERS with the unperceived similarity score was .27. Is there any theoretically relevant interpretation of this relationship?

TABLE 2

INTERCORRELATIONS OF VARIOUS MEASURES OF EMPATHY*

	Deviation Score	Empathy Ratio Score	Refined Empathy Score
Compounded score...	.58	−.27	.06
Deviation score......	....	.46	.55
Empathy ratio score..	....	....	.74

* All correlations are significant at the 5 per cent level with the exception of the .06.

We had been interested in the unperceived similarity score because we wondered what circumstances would lead a subject to see his partner as *unlike himself* when in fact he was like himself (on a test item having two response alternatives). These scores had a wide range, the raw scores ranging from two to thirty-two items and the ratio scores ranging from .05 to .62. We speculated that it might be "social stupidity" or perhaps "hostility" that resulted in what looked like *biased mis-*

[25] Hartorf and Bender, "A Caution . . . ," *op. cit.* p. 575.

[26] Note that the size of the raw compounded score varies with different subjects—as in the case of the raw empathy score.

perception of the similarity between self and partner. Three of our subjects fell above the chance expected unperceived similarity ratio score of .50. A plausible explanation is found in the concepts of a "likeness" and an "unlikeness" bias or what Fiedler has referred to as assumed similarity or dissimilarity.[27] What Hastorf and Bender had thought of as projection may be a bias toward seeing the other as like oneself whether he is (as in the case of compounded-score items) or is not (as in the case of projection-score items). The "unlikeness bias" is a tendency to see the other as unlike oneself, whether he actually is so (as in the case of empathy items) "refinement" which discriminates against highly similar predicting couples, as noted above. Moreover the unlikeness bias is not factored out of this refined empathy score. The empathy ratio score is precise in its removal of the likeness bias, but, as the .27 correlation with unperceived similarity for our sample shows, it is somewhat reflective of an unlikeness bias. In terms of this analysis the ERS is also a "compounded" score. The compounded (or perceived similarity) ratio score reflects empathic ability free from unlikeness bias, but compounded with likeness bias. This interpretation is compatible with the pattern of intercorrelations found in Table 2.

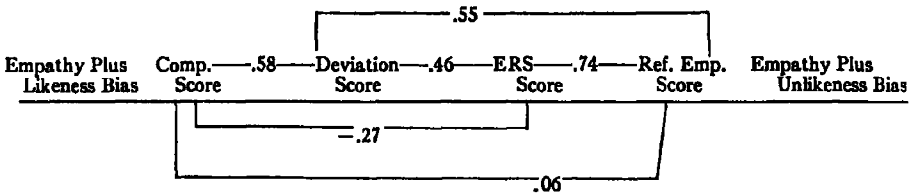


FIG. 1.—Continuum showing relationships among various empathy measures, with intercorrelations

or is not so (as in the case of unperceived similarity items). The "perceived similarity" score provides a convenient index of this unlikeness bias. We conclude that the ERS score is not the uncontaminated measure of empathic ability which we had thought it might be.

Empathic ability and likeness and unlikeness bias appear to contribute to the deviation score, the refined empathy score, the compounded score, and the empathy ratio score as follows: The deviation score appears to reflect empathic ability and both the likeness and unlikeness bias. By subtracting the projection score from the deviation score to obtain the refined empathy score, Hastorf and Bender eliminate the likeness bias. But this is an imprecise

In addition to the relationships already cited, the table shows that the compounded ratio score correlated with the deviation score and the refined empathy score .58 and .06, respectively, using the Spearman rho correlation coefficient.

A clearer way of ordering the relationships found in Table 2 is seen in Figure 1. Here the scores are arranged along a continuum showing empathy plus likeness bias at one extreme and empathy plus unlikeness bias at the other extreme. The pattern of intercorrelations found in the figure is compatible with the intrepretation that the compounded score reflects empathic ability and both likeness and unlikeness bias, and the ERS and the refined empathy score reflect empathic ability and unlikeness bias. Note that our data show little relationship between some of these "empathy measures."

[27] F. E. Fiedler, "A Method of Objective Quantification of Certain Countertransference Attitudes," *Journal of Clinical Psychology*, VII (1951), 101–7.

Perhaps no "pure" measure of empathy based on predictions of J-O *pairs* is possible: There appears to be no way simultaneously to factor out both the likeness bias and the unlikeness bias using pairs of prediction scores. Which one should be used when a measure of empathy is needed? In our sample only three out of sixty-four subjects had unperceived similarity ratio scores above the .5 chance level that would suggest an unlikeness bias. However, twenty of our subjects had a projection ratio score above the .5 chance level, suggesting the operation of a likeness bias. This is of course to be expected in a sample of roommates who in most cases have *chosen* to live with each other. Where there is reason to expect a sample-wide likeness bias, the ERS, which is free of this bias, would be the empathy measure to use. Calculation of unperceived similarity ratio scores will give insight into the extent to which unlikeness bias is operating in the sample.

Where one may expect a strong unlikeness bias, where J's may feel alienated from O's, either the deviation score or the compounded score would seem appropriate. This suggestion can be formulated more precisely. The incorrect-prediction score is composed of the unperceived similarity score and the projection score. When the mean raw unperceived similarity score is larger than the mean raw projection score, the deviation score or the compounded score should be used because an unlikeness bias is predominant. When the mean raw projection score exceeds the mean raw unperceived similarity score, the ERS which controls for likeness bias should be used.

This paper has reviewed the criticisms of predictive tests of empathy and has suggested some ways of coping with the most cogent of these criticisms. Use of test items with two response alternatives helps to overcome a number of mensuration problems. Tests composed of items which are largely culturally undefined and thus bias-free, like the Myers-Briggs Type Indicator, are available. Procedures exist for the identification and the isolation from the empathy score of both the likeness bias and the unlikeness bias, though not for the isolation of both simultaneously. Until new tests appear, a refined prediction score is still the best procedure for measuring empathy between subject pairs.

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The Play World of Camping: Research into the Social Meaning of Outdoor Recreation[1]

William R. Burch, Jr.

ABSTRACT

Two types of play-action systems—expressive and symbolic labor—are compared to indicate the theoretical value in studying outdoor recreation. Of several possible issues, two are treated: (1) conjugal solidarity and the division of play, and (2) continuities and discontinuities in player identities. For some classes of players symbolic labor systems appear to be more conducive to the development of conjugal solidarity and the provision of continuity in player identities.

INTRODUCTION

This essay explores the meaning of different forms of forest play. I would argue that the forms of leisure are as worthy of study as the forms of work, not simply because leisure is becoming a "central life interest"[2] for an increasingly large segment of the American population but because a well-rounded statement about human social life is impossible without examination of the dimensions of play. I propose that we take man, the player, seriously and that we test sociological theories in the variety of human contexts offered by outdoor recreation. To illustrate my proposal I will indicate how the study of forest campers can provide insight into some important theoretical issues. Of several possible issues, two will be treated: (1) the division of play and sexual identities and (2) identity formation and continuity in play.

BASIS OF INTERPRETATION

This report is the outgrowth of an attempt to develop workable observation schedules for forest recreation research and to collect material relevant to a questionnaire study. My interpretations are also influenced by five seasons of working with forest recreationists. From August 8 to 16, 1962, twelve campgrounds in three national forests in Oregon were systematically observed.[3] During part of this period and most of that from August 20 to August 29, interviews were conducted with 288 family camping groups in these and seven other campgrounds. Interview data are not tabulated here, as the interviews were primarily a vehicle for talking

[1] Revised version of a paper read at the 58th annual meeting of the American Sociological Association, August 26–29, 1963. I am indebted to Gregory P. Stone, Ramon Oldenburg, Wiley D. Wenger, Jr., and George Douglas for their critical reading of an earlier version of this paper.

[2] See Leo Lowenthal, *Literature, Popular Culture, and Society* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1961), pp. 109–41; and Robert Dubin, "Industrial Workers' Worlds: A Study of the 'Central Life Interests' of Industrial Workers," *Social Problems*, III (January, 1956), 131–42.

[3] The author, his wife, and their three children traveled and stayed overnight as campers. Twelve campgrounds having varying topography, attractions, and ease of access were observed with some intensity. About half the observation time was spent at camps where the predominant activity was "expressive" and the rest at camps where "symbolic labor" predominated. The observations were recorded on a form roughly similar to Table 1, with age and gender categories running horizontally and activity classes running vertically. In general, I found the notes in my journal of more value than the notes on my observation schedule. For further details and suggestions for future research see my *A New Look at an Old Friend—Observation as a Technique for Recreation Research*, Department of Agriculture, Pacific Northwest Forest and Range Experiment Station (Portland, Oregon, 1964).

with campers about their forest experiences. Data collected in the above manner seem to offer an important basis for heuristic considerations.

Camping is the central interest for the people and the prime function of the locations described in this report; camping viewed primarily as an inexpensive means of getting from one place to another is not considered here.

FRAMEWORK FOR ANALYSIS

The play world of camping, like other play worlds, has a specialized, set-apart playing field which clearly demarcates it from everyday life. As a player, one leaves behind his daily commitments which tend to constrain his spectrum of alternative actions, while on the job or in the good works of school, church, and lodge, one has his choice largely programmed by an established system of norms. In leisure, however, one has options on acceptance or rejection of a set of norms by accepting or rejecting a given play form and its attendant roles.

Camping, as a chosen system[4]—of play, provides the definitional arrangement which "contains" a variety of roles, experiences, and motivations. Granted that camping, like other play worlds, gains its existence, sustenance, and meaning by being part of a larger society; however, once set in motion, it tends to have a life and process uniquely its own. In this sense, that it is intrinsically rewarding and contains its own goals and sources of motivation, camping is an autotelic activity.[5] However, a given campsite is but the hub for a round of varying activities which in turn implicate other activities. Although camping is the general "motive" offered, its meaning undergoes considerable variation as we consider shifts in the elements of the system.

Camping differs from other play in that the campers, though isolated from the commitments of everyday life, pursue many of the routines of everyday life. The family unit for the duration of its engagement is a relatively self-sufficient unit containing the resources of existence without immediate *direct* dependency on others. Further, under the encompassing rules of the game required by camping environments and interactions, other games with their special rules emerge. Many of these games are not native to camping; however, in the process of adaptation to the new conditions, they take on the special coloration provided by the camping play system. While camping, men fish, collect rocks, play cards, and match wits; in short, within the social drama, numerous subplots are simultaneously and constantly unfolding. However, these subplots are not randomly distributed but are assigned attributes of given performing groups. Each of these subplots adds up to the total configuration of the unfolding play system. In this report, I am scanning the total system by abstracting some of the elements that make up this system.

In my observation of campers, it seemed that there were six identifiable types of play actions. These were:

1. *Symbolic labor.*—Productive play action characterized by trophies and the quest for trophies. It is the search for tangibles which signify that the search was not in vain. Hunting, fishing, and rock collecting are examples.

2. *Expressive play.*—Similar to what Roger Caillois calls *ilinx*, the "attempt to momentarily destroy the stability of perception and inflict a kind of voluptuous panic upon an otherwise lucid mind."[6] However, I use the term "expressive" to balance Caillois' individualistic bias, tak-

[4] "System" is defined as a complex unity formed of many often diverse parts subject to a common plan or serving a common purpose.

[5] See Alan Ross Anderson and Omar Khayyam Moore, "Autotelic Folk-Models," *Sociological Quarterly*, I (October, 1960), 203–15.

[6] Roger Caillois, *Man, Play and Games*, trans. Meyer Barash (Glencoe, Ill.: Free Press, 1950), pp. 45–46.

ing into consideration that *ilinx* always requires sympathetic others to be present, symbolically or physically, as witnesses to the induced disequilibrium. In fact, the disequilibrium requires an element of sobriety in others to complete its meaning. Dancing, water skiing, tree climbing, and rolling rocks down hillsides are examples.

3. *Subsistence play.*—The ordinary demands of food, shelter, and protection are fulfilled under atypical conditions with a wide range of potential existences from comfort to discomfort. These altered conditions imbue the ordinary with elements of play; it is made extra ordinary.

4. *Unstructured play.*—The creative and existential expressions of the individual as he experiments with the environment and himself. An example would be a child's attempt to see how many potential forms he can find in a pine cone.[7]

5. *Structured play.*—Play which is organized, finite, and ruled by collective sanctions for the duration of its existence. It has clearly defined goals, traditionalized roles, and explicit means to the goals. Games, contests, etc., are examples. This is similar to what Caillois calls *agon*—the contest.

6. *Sociability.*—"[T]he game in which one does 'as if' all were equal, and at the same time, as if one honored each of them in particular. . . . Sociability, if one will, creates an ideal sociological world in which the pleasure of the individual is closely tied up with the pleasure of the others."[8] The play form of human interaction may gain in intensity when conducted in unique settings. In camping, one can have intense sociality with a small number of intimates or extensive sociality with a large number of strangers—these levels offer differing meanings.

These types of play actions are not necessarily mutually exclusive, as they do not refer to something inherent in a given activity. What is classified is the predominant form of action in a given social situation. As one shifts groupings, the predominant form of action may shift.

DIVISION OF PLAY AND SEXUAL IDENTITIES

As in everyday life, leisure identities are composed of role shifts and their attendant rituals.[9] When one enters or leaves a given play world, he is expected to assume the appropriate uniform for his identity and to leave others behind if he is to avoid being named a "novice" or "spoilsport" in the context of play, or "insane" in the context of everyday life. The nature of this identity transformation varies with the play form, the "suiting up" phase being institutionalized. Tea parties are for women, football is for men; there are appropriate roles and uniforms provided by the play itself and many of these regulate the relations of the sexes.

From observations over the entire camping play system, it appears that play actions which are normative for women tend to be prosaic, while those assigned to men tend to be dramatic. As Table I indicates, the main exception to this rule is water skiing.

Interviews also indicated that female play actions tend toward the prosaic and practical. When the family groups were asked if they would like anything changed

[7] This is an acting process similar to what Harry Stack Sullivan calls "autistic"—the highly individual meanings of the child's words (see his *Conceptions of Modern Psychiatry* [New York: W. W. Norton & Co., 1953], p. 19). Also see Anselm Strauss, *Mirrors and Masks* (Glencoe, Ill.: Free Press, 1959).

[8] Georg Simmel, *The Sociology of Georg Simmel*, trans. and ed. Kurt H. Wolff (Glencoe, Ill.: Free Press, 1950), pp. 45–46.

[9] For elaboration of the idea that identity essentially involves changes of identity and attendant rituals of rebirth, see Gregory P. Stone, "Appearance and the Self," in Arnold M. Rose (ed.), *Human Behavior and Social Processes* (Boston: Houghton Mifflin Co., 1962); Arnold Van Gennep, *The Rites of Passage*, trans. Monika B. Vizedom and Gabrielle L. Caffee (Chicago: University of Chicago Press, 1960); Kenneth Burke, *Attitudes toward History* (Boston: Beacon Press, 1961), p. 268; and Erving Goffman, *Encounters* (Indianapolis: Bobbs-Merrill Co., 1961), pp. 85–152.

in the forest, men would consistently emphasize play items such as more fish, better boat-launching ramps, and better access roads, while women consistently emphasized comfort items such as better and cleaner toilets, keeping down the dust, and having piped-in water. ORRRC (Outdoor Recreation Resources Review Commission) Report Number 20 has a similar finding; 35 per cent of the males in their sample like to rough it on a vacation, while only 24 per cent of the females do.[10]

Although the data are not statistically significant, they suggest to me that for this group of Americans, play is reflective of the larger social world. Men as little boys grown tall tend toward actions of roughness and dirt; women tend toward actions of softness, cleanliness, and comfort.[11]

However, when the total system is analyzed in terms of different action contexts, the generalization does not hold. One discovers that different action contexts have different models for determining the appropriate roles of tre sexes.

For some action contexts in the play world of camping, the mythical American model of heroic masculinity is dominant. The less dramatic feminine role in this context is to sustain and encourage, to be an appreciative audience for masculine shows. In these roles, the actors have a pattern of responses similar to the first fruits ceremonies of the early Maori;[12] the male returns from his slaughter of game or conquest of adverse conditions to have the meanings of these acts admiringly validated by significant others. For this class of camper, there is an imagined model in American history for the woman's role—the sturdy but gentle pioneer mother.[13]

[10] Eva Mueller and Gerald Gurin, assisted by Margaret Wood, *Participation in Outdoor Recreation: Factors Affecting Demand Among American Adults* (ORRRC Study Report No. 20), (Washington, D.C.: Government Printing Office, 1962), p. 63.

[11] Lewis M. Terman and C. C. Miles, *Sex and Personality: Studies in Masculinity and Femininity* (New York: McGraw-Hill Book Co., 1936). Characteristics of American males are self-assertion; aggressiveness; hardiness; fearlessness; rough manners, language, and sentiment. Characteristics of American females are compassion, sympathy, timidity, fastidiousness, aesthetic sensitivity, and more emotion in general.

[12] Raymond Firth, *Primitive Economics of the New Zealand Maori* (New York: E. P. Dutton and Co., 1929), p. 216.

[13] Henry Nash Smith, *Virgin Land: The American West as Symbol and Myth* (New York: Random House, 1961), esp. chaps. vi and x. His entire discussion is relevant to an understanding of imagined models as motives for assuming a given play role. It is certain that if "playboys" and "playmates" can serve as models directing consumption

TABLE 1

ASSIGNMENT OF PLAY IN CAMPING*

Types of Play	Man's Play	Woman's Play	Joint Ventures
Symbolic labor:			
Fishing	X		
Hunting	X		
Expressive play (*ilinx*):			
Boat racing	X		
Water skiing			X
Scooter riding	X		
Swimming			X
Free play:			
Relaxing		X	
Drawing		X	
Sunbathing		X	
Reading		X	
Swimming			X
Rock throwing	X		
Organized play (*agon*):			
Cards			X
Football	X		
Softball	X		
Horseshoes	X		
Shooting	X		
Subsistence play:			
Camp establishment and maintenance	X		
Food preparation		X	
Fire play	X		
Sociability:			
Conversation			X
Drinking	X		
Singing			X
Catch	X		
Walking			X

* Obviously there are exceptions to these assignments, just as some women are blacksmiths or surgeons in the world of work although in general American society assigns these work roles to males. This table represents the normative play assignments for males and females as I observed them and as the available empirical data suggest.

For other action contexts, the dominant model is apparently companionate marriage and family togetherness. Here, models come from the myths of the media and present peers rather than mythical historical models. It appears that if the homogenization of the sexes is occurring on the recreation frontier, it is so only for this specialized group. To illustrate, I will compare action contexts where expressive play, such as water skiing, is predominant with action contexts where symbolic labor, such as fishing, is predominant.

In the fishing camp, the social balance of the camp resembles a suburban development from 8:30 A.M. to 1:00 P.M.,[14] as it is largely peopled by women and children while the men are fishing. From 1:00 P.M. to 5:30 P.M. the camp is practically deserted. The husband is accommodating himself to his family's demands by taking them driving, rock collecting, swimming, or hiking. From 5:30 P.M. to 7:00 P.M. there are more adult women than men, as the men are fishing again; and from 7:00 P.M. on the sex ratio returns to equilibrium.

Water-skiing camps, in contrast, appear to be a jumble of sex and age categories throughout the day with most family members engaging in similar styles of action regardless of age or sex. Water-ski activity begins about 9:00 A.M., reaching a short peak around noon, with the high point of activity at 3:00 P.M., declining until 7:00 P.M., when there is another short burst of activity, then lessening until action ends at 10:00 P.M.

Land-based activity has a similar pattern, though continuing well into the early morning hours. It is extremely rare for a group of men in the ski camp to line up their equipment and go off without the women.

As spheres of action are not sexually segregated in the ski camp, players are continually visible and on display, requiring a good deal of intricate face work. This inherent tension is apparently lessened by the tacit recognition that expression is the thing, rather than the degree of skill applied to the game. There is, however, a certain emphasis upon grace, agility, and poise. For example, the skier when released from dependency on the machine floats from the momentum provided to make a dramatic, poised, and upright entrance to the lakeside shore and audience. However, ineptness and error appear to receive a measured and humorous tolerance if one is having "fun."

In a water-skiing camp, there is no traditional assignment of roles on the basis of age or sex—the operative and performing roles can be equally filled by anyone capable of a minimum level of dexterity and acuity. In a fishing camp, however, spheres of action are sexually segregated and traditionally assigned. This allows invisibility of respective actions and continues the layer of mystification between the sexes which is brought from the everyday world. Here the male fisherman steals away in the dark of morning with his male conspirators, not returning until afternoon or evening when he resumes his family role obligations. As indicated, contexts of action vary as does the extent of the division of play.

It should be clear that, in family fishing camps, the operative barrier to women's fishing lies not in "objective" qualifications for the role but in the traditional assignment of the role. It takes little imagination to realize that generally fishing demands even less "objective" skill than water skiing, an indication of the importance of collective myth in organizing human endeavor.

Water skiing as a *mixed* activity is most likely due to its recent development with-

styles for some contemporary publics, then mythical historical models can direct other publics. Also see Orrin E. Klapp, *Heroes, Villains and Fools* (Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1962).

[14] All times are approximate, representing an average of what was observed.

out prior assignment to one or the other sex. Certainly, its blooming in the relatively equalitarian environment of the white, middle-class suburban and urban villages contributes to its being defined as proper for both sexes.[15] Fishing, on the other hand, is an activity traditionally assigned to males; however, even here invasion is occurring.[16] One can but speculate as to whether a loss in the activity's male status will occur. Perhaps fishing, if female encroachment increases, will be eventually defined as women's play. If the family trend in campground use continues, there is little question that further female concessions will be obtained—either through a broadening of the camp activity spectrum to provide more female activity opportunities or by further invasion of male activities.

These varying patterns of continuity and change certainly seem to offer an opportunity for testing long-held theoretical notions. Durkheim suggested that the division of labor developed concomitantly with conjugal solidarity and that, where there is weak sex differentiation, conjugal solidarity is weak.[17] Simmel's development of conflict theory centered on examples of male-female relations and the functional importance of this tension for conjugal solidarity.[18] He indicated that social unity requires social diversity. Perhaps the tendency toward homogenization of sex roles (as in "expressive play") creates neither diversity nor unity, with the fusion into undifferentiated, inter-

[15] Other than observation, the following sources give me some confidence in this assertion:

The Outdoor Recreation Resources Review Commission (ORRRC) national survey reports that "Nonwhites camp very little compared with whites" (*National Recreation Survey*, ORRRC Study Report No. 19 [Washington, D.C.: Government Printing Office, 1962], p. 33).

If we use income as an index of class position, there seems to be general agreement that those families somewhere above the poverty line and somewhere below the opulence line are middle class. Where to draw these lines remains a matter of some dispute. Several studies of forest campers indicate that an income range of $4,000–$9,999 is fairly characteristic of campers. Mueller (*op. cit.*, pp. 12–13) reports that most campers in their national sample earn between $5,000 and $9,999.

A study of northern Minnesota campers reports that 65 per cent of their sample had incomes between $4,000 and $7,999 (Marvin Taves, William Hathaway, and Gordon Bultena, *Canoe Country Vacationers*, University of Minnesota, Agricultural Experiment Station, June 3, 1960, p. 8).

A recent and more thorough study of northern Minnesota campers found that 78 per cent of a sample of 424 auto campers and 86 per cent of a boat-camper sample had incomes between $4,000 and $9,999 (Robert C. Lucas, "The Quetico-Superior Area: Recreational Use in Relation to Capacity" [unpublished Ph.D. dissertation, University of Minnesota, June 16, 1962), p. 300.

David Eugene Gray reports that the mean annual income of a sample of California auto campers is $5,276 (*Identification of User Groups in Forest Recreation and Determination of the Characteristics of Such Groups* [Ann Arbor, Mich.: University Microfilms, 1962], p. 138).

I cannot demonstrate "class" differences between water skiers and fishermen; however, the scanty data which are available (in ORRRC Study No. 9) suggest that my notions might be worth future exploration. Regardless of whether they live in urban areas over or under one million population or in rural areas, water skiers tend to be grouped at the upper-income levels. Also, water skiers who live in urban areas tend to be occupied as "clerical, sales workers (other white collar)" (pp. 138, 156). In both urban and rural areas, fishermen tend to be fairly evenly distributed in each of the income brackets above $3,000. Further, craftsmen, foremen, and kindred workers, regardless of where they live tend to fish most. In rural areas, however, fishing seems to be almost as attractive to professional, technical, and kindred workers (pp. 138, 146). It should be noted that fishing is often a solitary pursuit; no water skier, however, performs in solitude.

[16] *National Survey of Fishing and Hunting*, U.S. Department of the Interior, 1960, p. 20. In 1955, one of every eleven women fished, while in 1960 one of every ten fished. However, in both years the ratio of men fishing remained one in four. It was my observation that, in general, whenever a woman fished, she did so with a man; such a statement is not possible in the case of male fishermen.

[17] Émile Durkheim, *The Division of Labor in Society*, trans. George Simpson (Glencoe, Ill.: Free Press, 1947), p. 57.

[18] Georg Simmel, *Conflict*, trans. Kurt H. Wolff (Glencoe, Ill.: Free Press, 1955).

changeable roles failing to strengthen bonds of solidarity. Following these theoretical and observational leads, I would suggest as questions for future research: Is weak sex differentiation in spheres of play associated with strain in conjugal solidarity? Is clear sex differentiation in spheres of play associated with a heightened sense of mutual interdependence and a strengthening of conjugal solidarity? Do sustained familiarity and close contact in play breed greater mutual contempt or greater mutual understanding in the family unit? Are some forms of play "tension generating" and some forms of play "tension resolving"?

In addition to the opportunity for studying how sexual identities and roles vary as one varies the social context, the camping play system offers insight into the function of play for identity formation and continuity.

IDENTITY FORMATION AND CONTINUITY IN PLAY

Identities are variable with time and culture; what is appropriate at one historical juncture may be inappropriate at another. As men attempt to establish and extend their identities, they must work with the often conflicting definitional materials of both tradition and fashion, past and present. Often, resolution of this tension between continuity and change is found in play, where, for example, one generation's work roles become a later generation's play roles.[19] These rituals of reidentification may allow fitting of the life-cycle to alterations in the social cycle.

Rituals of reidentification allow the individual to take the materials at hand and build identities, partly of tradition and partly of the present, to establish himself as a situated object commanding meaningful response from significant others who may be living or dead, imagined, or present. A housewife dropping eggs and a pinch of seasoning into a packaged cake mix, the camper modifying his campsite with axe and saw, a third-generation son of an immigrant rediscovering a traditional old-country festival—all are essentially practicing rituals which symbolize ties to former available roles, thereby providing placement in the present by reference to the past.

Herberg suggests that the immigrant religion forgotten by the second generation is rediscovered by the third generation. He believes that this has an important function for the individual's identity. The old religion, by naming, places the individual within the general social context.[20] Perhaps for the old-generation, white, Protestant, middle-class person, symbolic labor with its rural connotations has a similar function in linking the present to the past by providing the opportunity for playing out values no longer widely accepted or possible.[21] These are persons, described by Burke, who believe that "the need for struggle is linked to the need for justification . . . [by] the importance of earning what we use . . . its psychological value to [the actor] resides precisely in the amount of labor caused, not the amount saved."[22]

This would partly fit some of my observations of play in the forest, where it

[19] Gregory P. Stone, "American Sports: Play and Dis-Play," in Eric Larrabee and Rolf Meyersohn (eds.), *Mass Leisure* (Glencoe, Ill.: Free Press, 1958), p. 258. His discussion of the transformation of work and play is most relevant.

[20] Will Herberg, *Protestant-Catholic-Jew* (rev. ed.; Garden City, N.Y.: Doubleday & Co., 1960), pp. 28–33.

[21] Gerhard Lenski, *The Religious Factor* (Garden City, N.Y.: Doubleday & Co., 1961). His data from the Detroit area indicate that Jews and Protestants do embody the "spirit of capitalism" in their values, attitudes, and beliefs. The urban tradition of Jews would eliminate the forest as an important play area for them, while the rural, small-town tradition of Protestants would enhance the forest as a play area.

[22] Kenneth Burke, *op. cit.*, no. 7, p. 124.

seemed that the *homo faber*[23] reaction to the packaged goods and roles of industrialization was one means of manufacturing role continuity. For example, at some point in time a camper may carve from the forest a crude campsite or modify an existing established one. Gradually, over time, others discover it, use it, and make their own innovations. The campsite simply grows, the product of many hands, with each person seeing it as a monument to his pioneer skills. Long after the original pioneer has moved on to more virgin territory, as did the mountain men and trappers of the West, the climax phase of the innovated campsite is reached. This is when the administering agency gives official recognition to the site by placing a table, building a fireplace, and giving it a name.[24]

Of the nineteen campgrounds sampled, only four were the controlled result of administrative initiative; on the other fifteen campgrounds the agency has followed after the campers and attempted to order the campground development.[25] However, even in these "planned" campgrounds, camper pioneering has continued with spillover and isolating campsites much in evidence at seven large campgrounds—Little Cultus, Big Lava, Little Lava, Todd Lake, Elk Lake, Scott Lake, and Rainbow Bay.

The campers' predilection for constructing elaborate boat docks, shelves, coolers, lean-tos, fireplaces, and so forth is a constant source of dismay to forest managers attempting to maintain "forest aesthetics" in their campgrounds. It is hard to believe that such construction efforts are guided solely by utilitarian demands. Most of the campers already have prebuilt coolers, storage chests, and so forth; also it appeared that completion of construction often coincided quite closely with the family's departure. My suggestion is that such forest "make-dos" have an intrinsic value far beyond their utility value and that part of the satisfaction is the sense of independence from the present and continuity with the resourcefulness imagined to have occurred in the past.[26]

The *homo faber* phenomenon was also observed in the campers' apparent enjoyment of cutting, hauling, and burning wood far in excess of their simple utilitarian needs for warmth and cooking. Of particular interest were six retired men who announced, with a touch of pride, their enjoyment of the various fire duties, particularly the chopping. Most of them were observed to provide a wood supply far beyond their needs. One of them gave me an elaborate description of how he selected a snag, cut it, and pulled it by his truck to his campsite, where he sawed and split it. Could this be a further clue as to ways one manifests his usefulness to self and others, through the tangible manufacturing of identity articles? These men represent a generation to whom work was both rewarding and intensively pursued. Their life-cycle, however, is caught in a social cycle which defines the elderly as a social problem. The elderly actor, oriented toward values of work and notions of scarcity, perhaps substitutes fuel production during his play to allow expression

[23] Hannah Arendt, *The Human Condition* (Garden City, N.Y.: Doubleday & Co., 1959), pp. 72–146.

[24] Four such camps were discovered in this study: two were in the process of being established, one at Horse Creek and one at the Green Lake trail head; and two were established and named, Big Cultus Wilderness Camp and sections of Little Lava Camp. Horse Creek was in the Willamette National Forest, Oregon. All others were in the Deschutes National Forest, Oregon.

[25] The slow rise of agency service to meet public demand is more a matter of traditional reluctance than of administrative sloth. Official campgrounds were originally established not as a public service but as a means of concentrating people and thereby protecting the forests from man-caused fires.

[26] A "primitivistic" response to the dominant values and artifacts of one's own time should not surprise us as it seems to have a fairly long tradition in the realm of social thought (see Arthur D. Lovejoy, Gilbert Chinard, George Boas, Ronald S. Crane [eds.], *A Documentary History of Primitivism and Related Ideas*, Vol. I [Baltimore: Johns Hopkins Press, 1935]).

of values which are cut off in other contexts of the abundant society.[27]

The opportunity to link past and present through one's play is rarely found in expressive play activity. Expressiveness by its transitory nature is dependent upon immediate recognition by a jury of peers. One cannot hold the particularly daring and exciting act to show others at some other time—it is of and for the present, dying at the instant of its creation. Only the exceedingly spectacular act will be endowed with life by being named and exchanged by an audience who may continue this act's existence until such time as a more spectacular act supplants it. But such acts are most often beyond the direct control of the expressive performer. In expressive play, cues come from the present; in symbolic labor, cues come from the past.

Reuel Denney documents the problem of durability for expressive play forms in his discussion of the cycle of play in hot-rodding.[28] He traces the development from a small group of dedicated hot-rod amateurs, on the fringes of legality, to hot-rodding as a public spectacular requiring a minority of professionals who work at play. Perhaps expressive play phenomena which lend themselves to spectacle will have more of the tension of fad and thereby not offer *homo faber* involvement. But play such as symbolic labor, which does not lend itself to the drama of spectacle, can retain durability. This stability may guarantee the building of continuity into play identities. If such is the case then the forms of play represent neither patterns of frivolous mass diversion nor serious personal escapes, but become meaningful arenas of human action, as linked to the dilemma of finding permanence in change as are the arenas of war, work, and love. The study of play, by exploring the social meanings of different play forms, may uniquely enhance our understanding of this dilemma.

CONCLUSION

I have attempted to demonstrate that forest campgrounds offer arenas where human players and their dramas reflect the issues of differentiation and equality, continuity and change. In such locations, where actors freely choose the games they play, the social scientist has a unique opportunity to test important theoretical questions of the "middle range."[29] It is my hope that we will not allow our academic Puritanism to blind us to the opportunities afforded by the study of play. The line between reality and unreality, work and play, is too thin and too tenuous to assume that gainfully occupied man supersedes playfully occupied man in our measures of the universal human condition.

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[27] Gregory P. Stone, "American Sports," *op. cit.*, pp. 256–57. He suggests that the news of spectator sports and the team loyalties learned early in life may provide a sense of social and personal continuity in a changing world. See also my "Nature as Symbol and Expression in American Social Life: A Sociological Exploration" (unpublished Ph.D. dissertation, University of Minnesota, 1964), chaps. v and vi. A study of three styles of camping indicates that adults attempt to retain the quality of nature experienced early in life.

[28] Reuel Denney, *The Astonished Muse* (Chicago: University of Chicago Press, 1947), pp. 138–56. See also distinctions between play and dis-play made by Gregory P. Stone, "Some Meanings of American Sport," in *College Physical Education Association*, 60th Annual Meeting (Columbus, Ohio, 1957), pp. 6–29.

[29] Robert K. Merton, *Social Theory and Social Structure* (Glencoe, Ill.: Free Press, 1961), p. 5. These are "theories intermediate to the minor working hypotheses . . . and the all-inclusive speculations comprising a master conceptual scheme."

COMMENTARY AND DEBATES

Social Analysis as a Sociological Vocation[1]

Elephantiasis, an abnormal enlargement and thickening of tissues, occurs in both the animal and the plant kingdoms. The term, it seems to me, might well serve to point to a highly similar sociological phenomenon: the abnormal expansion of one role into the action-space normatively allotted to others. There are two kinds of role-elephantiasis: In the first, time, energy, and means normatively defined as belonging to one role are used up by another, as when executives take their work home for the weekend in contemporary suburbia. Socially even more debilitating is the second kind in which norms of behavior proper in one role are applied in another where they are not proper. Treating one's wife as one's secretary, or one's secretary as a wife, that is, being functionally specific where one ought to be diffuse, or diffuse where one ought to be specific, are cases in point.

Role-elephantiasis can be measured in several ways. For instance, a sample of a particular population might be taken to determine what is seen by it as the proper allotment of time, energy, and means to various roles, and what norms the population expects actually to guide behavior in the roles under study. Then one can compare the actual allotment of time, energy, and means among these roles by conforming and deviant actors. The norms the deviant actors believe in, and those they adhere to, can be ascertained through depth interviewing and participant-observation. It is advisable to determine whether the deviant actors have a sense of guilt when they are afflicted with role-elephantiasis. Otherwise, statements about the intrapsychic effects of the violations of norms will not be supported by evidence but will be assumed on the basis of the existence of social censure. The problem of measuring the distribution of energy among roles (as distinct from that of time and means) poses some severe problems, as does the distinction between significant and insignificant transgression of the role-boundaries. Future research, it is hoped, will clarify these matters.

Many a sociological article opens with such a definition of a new concept and a discussion of methods to be employed to measure it. This is then frequently followed by presentation of some data relevant to the new concept and relating it to familiar sociological variables (e.g., "the distribution of elephantiasis by age and sex in cities with a population of over one hundred thousand"). Most sociologists, the author included, feel that such combination of theory and methods is the very foundation on which sociology as a science ought to be built and is in fact being constructed. But many of us also feel that something is lacking. This article is not devoted to conceptualization or measurement of what is lacking, but to outlining a suggestion of how to add the lacking ingredient, to make a fine mix even better. This is only in part a question of adding a new substance; more important are changes in institutions in which sociological training is provided, where the new spirit is to be given flesh and blood. Hence this brief discussion necessarily combines some "high-level" comments on the nature of sociology with some "low-level" ones, touching upon the mundane questions of

[1] I am indebted for comments on an earlier version of this article to Martin Wenglinsky, Ethna and Edward Lehman, Charles Kadushin, and in particular to William J. Goode.

sociological training and curriculum. Sporadic outcries for a sociology different from the prevailing one will leave little impression unless the trouble is taken to chart the course through which reform is to be made.

What is lacking is *social analysis*, the *systematic* exploration of societal issues, that is, concern with the methodological questions of sociological analysis of the great issues of our age, which invariably involves the study of macroscopic units. The subject of social analysis, though, is the issues, not the sociological units or building stones; the focus is on the instruments to be utilized to elevate the analysis of societal issues, to improve on amateur, intuitive, or journalistic sociology. Traditional training in sociology is no more a preparation for social analysis than training in biophysics or biochemistry is a substitute for medical training. Social analysis requires special training as well as distinct methods, knowledge, and a professional tradition. Social analysis requires more than a simple application of an existing body of knowledge to the study of a set of problems; it is also a question of studying the problems that application of sociology engenders. When sick, one would hardly exchange treatment by one M.D. for that of two Ph.D.'s in biology. Hence, the call for social analysis as a new element of sociological training is a call for *the professionalization of sociology*—for adding to sociology as a science, as the institutionalized desire to know, the systematic concern with application of knowledge, the institutionalized desire to help.[2]

Social analysis is not applied sociology; it differs from applied sociology in the way second-order concepts differ from first-order ones. The focus of applied sociology is, as a rule, a low-level, concrete social problem. Social analysis should concern itself (*a*) with the problems applied sociology raises, and (*b*) with the *systematic* application of sociology to societal issues, desegregation for instance, not excluding but hardly focusing on how to improve the management of a supermarket or a marriage.[3] The "client" is society.

The *subject matter* of social analysis is all of substantive sociology; but social analysis as a discipline is not to replace the fields of political sociology, race relations, or the study of stratification, but is to deal with *the generic methodological, intellectual, and professional problems which the substantive sociologies raise*. Each of the substantive fields combines—in addition to information about the subject matter—three and not two essential elements. To study politics one had best know something about politics, draw on a general theory and methodology, *and* be prepared to handle the generic problems of substantive analysis. The same problems would reappear if you were to study other substantive fields, for example, the sociology of religion or criminology, but would not arise if you were engaged in sociological theory per se or pure methodology.

What is the substance of social analysis and what generic problems does its study raise? The focus of social analysis, and its *raison d'être*, are the problems of the age, the application of sociology to the understanding of society, its major subcollectives, and a society's place in more encompassing communities. Biochemistry views the blood as having varying chemical compositions; medicine sees it as infested with illnesses. One day—when our knowledge of hematology is much more advanced—the distinction might disappear; meanwhile somebody had better be concerned with how to cure illness, using the very partial biochemical information available. *The* meth-

[2] Talcott Parsons, "Some Problems Confronting Sociology as a Profession," *American Sociological Review*, XXIV (August, 1959), 547-59.

[3] Cf. the concept of social analysis to that of "informal sociology" advanced by Herbert J. Gans, "'Informal Sociology': A Proposal for a New Publication," *American Sociological Review*, XXIII (August, 1958), 441-42.

odological question of medicine is hence how to act under partial information. Similarly, sociological theory and research slice society into social systems, role sets, and reference groups; social analysis is concerned with applying such concepts to the evolution of a world community, the redistribution of social wealth, efforts to advance the growth of civil rights, the development of "have-not" countries, etc. Sometimes the transition from sociological theory to social analysis is fairly straightforward and simple, often it is not; but always the problem of applying our fragmented knowledge to social action needs to be systematically studied.

Our purpose here is to call attention to the need to explore the distinct methodological and conceptual problems of social analysis rather than solve them. It will take a decade of efforts, by many members of the profession, before much can be said on the subject. Meanwhile, what might be referred to as the "methodology of first approximation" illustrates the kind of special training social analysis requires. In general, we quite correctly train students to achieve higher levels of precision by drawing better samples, using more refined measures, etc. As a consequence the trained sociologist often shies away from major segments of social data because for one reason or another, be it security considerations or budgetary ones, he cannot obtain the kind of precise data we taught him to look for. The field of analysis of societal problems is thus often left wide open to social commentators who have no methodological training at all. We should develop and teach the methods to be applied when information is fragmentary and vague, as it so often is, because the trained sociologist can still do much better—especially when he is trained to face this problem—than the uninitiated social observer. (Training in the use of first-approximation methods will include instilling in the trainee a commitment to his future clientele, to warn him of the limits of the data and the tentativeness of the conclusions.)

Obviously I do not share the feeling, expounded by C. Wright Mills in this context, that our investment in methodology was largely wasted or that our bets on general theory are misplaced.[4] On the contrary, it seems to me, our efforts are starting to pay off handsomely and that *hence* we are more ready than we ever were to apply our theories and methods to major societal issues and be systematically concerned with the problems such application raises.

A hardly novel historical approach to sociology serves to emphasize this point.[5] We started with grand social theories, formulated in emotion-laden terms (e.g., progress), covering no more and no less than all of history and all of mankind; we began by flying so high on the verbal trapeze that most of our propositions could not be pinned down; and those that could be often did not withstand empirical tests. Our grandiose designs collapsed.

Then, we foreswore high-jumps; we preferred to advance step by step, even if it should take us a hundred years to learn to walk firmly, rather than engage again in breathtaking but also neck-breaking gymnastics.[6] We sharpened our tools on the radio listening of housewives and focused our concepts by observing small groups of college sophomores chatting before a one-way mirror. *Such a concentration was essential for a transition period;* but behavior which is quite suitable for student days becomes an adolescent fixation when it dominates the behavior of a mature man. Sociological theory ought to be further extended and methods of col-

[4] C. Wright Mills, *The Sociological Imagination* (New York: Grove Press, 1957), esp. p. 50 and pp. 74–75.

[5] Cf. Ralf Dahrendorf, "Toward a Theory of Social Conflict," *Journal of Conflict Resolution,* XI (1958), 170–83.

[6] Lewis Anthony Dexter, "A Note on Selective Inattention in Social Science," *Social Problems,* VI (Fall, 1958), 176-82.

lecting and analyzing data improved, but our wings have sprouted; we are now ready to fly. It would be an overreaction to our earlier misadventure to remain earthbound to a restrictive interpretation of our discipline, to delay a new test flight of social analysis.

Another reason we, as a profession, shy away from social analysis is our fear of value judgments which, we sense, are more rampant in social than in sociological analysis. Weber's bequest to us, we keep telling generations of students, is the separation of understanding from criticism, which is the basis of all rational, and hence scientific, analysis. But—we do not always remember to add—Weber carefully distinguished between a *wert-frei* and a *wert-los* approach (between one free of values and one without values or literallly valueless). A *wert-frei* sociologist holds his values in abeyance while he follows the guidelines his data reveal, allowing them to speak rather than imposing on the data the findings his heart desires. Thus he is "free" from values while engaging in the procedural act of science. But this is not to imply that the work of the very same sociologist needs to be *wert-los*, either in his professional or his citizen role.

In his professional role, the sociologist, like any other scientist, must choose his research topic by non-scientific, normative criteria. I say "must" because there are no intrinsically scientific criteria for this selection. One might say we are out to fill in the lacunae of sociological theory, chart the unknown areas left on the map; we do not know what we shall find, but we know where the uncharted areas lie. But our map has more unknown than known spots, and how is one to tell, on what scientific consideration, which to chart first? Moreover, since so much of our charting is tentative, rechecking the known is as important as exploring the unknown. Hence the list of topics a sociologist can legitimately choose from for his study is as long as the list of topics there are. Our selection is thus invariably determined by intellectual curiosity, aesthetic values, fads, career interests, availability of funds, leadership of senior colleagues, and what not. But there is nothing intrinsic in sociology as a discipline that makes the study of macroscopic units less respectable than microscopic ones, now that we are equipped with the basic skills and tools necessary to handle both kinds of units.

But social analysis requires macroscopic analysis. Most of the problems of the age are those of large collectivities or are directly affected by them; in issues such as accelerated desegregation, redistribution of the national income in favor of the underprivileged, or averting nuclear war, the federal government and the national society play a critical role. The infrequency with which social analysis topics are selected derives not from any inner light that sociology sheds on research but from largely extrinsic shadows. It arises in part from the tendency to award more status to basic than applied research, and in part from the hangover of poor social analysis, the pangs of which are still with us. Actually by now social analysis could be fully respectable; the taste of past generations' brew should no longer hinder our distilling a new one.

Two more arguments in favor of sociology as it is (and the status quo in sociological training) need to be examined. It is said that sociologists, by learning to walk, will find out how to fly. You can learn from the fruit fly, it is correctly suggested, new laws of genetics that apply to all animals and plants. Similarly, we can derive from sophomores' chitchat universal laws of interaction which enrich our understanding of social behavior in general. But while it is true that in this way we can learn the "universal" elements of our theory—all the universal chemical characteristics of water are represented in any drop—we cannot study the emergent properties of com-

plex units in non-complex ones.[7] We will not learn much about the anatomy of elephants by studying that of fruit flies. Hence, while we ought to continue to study small groups for their own sake and for the light they cast on social behavior in general, *we ought to invest more of our resources in macroscopic sociology.*

But, it is said, as a second line of defense in favor of our present low investment in social analysis, you cannot direct scientists and tell them what to study. If sociologists find race relations an unrespectable subject, unless it can be used to perfect survey methods or to redefine the concept of prejudice, what can we do? What we can do is to realize that the distribution of scientific resources is not random, does not follow a laissez-faire pattern, and is "interfered with" regularly anyhow.[8] The distribution of sociological manpower is directly affected by the advantage of required courses, which as a rule include theory and research techniques over optional courses; by Ph.D. committees that approve and encourage some subjects and discourage others; by foundations and federal agencies—which we advise—who support some subjects to the neglect of others; and by space awarded in our journals, as well as attention granted at regional and national professional meetings, to some subjects over others. All these are occasions where theory and methodology are celebrated while social analysis is given, at best, second-class citizenship.

The institutions of sociology are lagging in this matter far behind many leading sociologists. Our journals are a case in point. They are the major windows through which, month after month, we display our hardware before prospective clients, competitive stores, and one another. Even a brief perusal of the *American Sociological Review* and the *American Journal of Sociology* of the last decades will show that theoretical and methodologically oriented articles are predominant; social analysis is little dealt with. Not that sociologists have not written outstanding essays on the issues of the day, without using them to redefine concepts or demonstrate correlations. But these essays did not find their way into our professional journals. Thus Parsons' article on McCarthyism was published in the *Yale Review;* Riesman's "Abundance for What?" in the *Bulletin of the Atomic Scientists;* Gans's studies of our urban problems in *Commentary;* Shils's insights into the role of intellectuals in new nations appeared in *World Politics;* Merton's classic "The Self-fulfilling Prophecy" in *The Antioch Review;* etc., etc.

One might ask, are not these journals precisely the place where such articles ought to be presented? Is it not the function of publication in these journals to bring sociology to the attention of well-educated segments of society? While this is of course true, the almost exclusive publication of such articles in lay journals puts them in danger of being lost to sociological tradition and training. They are not readily available to most members of our profession and are not easily encountered by new students or members of other disciplines who skim our journals. Nor are the disciplinary problems raised by these essays systematically explored; in short, they are only indirectly a part of sociology, rather than in the front row.[9]

[7] This point is elaborated in my *A Comparative Analysis of Complex Organizations* (New York: Free Press of Glencoe, 1961), pp. xii ff.

[8] See Robert Gilpin and Christopher Wright, *Scientists and National Policy-Making* (New York: Columbia University Press, 1964). See also my "National Guidance of Science," *The Moon-Doggle* (Garden City, N.Y.: Doubleday & Co., 1964), pp. 42–70.

[9] My practical suggestion is that some space in our journals be given to social analysis essays; there is really no danger that the nonprofessional publications will be deprived. Similarly, social analysis books written by sociologists (or on subjects sociologists ought to write about) should be systematically reviewed, and review essays—dealing with the generic problems they raise—be invited.

Probably the loyal sociologist will find it hard to accept, as I do, that our discipline devotes less energy, time, and means to social analysis than do other social sciences. Industrial development, for instance, is a major preoccupation of economists, while sociologists, who have at least as much to contribute to this field, have devoted comparatively little attention to it. Similarly, psychologists have made major contributions to the study of war and peace (see for instance the work of Charles Osgood, the recent president of the American Psychological Association). We have at least as much to give to this vital area as our colleagues in psychology, but here again we have given little.[10]

Finally, sociological scientism is revealed in the aloof attitude toward social action of many members of our profession. This is a severe case of elephantiasis in which the professional role of the sociologist has made deep inroads into his role as a member of the educated elite of the community. This is not just a question of being a bad citizen but of not living up to a special social obligation we have as persons who know society expertly. To indicate more clearly what I have in mind, let me point to another helpful (for social as well as sociological analysis) term, that of role pairs. Role pairs are roles which appear frequently together in a society, in the sense that they are carried out by one and the same actor. The importance of such combinations is that they provide the most effective means of communication known between two roles—personal union. They also allow economy of resources, such as that found in the housewife-mother pair, security and elevator-boy combination, teacher-researcher, doctor-medical professor, etc.

The role pair of sociologist-intellectual is a particularly effective one. Not that all sociologists were ever intellectuals or vice versa, but there seems to have been a much higher degree of overlap in earlier generations. The growing tendency to disassociate the two roles is particularly regrettable because the virtue of such role combination is greater now than it used to be in the days when it was more common, for now we command a body of theory and methodology as well as a store of validated knowledge about man-in-society which can provide much-needed background for speculation about society.[11] The social analysis of Daniel Bell, Lewis Coser, Nathan Glazer, David Riesman, Dennis Wrong, and other contemporary sociologists who fill this role pair is much more hardheaded, soundly based, and politically sophisticated than that provided by earlier generations of social analysts or by their former college mates who majored in English literature and still interpret the American scene in the light of moods revealed in *Moby Dick* or "understand" the Soviet Union because they suffered with Dostoevsky and Pasternak.

As a discipline we do not encourage, or at least do not train for, the sociologist–social-commentator pairing of roles. In earlier days the clergy and radical movements provided the sparks that fused sociological training with social concern. Today, in the age of specialization, more and more sociologists feel that what is proper behavior in their role as scientists is the proper behavior in their community role as well; the only way they face a social problem is through the lenses of the-

[10] Marvin B. Sussman, "The Social Problems of the Sociologist," *Social Problems*, XI (Winter, 1964), 215–25.

[11] For discussion, from various perspectives, of how the role pair of sociologist and social analyst operates, or fails to operate, see Alvin W. Gouldner, "The Myth of a Value-free Sociology," *Social Problems*, IX (Winter, 1962), 199–213; and Alfred R. Lindesmith, "Social Problems and Sociological Theory," *Social Problems*, VIII (Fall, 1960), 98–101.

For discussion of how the two roles inform one another, see Robert K. Merton, "Social Problems and Sociological Theory," in Merton and Robert A. Nisbet (eds.), *Contemporary Social Problems* (New York: Harcourt, Brace & World, 1961), pp. 697–737, and Robert K. Merton, *Mass Persuasion* (New York, Harper & Bros., 1946), pp. 185–89.

ory and methodology. Civil defense, for example, becomes a subject for a study of attitudes ("people who fear war more are also more in favor of fallout shelters") or an occasion to try out a new computer program in mass dynamics.[12] The sociologist's scientific role is pre-empting time, energy, and resources that belong to his role as intellectual, as one who is committed to societal issues and expresses his concern about them more effectively than other observers since he knows more than they about the society he is commenting upon. Thus he not only is against nuclear war, but applies his knowledge of society to understand why nations become inflexible in the face of such a danger and freeze rather than act, and shares his analysis with those who seek to reduce the danger through political action but lack the benefits of the sociologist's training and expertise.

Finally, as part of the liberal arts tradition, social analysis can broaden the horizons of the undergraduate student and prepare him more adequately for life in modern society. Sociology on this level should contribute to the development of the skills and understanding necessary for the student as citizen and not use the college mainly as a recruiting ground for professional sociologists.

How can social analysis be restored to its due place? How can we repair the roles of sociologist and intellectual? How can one advance the discipline of social analysis? Answering these questions requires a somewhat detailed examination of the way sociology is taught on both undergraduate and graduate levels and an outline of a program for modifying these teaching patterns. To this end, a number of suggestions are offered here. However, the detail of what follows is not crucial; it serves mainly to illustrate what institutional cloak social analysis might wear. One might find one of these suggestions or even all of them unsuitable and still persist in the search for institutional foundations for social analysis. What one cannot forego is the search to cast new molds to hold the new content, and the effort to bring the collective forces of the profession to bear where departments, set in their ways, are not engaged in enough innovative drive on their own.

On the *undergraduate level*, what first needs revision is Sociology I, both in terms of the material used for teaching and the way it is taught. Judging by the textbooks most commonly used, sociology is still introduced as a set of theoretical perspectives and concepts around which some substantive material is arranged as illustration. Thus many textbooks open with such concepts as role, norms, and interaction, or personality and social system. The social issues behind these conceptualizations are, as a rule, most successfully concealed; sometimes even the writer himself seems unaware what these concepts have been molded for. We rarely speak up and tell the student that what we are really talking about when we distinguish among ends, means, norms, and conditions is man's freedom: that man's action is not determined by forces beyond his control, whether they lie in the relationship between his parents, his genes, the shape of his head, or the texture of his hair; that hence man is ultimately responsible for his conduct, though society, by prescribing the proper behavior (or norms), makes it painful for him to follow his inner voice when its suggestions do not coincide with the expectations defined by his "role." Similarly, we introduce the student to the difference between primary and secondary relations, as if the distinction were the main point, and discuss the historical transition from *Gemeinschaft* to *Gesellschaft* as though it were but one illustration among many of the usefulness of two basic concepts, and not a key to understanding the development of

[12] Robert S. Lynd, *Knowledge for What?* (Princeton, N.J.: Princton University Press, 1939), and Peter L. Berger, *Invitation to Sociology* (Garden City, N.Y.: Doubleday & Co., 1963), esp. chap. iv.

modern society. We tell the student that social structures are composed of actors who have similar, superior, or inferior statuses, as though the essence of stratification is that people are stratified; the inability to produce an egalitarian society and the difficulties raised by efforts to approximate it become marginal, if they are mentioned at all.

Most students who take "Soc. I" never take another "Soc." class, and even fewer become professional sociologists; thus to most of those who take our introductory classes, we tell little about the society they live in or about the essence of sociology. For many of them sociology will remain forever largely a distasteful conceptual machinery which one masters for the exam and forgets soon after.

The situation is further aggravated by the way introductory classes are frequently taught. While there are many exceptions, the tendency is for it to be taught in "sections," by graduate students or young instructors who tend to be, though by no means always are, in a stage of their professional development at which their respect for the conceptual framework they so recently mastered is greatest, and their willingness to question the tradition followed by the majority of their senior colleagues—by engaging in social analysis—is particularly small. Moreover, since the number of instructors needed for introductory sociology is large, and the number of sociologists who are not just scientists but also social analysts is small, most introductory classes are more classes in an esoteric language than channels for the communication to students of new ideas expressed by this language. Thus teaching further divorces the textbooks from social issues to make more room for a technical apparatus that will serve largely the very small minority of students who will become professional sociologists.

What is needed is a different kind of textbook and a different mode of teaching. The textbook should be built around issues, and the concepts should be introduced "in passing." Thus once we tell the student about the emergence of the modern society from the traditional one, we might call attention to the terms that sociologists use, when speaking technically, to refer to these types of societies and to their components. Once we have analyzed the social foundations of justice, we might consider the concept of "status inconsistency," and after we have explored the ways man learns to control himself, we might call the process "socialization" and mention "affective neutrality," etc. In this way we shall leave a much richer inheritance for those many students who do not pursue sociological careers, rather than continue to train generations of students alienated from sociology.[18]

Another approach to the same problem might be a course on American society to replace "Introductory Sociology" or supplement it, as Soc. II. But again care is to be taken to see to it that this is really taught as a course in social analysis, distilling for the students what sociological research and analysis have found about American society and not just presenting

[18] To be successfully applied, such an introductory text will have to be used by one of the leading members of the faculty, who has had a chance to see in his own work the limitations of overemphasis on methodological subtleties and conceptual hair-splitting; and the importance of the problems sociology ought to help us to understand. (The student will be fully introduced to the distinctions between a "cohesive" and a "primary" group, and to the many mistakes one can make in drawing a sample, and similar trade secrets, in more advanced classes.) The more committed the teacher himself is to social analysis, all other things being equal, the more natural appeal will he have to the undergraduates. Since there are not too many such faculty members available for introductory teaching—each year probably only one can be found even in larger departments—they might give one weekly lecture to all the introductory students, while teaching assistants would serve as discussion leaders of small sections, into which the class would break for additional meetings during the week. These teaching assistants should sit in on the grand lecture.

the material on life in America as illustrations of the pattern variables, or of distinctions between social and cultural systems, and the like. (It might be a good idea to have such a course, repeated on a higher level, for first-year *graduate* students.[14] We produce too many sociologists who know their discipline better than their society. Not that every student of biology should know some medicine, but he should at least have handled a warm body, not just slides and skeletons.

Second, the graduate course in American society would serve as the introduction to the substantive sociologies, so that a student planning to specialize in political sociology or stratification, education sociology, or some other field, would know more about those areas in which he will not specialize without having to take a course in each. Hence, it seems desirable that the course in American society (or social analysis) enjoy the same status as courses in general theory and methodology should; that is, it should be required. At least it should be offered as an optional substitute for the second (often third and fourth) methods and statistics courses which departments of sociology tend to require. The special importance of this will become evident immediately.

The introduction to the social analysis course (and/or the American society course) might well serve as an introduction to a sequence (or specialization) in social analysis, much as a first-year course in theory provides both a general background for all graduate students and can be taken as a first step in a theory specialization. At present, most departments of sociology are not systematically concerned with training and nourishing the talent that could assume the sociologist-intellectual role pair, which includes newspaper and magazine editors, journalists, commentators, non-fiction writers, political advisers, social critics, and teachers of social analysis. To be sure, most departments have several substantive courses, but these are usually geared to the application of theory or methodology to some substantive area and hence best serve sociologists who wish to become specialists in a particular field. But little attention is given to analysis of social issues, especially not to the generic problems social analysis raises. As a consequence, we do not train the editors, journalists, political advisers, etc., of tomorrow, though of course it does happen that a sociologist—despite his training (or often interrupting his training)—becomes one of these. As a consequence the role of editor, journalist, etc., is largely taken over by English literature majors, political scientists, and lawyers. At the same time, we do not hold on to the talent that has such a political, social-analytical potential. While I am not aware of a nation-wide study to support my impression, it seems that students with such a potential are not attracted to sociology as a graduate major, and if they choose it are not likely to stay with it, and if they stay with it are not likely to realize their social-analytical potential. Since at the same time students who are attracted to more quantitative fields increasingly choose "harder" ones than sociology, we are losing at both ends of the scale and are, it seems, drawing an increasingly poorer quality of new students.[15]

The methods courses are often the symbol of the screen that keeps the potential social analyst from sociology. Our methodology requirements have become more and more expansive and exacting, which is fine. But we ought to distinguish more sharply between training in methodology that provides the necessary background for being able to *consume* sociological products—for

[14] Such a graduate course is especially important since many students who take graduate sociology have taken little or no undergraduate sociology.

[15] See Elbridge Sibley's discussion on the "Calibre of Students" in his *The Education of Sociologists in the United States* (New York: Russell Sage Foundation, 1963), pp. 23–25.

example, learning to read a multivariable table—and that needed to labor in those sociological vineyards that involves the command of quantitative methods. A social analyst must be able to "consume" quantitative sociology a taste which can be acquired in a one-semester course; beyond that he should have the option of pursuing subjects closer to his heart and more in line with his specialization than quantitative methods.

A sequence of social-analysis courses would include (beyond a methods and a theory foundation) a large number of substantive courses, including a greater than the usual number of credits in other departments, such as philosophy, literature, or political science.[16] Finally, we will have to develop a disciplinary course in social analysis, which will pay special attention to the qualitative methods and distinct problems of studying macroscopic units, and the problems of inference from partial information. Here the student will learn to face the problems of evaluation (or value judgments) which a social analyst is likely to find more difficult than other sociologists, and the ethics of advising and consulting. The social analysts are thus in essence those trained to apply the tools and findings of sociology to society, a task now left to outsiders, neither trained nor equipped to crack our codes, or to insiders with little systematic training in the methods of application in general and of social analysis in particular.

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[16] It might be necessary to require that a student who takes an introductory course in another department will also take another, more advanced course in the same department to provide a deeper understanding of the sister discipline and to prevent accumulation of "easy" credits by taking several introductory courses in various departments, without probing in depth any one discipline.

Comment

With Etzioni's central thesis and much else besides it is easy to agree. He is sensitive to a potent trend in contemporary sociology and gives renewed and vigorous expression to it. He has observed that the practitioners of our discipline, excellent as they are, have often contented themselves with trivialities and have not taken into their purview the larger problems that surround them. Like Isaac Newton—but unlike him in impact—they have been picking up an occasional pebble or shell on the seashore while the great ocean of society lies all undiscovered before them. Etzioni does not doubt the importance of the pebbles and shells—they can be fused together into an edifice of science—but he reminds us that the ocean is still there, clamoring for attention.

Etzioni has, in fact, great faith in the current condition of sociology and even more in what it promises for the future. As good as it is, something is missing that would "make a fine mix even better." Indeed, it may even be said that some reform is needed. The answer is something he calls "social analysis," which would add application to knowledge and institutionalize "the desire to help." Knowledge, in short, is not enough. We must discover high-level applications for what we know, we must take seriously Robert S. Lynd's famous question—*Knowledge for What?*—and we must bring our resources to bear upon such general social problems as desegregation, the development of a world community, the redistribution of wealth, and the growth of civil rights. Otherwise our activities are sterile and our instruments useless. One is reminded of Lotze's remark about logic: that the constant whetting of an axe is apt to be a bit tedious if it isn't proposed to cut anything with it. Etzioni wants to terminate the tedium and begin to cut.

With most of his sentiments I am happy to concur. Some questions remain, however, about which it is perhaps appropriate to make a comment or two.

In the first place, Etzioni is convinced that sociologists "know society expertly" whereas the "uninitiated social observer" and "social commentators" do not. I should like to share his conviction but unfortunately a certain skepticism intrudes. Few if any sociologists are superior in social analysis to such publicists as Walter Lippmann and James Reston. It is useful, if embarrassing, to recall that both of them are innocent of sociological training. Mr. Lippmann graduated from Harvard before there was such a thing as a Department of Social Relations and Mr. Reston did his "graduate work," so to speak, in the sports department of the Springfield, Ohio, *Daily News* and as publicity director of the Cincinnati Red Sox. It is my own impression that most of us, as sociologists, tend to denigrate the enterprise and accomplishment of publicists in general, and in any event it is difficult to deny that the best of them are well qualified for social analysis. It would be even more embarrassing—I hesitate to mention it—if we were to discover that undergraduate training in history, philosophy, constitutional law, or even English literature provides a better background for social analysis than sociology does.

On the other side, Etzioni in fact underestimates the degree to which the introductory texts now perform the tasks that he demands of them. More perhaps than any other kind of sociological literature they are informed by macroscopic concerns, and few if any of them contain only the "distasteful conceptual machinery" to which he refers. Similarly, sociologists have begun to take quite seriously the problem of teaching sociology to undergraduates who do not plan to become professionals in the field.[1]

In the second place, there appears to be in Etzioni's essay one example at least of the kind of role confusion that he himself deplores. I am a little alarmed by his hope that the distinction between biochemistry and medical practice could some day be erased and that sociologists, like medical practitioners in their sphere, should minister to the ills of society. He is erasing here the distinction between pure and applied science, between knowledge and art. The trouble with this erasure, in the present instance, is that it pushes sociologists into the political arena. What Etzioni wants, for example, is what we all want—a world community, an equitable distribution of wealth, civil rights for all, and the development of the underdeveloped nations. But the "we" in this sentence means American sociologists who, with a near unanimity I should suppose, do want these things. There are others, however, who do not, including those who supported the Republican presidential candidate in the last election. A world community is anathema to the Daughters of the American Revolution, "the redistribution of social wealth" is the red flag of socialism to the *Chicago Tribune*, and no member of the White Citizens Councils of the State of Mississippi favors "the growth of civil rights." Should sociologists then transform themselves into a political party in order to advocate and advance these worthy causes? I rather think not. I think it important to maintain the distinctions between normative judgments and categorical judgments, between social action and sociological inquiry, and between a social problem on the one hand and a sociological problem on the other.

A part of the problem can be seen too in Etzioni's reference to the essays of Bell, Coser, Glazer, Riesman, Wrong, and others, all of which are sophisticated and many of which are sound. About a proposition in physics or genetics or astronomy, however, we ask not whether it is sophisticated and sound, but whether or not it is true. We

[1] See, e.g., Charles H. Page (ed.), *Sociology and Contemporary Education* (New York: Random House, 1964).

can say that Wrong is right and Riesman is wrong if we happen to agree or disagree with their opinions, but we cannot say, as Glazer suggested in his brilliant *Commentary* review of *The American Soldier*, "as Wrong or Riesman has proven" or "established." We cannot say that they have contributed laws or principles on which we can cumulatively build.

I happen to favor the kind of sociology that Etzioni advocates. It carries a price, however, that I am not sure he is willing to pay. If we adopt his recommendations we shall have to surrender our fondest hope that sociology is a science and content ourselves instead with a humanistic discipline, an activity of intellectuals rather than of scientists. If this is what he means, I am happy to second the motion. It is not, however, a "role pair," but rather a role disjunction.

It may be observed, in short, that our problem has four cells instead of two. No one, I take it, wants a sociology that is normative and microscopic. Etzioni wants a sociology that is normative and macroscopic. And I should prefer one that is categorical and macroscopic. There is clearly a problem here that merits the best attention of all of us. To the statement of this problem Etzioni has made a significant contribution, and to its solution he has given an important impetus. The principal danger is that the new sociology that he recommends will itself suffer from elephantiasis.

Robert Bierstedt

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Reply

There is obviously a wide range of agreement between Robert Bierstedt and myself. Rather than reiterate the points of consensus, let me focus on the few points of difference, moving from smaller to somewhat weightier points.

Regarding Reston and Lippmann, Bierstedt is arguing from the exception to the category. Our discipline, I suggest, needs to train twenty or so social analysts a year. The fact that each generation turns out one or two outstanding social analysts without such training does not show it to be superfluous. The overwhelming majority of social commentators, editorial writers, etc., who are uninitiated to sociology have a poor record of understanding social issues, from race relations to the radical right. Second, one should not compare the performance of these social commentators to the sociologists of today, most of whom have no training in social analysis either. My point is that the average sociologist trained in social analysis will do better than the average social commentator who is not.

An alternative proposition to that of Bierstedt on the value of liberal arts education for social commentary would be that these disciplines install blinders which can be removed through training in social analysis, but rarely without it. To illustrate: Training in constitutional law tends to condition observers to stress formal over informal factors (see, e.g., various suggestions to solve the problems of armaments and east-west conflicts by revising the United Nations Charter); English literature tends to make its graduates psychologistic rather than structural, and to ignore the difference between national character (or "period-moods"), subculture, and deviant behavior; philosophy leads a surprisingly large number of my colleagues to claim that they can learn about human behavior by introspection.

More important, I do not advocate that the difference between basic and applied science be abolished (though I do hold that the greater our knowledge of both, the smaller will be the difference between the two). I believe that sociology, as a science, is well launched on the twin bases of general theory and methodology. I favor, however, much larger investments in ap-

plying this sociology to social analysis. I do not wish to demolish a partition but to build up a wing.

Whatever the ideological and political commitment of a sociologist (as long as he has one), he will agree that more effort ought to be invested in macrosociology. Even if he favors, in his role as a citizen or political activist, curtailment of federal "intervention" rather than its increase, as a sociologist he is interested in the study of the effect of government on society and vice versa, a subject we largely neglect. Even if he favors the Cold War over disarmament, he still is interested in the social factors affecting international relations, a subject we shall never advance much in small-group laboratories, etc.

Combining political action (whatever the color) in an extra-sociological role with that of professional sociology seems, to me, a fruitful role pair. For the sociologist, it makes for interest in social analysis.[1] For the politically engaged, it makes for a better founded engagement.

True, sociological training favors the left-liberal over the right-conservative, which might make some conservatives fight social analysis. But it is important for us not to legitimate the latter position by defining it as an objection to left-sociology, as Bierstedt seems to imply, but to recognize it for what it is—a fight against science, a well-known stance of the less enlightened conservatives.

I do not advocate a sociology that is an ideology—a set of normative statements about society. With others, I seek a sociology that would be both micro- and macroscopic and as scientific as possible. The articulation between sociology and ideology will be found in the selection of problems to be studied, *which is inevitably and legitimately normative,* and in the role pair of a social analyst who is both a scientifically equipped sociologist and a politically engaged citizen and activist. Bierstedt himself proves a fine example of the feasibility and fruitfulness of this combination, even if he sees it as a disjunction.

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[1] Not that every sociologist has to be either politically engaged or a social analyst; there is legitimate room for pure theorists, methodologists, etc. But I would like to see relatively more sociologists devoted to social analysis and the profession devoting more attention, resources, and legitimation to this line of specialization.

Dynamic Inferences from Static Data

The recent paper "Legal Evolution and Societal Complexity" by Richard D. Schwartz and James C. Miller[1] employs a method which has become increasingly commonplace in cross-cultural analysis, and raises what I think is an interesting question; namely: What conclusions, if any, can one draw regarding evolution or development through time by inspecting a Guttman scale constructed from cross-sectional static data? Schwartz and Miller evidently assume, as I did in an article cited by them which was published six years ago,[2] that the sequence of appearance of the items in the scale can validly be interpreted as denoting a developmental or evolutionary sequence. To be sure, Schwartz and Miller occasionally enter some demurrers in this regard, even to the point of briefly proposing and rejecting an alternative interpretation at one juncture. Furthermore, it is only fair to point out that in their footnotes, they allude to some of

[1] *American Journal of Sociology,* LXX (1964), 159-69.

[2] Stanley H. Udy, "'Bureaucratic' Elements in Organizations: Some Research Findings," *American Sociological Review,* XXII (1958), 415-18. A later article, "Administrative Rationality, Social Setting, and Organizational Development," *American Journal of Sociology,* LXVIII (1962), 299-308, sought to correct the earlier error and substitute a more appropriate interpretation.

the problems commented upon here. Nevertheless, their basic position is quite clear; they do not really seriously consider that the method they use is basically affected by these problems, and it is evident that their occasional apparent ambivalence on this score was not allowed to have any effect on their conclusions. For example, on page 168 they state: "We have seen that damages and mediation almost always *precede* police in the primitive world" (*italics* added).

I find myself in the rather awkward position of now objecting to this method, despite the fact that I used it myself some years back in the article cited above, and did so without exhibiting any disquietude or ambivalence whatsoever. But it would appear that this procedure is, at best, of highly dubious merit. The essential difficulty lies in the inference of dynamic conclusions from static data. It does not seem inconceivable that under certain rather highly restrictive conditions this could be done from a Guttman scale, but the problem is much more complex than has often been assumed. So far as I am aware the logic of such a process has never been systematically explored and developed *in abstracto;* the sources cited by Schwartz and Miller either simply employ the method uncritically in an empirical investigation or are concerned with problems of its immediate application other than those deriving from its basic logical properties. Nevertheless, despite the absence of such an exploration, one can at least hazard some suggestions as to the conditions under which the order of appearance of items in a Guttman scale would reflect a developmental or evolutionary sequence. Even the most casual efforts in this direction are sufficient to cast serious doubts on the validity of this method under most ordinary research circumstances. For example, one crucial condition would seem to be the absence of simultaneous or nearly simultaneous development of the characteristics in question. For if such characteristics do develop simultaneously—or so nearly simultaneously that a cross-sectional sample would be unlikely to yield any cases in the process of development—the researcher cannot exclude the possibilities that (1) the scale types represent qualitatively different types of units which in fact develop independently of one another; (2) more complex types develop first and subsequently drop some characteristics; or (3) some combination of the above occurs, together and/or singly, or together with some development congruent with the scale pattern.

Therefore, unless one can realistically assume this condition of "non-simultaneity," one is faced with a situation wherein any developmental sequence whatsoever is consistent with any scale pattern at all.[3] Although, as indicated earlier, Schwartz and Miller are not entirely unaware of this problem, they certainly do not seem to take it very seriously. At no point do they present any data or theoretical argument in support of the implied contention that their material meets any conditions essential to the argument they make from it. Perhaps it does, but in view of the rather highly restrictive nature of at least one necessary assumption, the burden of proof would seem appropriately to fall on them. "Alternative interpretations" involving simultaneous or nearly simultaneous development of traits with subsequent dropping of some of the traits are very possibilities which cannot be dismissed a priori as merely farfetched. For example, I had erroneously assumed[4] in the article previously cited that a scale expressing differ-

[3] Perhaps not quite. It is conceivable that a method might be devised whereby developmental sequences could, under certain conditions admitting of at least some simultaneous development, be inferred not by inspecting the order of items in the scale but by analyzing the pattern of errors. However, the exact procedure which would have to be followed, and the necessary assumptions, are by no means obvious in the absence of more extensive exploration of the formal properties of the situation. It remains clear, however, that in the absence of the condition of non-simultaneity, the method followed by Schwartz and Miller is not appropriate.

[4] See my " 'Bureaucratic' Elements . . . ," *op. cit.*

ent degrees of bureaucratic complexity indicated a developmental sequence. The scale had been developed from cross-sectional, static data. However, dynamic historical data from voluntary associations suggested subsequently that the actual developmental sequence might well be virtually the reverse of the scale pattern: a more or less effective way of infusing a voluntary association with some modicum of rational bureaucratic administration is to start with more bureaucratic complexity than is really technically necessary to its task with the prospect that certain of the most complex characteristics will subsequently drop out.[5]

I do not mean to insist that a parallel situation necessarily obtains with the Schwartz-Miller data, but merely to point out that there is no reason to believe that it does not. On the contrary, it seems quite reasonable to presume that, at least under conditions of centralized authority, several characteristics of a legal system would be quite likely to be promulgated at once. To be sure, Miller and Schwartz indicate that no cases involving sequences different from the one they presume have come to their attention. But since they present no dynamic historical data, it would appear that in a very real sense no cases involving the sequence they do presume have come to their attention either. We are left with the fact that, in the absence of at least an assumption of non-simultaneity, which assumption seems highly questionable in the present context, no inferences concerning developmental sequences can be made without recourse to dynamic historical data. Moreover, a thorough exploration of the matter might reveal that further assumptions are necessary as well.

Are we to conclude that construction of scales such as that of Schwartz and Miller is a useless exercise? I think not. It would seem quite appropriate to employ such scales in the analysis of requisite, as opposed to prerequisite, structures. Thus if one assumes that all of the societies studied are basically alike, and that no structural substitutions are possible, one can, for example, infer from the Schwartz-Miller data that societies possessing police but not mediation are unstable, as would be societies with counsel but not police and/or mediation, and so forth. From this point of view one can then develop hypotheses regarding differential problems involved in various specified developmental sequences. But data of an altogether different order are necessary to discover which sequences actually occur.

It has not been my purpose here particularly to castigate Schwartz and Miller, who have written an article which I found to be very interesting and stimulating, and which contains an error that I too was guilty of committing. Neither has it been my purpose to present a definitive solution to the problem, other than to suggest that if one is found, it may be extremely difficult to apply in practice. Rather, my intent has been to call general attention to a difficulty which I think is an important one and which came forcibly to my attention since I was caught in it myself.

STANLEY H. UDY, JR.

Yale University

[5] F. S. Chapin and J. Tsouderos, "Formalization Observed in Ten Voluntary Associations," *Social Forces*, XXXIII (1955), 306–9; and their "The Formalization Process in Voluntary Associations," *Social Forces*, XXXIV (1956), 342–44; A. Meister, "Démocratie et participation dans les associations volontaires," *Sociologie du Travail*, III (1961), 236–52.

Reply

I am grateful to Stanley Udy for his comments on the Schwartz-Miller paper. He is quite right in calling for a systematic exploration of the method. Nevertheless, I think that the method will stand up under close scrutiny. As a first step toward such an examination, let me spell out some of the assumptions which underlay the work.

Our scale analysis of cross-cultural data was aimed at discovering the sequence in which legal roles would be added if they were acquired one at a time. In principle, the scale might serve equally well to predict which item or items would be dropped if change were in the opposite direction, toward a less complex legal system. While we recognized this as a possibility (p. 163, n. 24), we assumed that the usual sequence of change would move in the direction of greater rather than lesser complexity. The findings of archeology and culture history lend plausibility to this assumption.[1]

In facing the problem of simultaneity, we took a conservative view. Admitting the possibility that two (it should have been two or more) items "might sometimes occur simultaneously," we noted that the finding of all possible scale types indicated that no two items invariably did so. From this fact, we inferred only that "when they occur separately, one regularly precedes the other." The process of reasoning may be schematized in the following way. For each of the societies with items A and B we asked which of these items did *that* society have, at an earlier point in time, when and if it did not have both. In the absence of historical data, we had to infer the answer from those known societies which had A *or* B. Admitting that the A and B society might earlier have had neither, we asserted that, if it had one or the other, the item it had was in all probability A. This seemed likely, since all of the cases which we found with A or B had A and not B. What reason was there to suppose that such societies, currently having characteristics A and B, had earlier been different from current societies with one of the two items? If there were societies which had B but not A, they escaped our attention. This might have occurred because of the limitations of our sample, but that is surely not a problem unique to this method. It might have occurred because societies with B but not A are in an unstable state, maintaining that combination for a limited time before dropping B or acquiring A. The short duration of B without A might explain our failure to note any such instances. It would also suggest, however, that A, if not a temporal predecessor of B, is at least a requisite needed for the maintenance of B, but this would hardly alter the basic evolutionary picture presented.

I agree with Udy that it would help confirm our developmental inference, and help test the "requisite" rival hypothesis, if we had historical data showing the diachronic development of these roles in given societies. The absence of such data for the types of societies studied here has long been lamented by anthropologists. The history we have amounts, in Scott Greer's phrase, to "a few scattered postage stamps in a big empty album." I would welcome a test of our hypothesis against such data, but I wonder whether, considering the paucity and bias of available historical materials, inferences drawn from them would be nearly as trustworthy as ours. If such information convincingly disconfirms our hypothesis, fine. Should not the burden of proof lie on those who believe that it might?

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[1] An outstanding authority for this statement is Ralph E. Turner, *The Great Cultural Traditions* (2 vols.; New York: McGraw-Hill Book Co., 1941). A similar picture emerges from the separated reports and summary chapter in Robert J. Braidwood and Malcolm M. Willey (eds.), *Courses toward Urban Life* (Chicago: Aldine Publishing Co., 1962). Cf. also V. Gordon Childe, *Man Makes Himself* (New York: New American Library, 1951), and *What Happened in History* (rev. ed.; New York: Penguin Books, 1954), as well as Julian H. Steward, *Theory of Culture Change* (Urbana: University of Illinois Press, 1955).

Hayes, "Item Order and Guttman Scales"

In his article, "Item Order and Guttman Scales" (*AJS*, July, 1964), Donald P. Hayes reports on the administration of Guttman scales in both serial and disarrayed item order. He found that those who answered the disarrayed items scored higher, and he concluded that this contextual effect raises questions about the ordinal properties of Guttman scales. In fairness to the Guttman scaling method, some comments are in order.

Nowhere in the article is mention made that Louis Guttman was well aware of this contextual effect. On page 214 of Volume IV, *Studies in Social Psychology in World War II*, Guttman states: "Slight changes in the question wording, in the order of presentation of the answer categories, in the position of the question in the questionnaire, and many other factors known to all pollsters may affect the findings of one's poll." In this connection, Guttman suggested his intensity component as one way of establishing an invariant zero point at which the population studied could be dichotomized, regardless of these contextual factors.

Further, the contextual effect described may be of little practical importance other than to inform us that we should use our scale questions in the same order on different populations if we want to avoid contextual effects. The crucial point is not the rise in scale response by the group taking the scale in the disarrayed form; the crucial point is whether or not the rank order of individuals relative to each other would be disturbed by such an effect. If the disarrayed items merely moved every respondent up on the scale a certain amount and did not disturb the individual rank order, then the scale is still distinguishing among the respondents in the same ordinal fashion. It is important, if Professor Hayes wants to show the serious effects of item order, that he present additional data demonstrating that the item-order changes do drastically affect the rank order of individuals relative to each other.

State University of Iowa IRA L. REISS

Reply

Were it the case that in changing the item order, one merely shifts the entire distribution of scale types, there would be no reason to be concerned about the particular form of contextual effect reported in the article. Table 4, and indirectly Table 1, provide no evidence that such is the case. Since the disarrayed form produces an impression of a more anxious population when compared with the serial order, a change to a disarrayed order produced a substantial (57 per cent of the subjects) rise in anxiety level. However, nearly half changed not at all or became less anxious. The pattern is essentially the same for the change to the disarrayed form, nearly half fitting in less anxious scale types while the other half did not change or became more anxious. Unfortunately, it appears that the rank ordering of subjects *is* substantially altered, the order effects not uniform.

With regard to the use of a "zero point" to dichotomize the distribution, its use may indeed provide some control for the effects of context upon response; yet there is a restriction on this technique. Guttman scaling principles have much wider application than to bipolar attitude scales. The two scales used in the article (manifest anxiety and mathematical sophistication) are typical of instances where there is no zero point. In these cases, the only zero point is a theoretical one, since it is difficult, though perhaps profitable, to imagine no mathematical sophistication whatever, except in very young children, or no anxiety, even negative anxiety.

DONALD P. HAYES
Cornell University

BOOK REVIEWS

A Protest Movement Becalmed. By LEO ZAKUTA. Toronto: University of Toronto Press, 1964. Pp. vii+204. $5.95.

Social scientists have a penchant for studying those activities which are *significant* in terms of their own values and action concerns and *interesting*, that is, special and peculiar, rather than analyzing the significant case which might further theory or test a hypothesis. Political scientists, sociologists, and psychologists have been more concerned with left reform movements and with extremist groups of different varieties than with conservative or moderate movements. Sociologists of work have paid considerable attention to such curious occupations as morticians, hoboes, barbers, gamblers, dance-hall girls, and jazz musicians. Latin America, which offers the best laboratory for comparative study, with twenty nations having a number of common elements and the longest history of ex-colonial nations seeking to develop, has received relatively little attention from North American students of comparative social and political development. The Philippines and Australia, which from an analytic point of view are among the most sociologically interesting nations, possessing seemingly stable combinations of institutions and values that challenge conventional sociological wisdom, have been almost totally ignored by American sociologists. Most of us apparently prefer to spend our years abroad in more exotic surroundings.

A Protest Movement Becalmed, is much to be welcomed in this context, for it represents a genre of almost non-existent literature, the study of declining or static social movements which appear to have lost their ability to press to power. Political sociologists prefer to study successful or growing groups; few have worked on those that have definitely failed. The Co-operative Commonwealth Federation (CCF) is unique among radical social movements in having had a strikingly rapid growth and an equally rapid decline in popular support. At one point in 1943, the Canadian Gallup Poll reported that its support among the electorate was as great as that of the two old major parties (29 per cent). In the provincial elections in Ontario in that year, it rose from no representatives in the provincial legislature to thirty-four, just four less than the Conservatives who became the government. The following year, the CCF formed the government of Saskatchewan with a majority of the popular vote. The Gallup Poll revealed constant loss of support in 1944 and 1945. By the national election in June, 1945, the CCF could secure but 16 per cent of the vote and 12 per cent of the members of the House of Commons. And these disappointing results were to be its high point of national support. In all succeeding elections, backing for the CCF, or, as it has been called since 1961, the New Democratic Party (NDP), has hovered between 10 and 14 per cent. After having been the official opposition in the industrial province of Ontario, it so declined that it has been little represented in the legislature since 1951. The Saskatchewan party lost office in 1964.

Professor Zakuta indicates that the success of the party in the early 1940's pressed its leaders to moderate their radicalism so as to appeal to those who favored reform but feared socialism. With this change in ideology went a decline in rank-and-file participation in party activities and a loss in élan among the radicals who had seen in the party a way to effect chiliastic dreams. Growth in membership and support "robbed the 'old timer's' vision of its [the party's] uniqueness and simultaneously transformed the intimate band of 'comrades' into an impersonal army." Accompanying the decline in, and demand for, voluntary ascetic devotion went an increase in party funds and organized trade-union support. These assets enabled the party to pay active workers and leaders for services which were provided by devoted enthusiasts This period of affluence did not last long. But when the party returned to the status of a weak third party with little hope of reaching power or major party status in the foreseeable future, it could no longer secure the devoted services of ideologically committed comrades and thus was seemingly worse off in terms of activity than during its period of initial elec-

toral weakness after its foundation in the early 1930's. Surprisingly, however, the moderate ideology adopted in the middle 1940's continued to influence the program of the party after it lost its hopes of achieving national office. In fact, defeat and decline did not press the party toward becoming an embittered ideological sect, as has occurred with many other comparable minority movements and even with declining major parties such as the American Republicans. Rather, the CCF continued to move toward the reformist or moderate right, going so far as to change its name to one which carries no connotation of socialism, the New Democratic Party. The NDP stresses various welfare-state reforms rather than government ownership. Unkind critics call it "the Liberal Party in a hurry."

I have dwelt on the historical record and related internal and ideological processes, as presented by Zakuta, because they point to the real puzzle of the changes in this movement, one to which the book unfortunately does not address itself except briefly at the end. The reader is left wondering how and why a minority party in a North American federal structure has been able to maintain itself after it has lost its élan, its radicalism, its sense of special mission, even its unique program. The NDP in 1963 secured 14 per cent of the national vote, more than the CCF obtained in any election since 1945. In 1960–61, the Canadian Labor Congress, the united trade-union movement primarily encompassing affiliates of the AFL–CIO, officially joined the NDP, a phenomenon not paralleled in the past. This support by the unions for a small left third party was not precipitated by any crackdown on the labor movement, or shift to the right in Canadian politics.

"Whatever has happened to the CCF?" is the first sentence of the last chapter. And Zakuta answers, nothing very much, it " 'went the way of all organizations.' In technical language, it became institutionalized." But this is only one part of the answer. There are much more interesting aspects of it which remain to be answered. The fact that so many Canadian voters, the heads of the trade-union movement, plus capable politicians, prefer to support an electorally hopeless third party, which has not been able to directly influence government policy through participation in it, challenges many of the generalizations which have been made about the failure of socialist and other third parties in the United States. These generalizations involve assumptions about the motivations of voters, about the pressures to conform placed on representatives of economic interest groups, and the concerns of ambitious political leaders. The CCF–NDP (like the Philippines with its 400,000 university students, high intellectual unemployment rate, low income, and reasonably stable two-party competitive politics) presents interesting issues, the study of which offers much to anyone interested in testing and elaborating generalizations about national social structures, ideology, bureaucracy, and political behavior.

SEYMOUR MARTIN LIPSET

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The Sociology of Economic Life. By NEIL J. SMELSER. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. viii+120. $3.95.

It is with some regret that I pronounce this an outstanding book. The regret arises from the fact that Smelser is my competitor on this subject, and it now appears that we may take alternate assignments for as long as I remain professionally active. After that, Smelser may find a new and more challenging competitor.

The first thing to be said about this book is that it is absolutely clear, and occasionally slightly exciting, on a subject that sociologists find fearsome or dull, or both. The author demonstrates once more his economic literacy and the utility and shortcomings of economic analysis. I fear that one of his intended audiences, students of economics, will not read the book because their professors, being conscientiously narrow-minded, will not assign it. This is too bad, because the exposition cannot be dismissed as economically naïve and thus must be taken seriously. The polemical tone that creeps in from time to time may make the occasional economist wandering in these vineyards defensive, but I cannot find it in my heart to criticize the author on this score. There is really no point in taking the economists at their own evaluation, for, when they are "best" by the estimate of colleagues, they are poorest as social scientists. Model-building based upon nonsensical assumptions has

gone to the point that professionals are known to take seriously the work of fools: witness mathematical "welfare economics," which has nothing to do with welfare or the operating economy, or the silly pretensions of Milton Friedman, to whom economists render a wholly undeserved deference. By my view, Smelser has been too kind with the fools, but he has at least attempted to display the underpinnings of rarefied economic analysis before showing the consequences of erroneous sociological positions.

The author, thankfully, starts conventionally with a review of the contributions of economists, sociologists, and anthropologists to the analysis of the social setting of the economy. I say "conventionally," but I find his summary of leading theoretical figures most unconventionally succinct and perceptive. He then moves on to a more analytic view of the economy and its social setting: first structurally (chap. iii) and then in terms of economic processes—production, distribution, and consumption (chap. iv). A final chapter deals with economic development, and on that I shall comment further.

Although I have been a leading and vehement exponent of the view that industrialism has neither common origins nor common destinations, I think that Smelser has gone much too far in abandoning the predictability of structural consequences. His summary of the sources of variability (pp. 105–6) is quite acceptable, and yet it does not lead to the conclusion that no similarities are to be expected. As is so often the case, out of error comes utility: Smelser pushes himself into a corner unnecessarily, and emerges with generalizations in terms of processes rather than of forms. These common processes are structural differentiation, integration, and social disturbances. This dynamic view is so much superior to the normal "ultimate destination" argument that one can only be happy that Smelser has sinned, and sinned bravely.

This book, though a marvel of clarity, is compact. I do not think it is a one-week assignment in an introductory course. Like other volumes in the series of which it is a part, the book insults neither the teacher nor the student. If there were in fact a market for scholarly books, one could hope that sociologists who feel abashed at their colleagues, the economists, would buy, read, and eventually assign this book.

WILBERT E. MOORE

Russell Sage Foundation

Personality and Social Systems. Edited by NEIL J. SMELSER and WILLIAM T. SMELSER. New York: John Wiley & Sons, 1963. Pp. xi+660. $8.95.

Smelser and Smelser have drawn a sample of sixty-one articles heavily biased toward excellence from what may be known henceforth as the "personality–social-system" field. No reader will fail to have his ideas drastically extended by this broad selection.

Especially impressive are the many articles that deal constructively, both empirically and conceptually, with the slippery issues surrounding distinctions between personality and social role. If given the attention it deserves, Levinson's brilliant analysis of the concept "role" could prove more beneficial to sociology than any other article published in the past five years. Articles by Wilensky and Edwards, by Lieberman, and by Inkeles are also especially good in this connection. Edward Bruner's piece on cultural transmission and cultural change, which presents a refreshingly simple and appealing hypothesis, should be read right after those by Inkeles and by Wilensky and Edwards. Parsons, in a rather difficult essay, points out the relevance of the psychoanalytic notion of object relations to both personality and social role.

Characteristically, the contributions are solid—one knows something for having read them. Thus, Toby provides some important facts about the education of lower-class children, and the McCords do the same concerning the effects of parental role model on children's criminality. Similarly, the main issues involved in the problems of national character and the ecological approach to social psychiatry are laid out, probably definitively, by Farber and by Clausen and Kohn, respectively. These are all classic papers.

Many others are equally worthy of mention, such as the Yarrow, Clausen, and Robbins effort on the social meaning of mental illness, and the paper by Jackson on the adjustment of the family to alcoholism. And, of course, Goffman's famous analysis of em-

barrassment. Every bit as brilliant as Levinson's article, it just happens not to have been addressed to as central a problem.

Perhaps one tentative warning is in order. The unusually large critical ratios reported in the article by Neiman would require extremely small standard deviations and/or extremely high correlations between scores for the type of instrument employed and the indicated sample sizes. One wonders whether the correct formula was applied.

The editors have performed a service by giving prominence to many of these papers. Probably, they will have created also a permanent awareness that the culture and personality and family and personality fields are but special cases of what has been until now a merely implicit more inclusive category. Possibly an increased appreciation of the relevance of personality variables to practically all areas of sociology might result.

Robert A. Gordon

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Swedish Working Wives: A Study of Determinants and Consequences. By Murray Gendell. Totowa, N.J.: Bedminster Press, 1963. Pp. 269. $4.00.

In 1955 the Swedish Institute of Public Opinion conducted a national survey of adult Swedish women aged 18–55. This survey was part of a larger study sponsored by an unspecified number of voluntary organizations which were interested in finding out how they might gain new members and more effectively interest current members in their activities. Four years later, in 1959, Murray Gendell undertook a secondary analysis of these data, which resulted in a Ph.D. dissertation in sociology from Columbia University in 1962 and the publication in 1963 of the slim volume under review.

Gendell's scheme for analyzing the *determinants* of employment among married Swedish women consists of three major components: (1) their jobs—the quantity and quality; (2) the wives themselves—their physical and mental state; and (3) the wives' situations—their domestic obligations and household facilities, human and otherwise (pp. 66–67, 217–18). The *consequences* of employment which he attempts to illuminate cover such variables as marital satisfaction, child-rearing practices, political and religious participation and values. His analysis is essentially a comparison of three subgroups of Swedish married women in the SIFO sample: about 109 full-time working wives, another 109 part-time working wives, and 560 non-working wives.

Although the problem is endemic to secondary analyses, I have rarely seen a published study that demonstrates more emphatically than this one the hazards and limitations of utilizing data for analysis purposes very different from those guiding the initial design. As the author himself repeatedly warns us, his analysis is often little more than "tangential and inconclusive," "illustrative rather than demonstrative." On child-rearing, the "data are particularly sparse and unsystematic" (p. 152). On motherhood, "only a small part of the survey aimed at obtaining data relevant to this matter" (p. 144). On motivation for employment, the author notes: "Although conceptual analysis reveals a potential complexity frequently ignored in empirical investigations, the data available to us restrict consideration to the question of financial need" (p. 219). The analysis of the "wives' situation," and of the bearing of household facilities and domestic obligations upon their employment, rests upon an item-by-item analysis of contrasts between full-time, part-time, and non-working wives in proportions owning a sewing machine, a vacuum cleaner, and a refrigerator.

The limitations of the data and the sample are in no sense compensated for by the author's treatment of these data. Not one of the eighty-nine tables in this study informs us of the nature of the jobs these women were employed in. One knows such data were available only because at one point in the analysis he classifies full-time working wives by the relative prestige of their husbands' compared to their own occupations (pp. 140–41). For a woman to be a laundress rather than a lawyer, a file clerk rather than a textile designer, must surely be as relevant to such variables as marital satisfaction, motivation for work, persistence of employment, and household management as the standard controls of age, education, and place of residence with which the author deals. The reader can only *infer*, from the educational attainment and place of residence of the respondents,

that very few of the working wives were in technical or professional fields. One finds, for example, that about 41 per cent of the respondents were rural residents, another 44 per cent lived in small towns and villages, only 14 per cent in cities of 100,000 or more. Even among the "big city" residents, only 29 per cent had gone beyond elementary-school education (which only 7 per cent of the rural residents had done). It would have been of far greater interest to American readers if some attention had been paid to the extent and nature of occupational participation of big-city, well-educated Swedish wives compared to their counterparts in the United States.

This reader found certain of the Swedish data referred to in footnotes and occasionally included in text tables to be of greater interest than the data from the SIFO sample. In fact, the volume was most helpful as a bibliographic guide to such studies. These other data show that very few Swedish women "never worked," but also that as of 1955, only a small minority of Swedish wives worked after marriage. Since the average age at first marriage was 25 in 1955 (a decline of two years since the 1870's), and since so very small a proportion of Swedish women secure education beyond elementary school, this means an average of about ten years between the end of formal schooling and marriage. It would be most interesting to learn what Swedish girls and young women do during those ten years. Why don't more of them get advanced education? Why don't more acquire an interest and motivation for work that persist after marriage? What is the nature of these young women's roles in their families and communities before marriage? This is such a striking contrast to the pattern in the United States that it is clearly a plea for a fascinating comparative research study during late adolescence and early adult years, but it is not one that Gendell makes.

This is at best a pedestrian dissertation exercise, and should have remained that. It is a pity Gendell did not attempt to distil his dissertation into a few articles, or more ambitiously, to write a volume on Swedish working wives based on all published studies on the topic, subordinating his own weak data to a minor illustrative role.

Alice S. Rossi

University of Chicago

Towards a New Sociology—Essays and Studies. By Martin Allwood. Mount Pleasant, Iowa: The New Prairie, 1964. Pp. 150. $2.90 (paper).

If one counts oneself among those who are not exactly enthusiastic about the state of sociology in this country, one has frequent occasion these days to recall the old adage that fearing one's friends leaves little time for worrying about one's opponents. The phrase "new sociology" is enough by itself to make one think of this adage with acute apprehension. The present collection of papers by Martin Allwood, associate professor of sociology at Iowa Wesleyan College, proves that the apprehension is more than justified.

The volume contains some "theoretical" essays, foremost among them a programmatic piece entitled "Sociology and Freedom" and a chapter entitled "Sociological Action Models," and a number of papers on empirical subjects ranging from the Hungarian revolution of 1956 to the reception of American comics in Sweden. Allwood believes that sociology must concern itself with humanly important problems, among which he singles out "the area of Christian freedom, individual integrity and constructive deviation from all norms" (p. 14). The method advocated for this purpose is supposed to be *Verstehen,* apparently understood by the author as some sort of individual and value-oriented antidote to "the horrors of Durkheimian society-worship" (p. 23). Weber is credited with the view that "the state of the individual human being is not to be inferred or interpreted by self-appointed 'analyzers,' but can only be safely judged by his own uninterpreted, authentic statement" (p. 26). Durkheim, on the other hand, is given a bad mark because he "does not seem to have considered the spiritual dimension of solidarity worthy of serious thought" (p. 29). So much for Allwood's "theory." The papers on empirical subjects provide some conceivably interesting reports on sociological studies in Sweden and Germany, although there is hardly anything here that is "new." Among other matters may be found an attack on American family sociology for supposedly emphasizing sex as against love and a suggestion that the United Nations sponsor a new artificial world language.

It is difficult to imagine what purpose the publication of this mélange was intended to

serve. It will certainly not assist anyone looking for sociological insight, "new" *or* "old."

Peter L. Berger

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The Booster and the Snitch: Department Store Shoplifting. By Mary Owen Cameron. New York: Free Press of Glencoe, 1964. Pp. xii+202. $5.95.

Shoplifting is an extraordinarily interesting crime for the sociologist. This study of it calls into question some firmly held sociological prejudices about crime and deviance and, indeed, about American society generally, and goes on to suggest some needed additions and revisions to theories of deviance. Cameron's carefully analyzed body of facts might well serve as a touchstone for future theorizing; no theory should be accepted which cannot encompass and make sense of what she has described.

Briefly put, her major findings are that shoplifting is quite frequent and that it is committed, for the most part, neither by professional criminals nor by compulsive kleptomaniacs; rather, most shoplifters are "normal respectable citizens," who covet things that do not quite fit into their typically lower-middle-class budgets. They steal habitually, not impulsively; come to the store prepared to steal rather than buy; have no criminal associations and probably do not think of themselves as criminals; are dealt with and disposed of primarily by private-store police rather than official police agencies; and seldom repeat their crime once they have been apprehended. She shows that relatively few of those apprehended are prosecuted, but that Negroes are far more likely to be tried than others. She speculates, in line with her data but not proved by it, that the typical shoplifter learned the art of shoplifting as a juvenile, thinks of herself (the majority are women) as not really engaged in doing anything wrong, is unprepared for the consequences of being caught, and is unwilling to accept the identity of thief and for this reason gives up the habit easily.

The data are not really very good, as the author is careful to point out, but are the best that anyone is likely to get. It is impossible to interview the vast majority of apprehended shoplifters, because stores refuse to reveal their identities. But Cameron was able to get access to the carefully kept files of store detectives, as well as to court records of the minority who are tried. This enables her to show that the one store whose records she studied apprehended two-thirds as many women as were officially charged with larceny in the entire city of Chicago (in which the store was located) during a period of several years. She can thus argue conclusively that official records are inadequate because they fail to include the large number of crimes policed by private agencies and that the inadequacy distorts our conceptions of the extent of crime, relative frequency of different crimes, and the character of the persons committing crimes. The differences in residential distribution of those apprehended in the store and those brought to trial show that Sutherland was more right than he knew in arguing that our official crime statistics do not adequately reflect what is going on, and that we cannot ignore Cameron's suggestion that we look into the operations of private police more thoroughly than has been done.

Cameron uses her material to make the point that systematic crime can be carried on in complete isolation from other criminals and without defining oneself as a criminal, and yet not be of an obsessively neurotic character. She goes on to suggest that sociologists should not rely exclusively on subcultural or professional orientation as an explanation for crime, but should extend their typologies and theories to cover the lonely crimes of respectable people, following the leads contained in her study and in Cressey's study of embezzlers.

But I am most struck by the implications of the study for the casual and uncritical sociological reliance on middle-class morality as an explanatory variable. We are frequently assured that middle-class Americans espouse and live by such values as honesty, thrift, and hard work. Yet Cameron's data make clear that their values do not prevent them from stealing as much as a quarter of a billion dollars a year from retail stores, as customers, and perhaps three times that as employees. She suggests, and I think quite rightly, that values are easily reinterpreted to fit specific situations, that countervailing rationalizations are easily brought into play ("It's all right to

steal from a big company"), and that law-abiding behavior is more easily traced to fear of the consequences of being caught than to any deeply held values.

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Stigma: Notes on the Management of Spoiled Identity. By ERVING GOFFMAN. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. 147. $4.50.

Stigma represents a set of virtuoso variations on a grand theme announced first in the author's *Presentation of Self in Everyday Life*. In order for interaction to proceed smoothly, the several participants must have a socially accredited identity acceptable to the others. The mutual acceptance of identities forms the basis of the "working consensus" on which the interaction is based. Much of Goffman's subsequent analysis in *Presentation of Self* concerns itself with the tactics of face preserving (both one's own, and, in the case of tact, the other fellow's). In "Embarrassment and Social Organization" (*AJS*, November, 1956) Goffman deals with the problem of temporary discreditations of identity and the social conditions which give rise to them. Now, in *Stigma,* he deals with the problem of the permanently discredited (or discreditable); those with some attribute that leads them to be "reduced in our minds from a whole and usual person to a tainted and discounted one."

The notion of identity is crucial to Goffman's analysis. He first distinguishes between virtual social identity—the stereotyped imputations of attributes and category we make of another met in the routines of social intercourse—and actual social identity—the category and attributes he actually may possess. The stigmatized are those who have a deeply discrediting discrepancy between the two. Their central problem is acceptance by the world of normals.

Some of the stigmatized are known to us as such, either by virtue of prior information about them or, as in the case of the disfigured, by contact. Many are not immediately known as such, but a stigmatizing attribute is part of their life history, part of what Goffman calls their personal identity, as in the case of the homosexual or the former mental patient. The bulk of the book is given over to the problems of the discreditable in managing information about their failing and their techniques for doing so including passing, covering, "minstrelizing."

Goffman goes on to present a third subcategory of identity, "ego identity" (self-concept would do as nicely), and the influences upon it. Particularly important are the roles of the out-group of normals and the in-group of fellow sufferers in prescribing to the stigmatized how he should see himself.

The book emphasizes that the dividing line between normalcy and stigma is not as deep as the set of colostomies, cripples, prostitutes, lushes, homosexuals, Jews, and junkies Goffman parades before us would lead us to believe. The deeply stigmatizing attribute may scar all, or at least most of the faces required of the individual in his repertory of social relationships. Yet few of us are totally without blemish; few have actual social identities which are completely up to snuff. For those traumatized by the realization, in *Presentation of Self,* of how phony we all really are, this is tame stuff. Revealing the taken-for-granted character of idealized social norms and the importance and ubiquity of deviations from them is the hallmark of Goffman's perspective.

In many ways, this is an irritating book. It is clearly padded, about twice as long as it need be. The style is tour de force-ish. There is much that is too cute and only near relevant, in sum, too much schmaltz and not enough liver. In spite of these things, the problems tackled are important and what Goffman says about them is both insightful and right. Many contemporary sociologists look at interaction; few really see it in full depth and subtlety. And, taking *Stigma* as another case in point, none see so well as Goffman.

EUGENE WEINSTEIN
Vanderbilt University

Adolescent Personality and Behavior. By STARKE R. HATHAWAY and ELIO D. MONACHESI. Minneapolis: University of Minnesota Press, 1963. Pp. xiii+188. $5.75.

This book consists of 104 large pages of text and an additional 75 or so pages containing 133 tables of data. It reports an ef-

fort to chart Minnesota Multiphasic Personality Inventory (MMPI) patterns for adolescents and to use the MMPI to predict deviancy: delinquency and school dropouts. In major respects it is less a monograph than a collection of articles, since the core chapters are reasonably discrete entities concerned with separate subject matters. Little if any theoretical or other intrepretive rationale is attempted.

First data (consisting of the MMPI and school records) were drawn in 1948 from 4,000 ninth-grade students in Minneapolis. In 1954, similar data for around 11,000 additional ninth-graders were added from over half of Minnesota's 87 counties. Police and court files of each student's county of residence were searched between two and four years after the testing. (Apparently, mobile youngsters were not relocated, and no adjustment for mobility was made in the tabulation.) When they became twelfth-graders, an unclear proportion of the students were retested with the MMPI, and additional personal and scholastic data were gathered. One product of this research design is the analysis of changes in MMPI profiles between the ninth and twelfth grades.

The authors use the MMPI as an established tool. Indeed, the write-up vacillates between prediction of the outcome variables and a casebook approach to the instrument itself (setting norms, defining administration and scoring procedures, identifying groups with frequent invalid profiles, comparing adult psychotics with adolescents, etc.). One feels that the tabular presentations would have been simpler and more useful had the latter purpose been neglected. The tabular detail is distracting; often, each cell reports the observed frequency, the expected frequency, and the contribution of this difference to the total χ^2 value.

The first four chapters sketch the purposes of the report; describe the samples and the non-MMPI data, including how delinquency ratings were established; describe MMPI code procedures; and give normative MMPI data. Chapter v relates "the variables" (town size, sex, SES, marital status of parents, etc.) to "personality" (MMPI high-point code score). Chapter vi reverses the analysis: each high-point group is considered separately and the question of what background characteristics occur with uncommon frequency in each group is considered. Chapter vii deals with change and development during the adolescent years, but hardly begins to explore the fascinating possibilities in this area.

Chapters viii and ix report the use of background variables and MMPI high-point scale scores in predicting delinquency and school dropouts. As in prior chapters, no complex or multivariate analysis is attempted, and the results are discouraging to advocates of the MMPI; in fact, teachers using a simple five-point rating scale were substantially more adept than the MMPI at identifying predelinquents and no MMPI class came close to accurate prediction. Unfortunately, nothing direct is known of what cues teachers were reacting to, though they appear to have been very much influenced by high-school grade averages. One interesting comparison of individual and group data is reflected in the conclusion that the average severity of delinquency among a subgrouping dropped as the delinquency rate of the group dropped.

Two plaintive sentences (p. 4) set the tone for the manuscript and doubtlessly describe one of the anomalies of our computerized age: "The intricacies of interaction among the variables we report in our tables defy our ability to report. If we had collected less information, we probably could have given a better synthesized and more theoretically coherent report."

Ernest Q. Campbell
Vanderbilt University

Big School, Small School: High School Size and Student Behavior. By Roger G. Barker and Paul V. Gump. Stanford, Calif.: Stanford University Press, 1964. Pp. 250. $6.75.

This book represents a pioneering and intriguing effort to relate the ecological environments of high schools to student behavior. The usual study of the school setting assumes uniformity in the environment schools present to the students. *Big School, Small School* is notable in that it seeks out and explores environmental differences among schools. The concern for the ecological environment is restricted to its parts or to what are called "behavior settings" (e.g., the school lunchroom, the student council meeting, baseball practice). There are several methodological problems associated with behavior settings. One is

the question of what is and what is not a behavior setting. Another is the problem of means of surveying the behavior settings within a high school. The reader is referred to other publications by the authors for a full discussion of these issues. There is also the serious matter of comparability of behavior settings. As the authors point out: "Institutional units that are equivalent in a fundamental sense within schools, between schools . . . are essential if we are ever able to have a comparative science of schools. . . . Classes are not adequate for this purpose" (p. 45). The matter of comparability of units is reported to be solved by the use of an instrument referred to as "K-21." What is the nature of the instrument? Again, unfortunately, the reader is referred to other publications for the answer.

Having instructed the audience that behavior settings can be identified, surveyed, and equated, the authors turn to the matter of the kinds and distribution of behavior settings in small (83–151 students) and large (2,287 students) high schools. The theme of the findings in this respect is that neither number nor variety of behavior settings grows in proportion to the number of students (i.e., the largest school contained twenty times as many students as the smaller ones, but only five times as many behavior settings). Noting this relationship between school size and behavior settings permits the observation that large high schools have overpopulated and small ones have underpopulated non-classroom behavior settings. The major focus of the book is on the impact of over- and underpopulated non-class behavior settings on the behavior of high-school students. The exploration of this impact was guided by a series of hypotheses dealing with the nature and extent of student participation in non-class behavior settings, the satisfactions derived from such participation, and the forces toward such participation. The findings indicated that students in small schools participate in a wider variety of behavior settings and a larger proportion of them hold positions of importance and responsibility than do students in large schools. The satisfactions received from participation are of different kinds for small- and large-school students owing to the greater centrality of participation in the small school, and the small-school student reported more culturally sought satisfactions. Small-school pupils reported more self-generated attractions as well as foreign (i.e., not self-generated) pressures toward participation than large-school students. What seem to be the most meaningful hypotheses developed to guide the study were not explored. (Underpopulated schools developed "less sensitivity to and less evaluation of differences between people" and "greater functional self-identity" on the part of the students.)

The size of a school is very much an issue in public education. Its solution ordinarily rests on folklore, finances, and geography. The authors conclude, on the basis of their values and research, "that a school should be sufficiently small that all of its students are needed for its enterprises." They do not place this conclusion in juxtaposition to the research on academic achievement and school size, nor do they raise the question of the aftermath of the participation experience of graduates of small high schools. Does it hinder or facilitate post–high-school adjustment to our highly urbanized and overpopulated behavior settings, or does it make any difference?

Richard O. Carlson

University of Oregon

The Violent Gang. By Lewis Yablonsky. New York: Macmillan Co., 1962. Pp. xiii+264. $4.95.

The thesis of this book can be summarized as follows: Gratuitous violence among certain slum gangs is increasing, as there appear growing numbers of "sociopathic" youths from burgeoning slums so disorganized that normal socialization processes cannot take place. The violent gang emerges spontaneously as an agency for satisfying the sociopath's peculiar need dispositions and lack of competence to engage in more demanding kinds of social behavior. The "disturbed" boys who comprise the core members of violent gangs presumably have no superegos and consequently no guilt feelings; they are irresponsible, untruthful, insincere, and insensitive to social demands. As gang members they are given to paranoid, grandiose exaggerations about their gangs' sizes and exploits. The violent gang is a "near-group" whose nucleus of highly sociopathic leaders is surrounded by shifting and ill-defined satellite members who move in and out of the leaders' sphere of influence almost at will.

Gravitational pull toward the inner core is more or less directly proportional to the degrees of sociopathy possessed by the satellites; the cohesive forces of empathetic sentiments which bind ordinary "real" delinquent and social gangs are here lacking, producing what the author calls "compensatory paranoid pseudocommunities."

For four years during the 1950's Yablonsky was director of a privately sponsored crime prevention program in Manhattan's Upper West Side, where he made contact with the Balkans, a gang of about thirty regular members—mainly Negro and Puerto Rican—led by five "severly disturbed" sociopaths. The substantive contents of the book consist mainly of an anecdotal account of the gang during its eight months' existence, with emphasis upon those features illustrative of the author's thesis.

I feel that Yablonsky is rather more insistent upon the correctness of his theory than his evidence warrants. Granting that qualitative data are notoriously difficult to deal with critically, at several crucial points Yablonsky's substantive material appears to contradict his contentions. As a "near-group," the violent gang is presumably a "mob-like collectivity that forms around violence in a spontaneous fashion"; it has an indefinite number of members, lacks specific roles, and has no consensus of expected norms. But the Balkans —a name the boys picked for its military-diplomatic connotations—existed under one leadership clique for many months; its president (*sic*), Duke, appears to have been highly responsive to the norms impinging upon him as a leader, and even to have formed an attachment for Yablonsky; the interview protocols contain several references to various roles and member categories; the boys spent most of their time not in fighting but in hanging about on corners and later in a basement clubroom they found for themselves; some of the boys spoke fondly of parents and other adult figures. These seeming facts jibe neither with Yablonsky's near-group concept nor with his description of the sociopath as one who "does not have the ability to identify or empathize with any others."

Another difficulty concerns the latter point: if the author is correct in regarding sociopathy as a bona fide clinical category (he seems to use it synonymously with psychopathy on p. 197), its accurate diagnosis would require the skills of a psychiatrist or psychologist, but apparently no such professional diagnoses of the boys were made. On what basis, then, did Yablonsky, a sociologist, determine that the Balkan leaders were sociopaths? He does not say.

But if one finds the author's main theoretical formulations unsatisfactory, his book contains (p. 205) an alternative explanation of the violent gang which is not dependent on either his near-group idea or on psychological reductionism: in a society that motivates toward the achievement of success and notoriety, the disadvantaged slum boy with limited social ability and training can achieve a simulacrum of these goals through the use of an elemental violence which serves as a ready means for upward social mobility within the gang and, to some extent, in the larger society.

R. W. England, Jr.

University of Rhode Island

Changing Parental Attitudes through Group Discussion. By Carl F. Hereford: Austin: University of Texas Press, 1963. Pp. 198. $5.00.

In this book, Hereford describes an experiment designed to test the effectiveness of a community adult-discussion program. The program consisted of lay-directed informal discussions of parent-child relations which were intended to alleviate anxieties of parents about the responsibilities of child care as well as to change their attitudes toward child care in a direction more consistent with current psychological theory. These were not the public aims of the program (it was vaguely defined as being useful to parents) but rather the hypotheses of the research—what was expected to happen—and the standards for judging the effectiveness of the program.

After an introduction to the purposes and history of the community health program in Texas and a somewhat peripheral discussion of the training of lay discussion leaders, the major portion of the book is devoted to a step by step chronological description of the research design. Parents participating in child-rearing discussion groups sponsored jointly by the County Mental Health Association and the local PTA's were asked about their conceptions of child-rearing before the first dis-

cussion group and after the last one, a period of about three months. Approximately nine hundred parents from various school districts were tested. In addition, the children of participating parents were rated on a series of honorific variables and participated in a sociometric test with their peers, at both times. Three control groups from the same school districts were used: (1) parents who attended a lecture series ($N = 102$); (2) parents who signed up for the discussion program but did not attend ($N = 160$); and (3) a simple random sample of parents ($N = 271$). The experimental design is nothing original, but some of the devices used to control extraneous variables in the real-life situation are ingenious and might profitably be examined by other researchers engaging in similar experiments.

The results are surprisingly unambiguous. Findings from an analysis of covariance indicate that parents participating in the discussion program did change and in the expected directions. For example, fewer parents said they used corporal punishment, more parents attributed children's misbehavior to their own attitudes, and more parents claimed they changed their behavior to solve a problem in child-rearing after they had participated in the discussion groups. The control groups did not change significantly. The treatment of the data is careful and thorough, leaving little doubt in the reader's mind that the discussions and not characteristics of the subjects were responsible for the reported changes in attitudes. In addition, some of the results indicate that the observed change in parental attitudes may have some effect on the behavior of their children. The sociometric position of the children whose parents were in the experimental group was somewhat altered in the period between the initial testing and the retesting. However, teachers' ratings of pupils whose parents were in the experimental as well as the control groups show more or less random fluctuations.

The discussion program was set up as applied social science. The book describing the test of its effectiveness reads like a handbook for future communities wishing to set up such a program. There is no attempt on the author's part to set the experiment in the context of social theory. For example, at no time does the author suggest what theories of group functioning gave rise to lay-led informal discussion groups as the probably most effective vehicle for the desired changes in parental attitudes. Neither does he suggest what relevance his findings have for social science. The possible usefulness of this study for building theories about attitude change are left totally to the reader's imagination.

This book describes a well-designed and carefully executed experiment. Any social scientist attempting to design a similar experiment would find it useful as an example of clever research methodology. The findings from the study provide additional evidence of the power of peer groups upon individual's attitudes. They also suggest the effects of parental attitudes upon children's behavior. However, any conclusions about the value of this study for sociology must be extracted from the book by the reader. Anyone wishing a general knowledge of the effects of group discussions upon attitudes will be disappointed by this book.

REBECCA VREELAND

Harvard University

On the Shop Floor: Two Studies of Workshop Organization and Output. By T. LUPTON. New York: Macmillan Co., 1963. Pp. vii+201. $7.50.

Worker resistance to managerial controls is a behavioral phenomenon presumably fully explored by American scholars such as Collins, Dalton, Roy, Coch, French, and others. Lupton's study in two British workshops takes another look at the conclusions drawn by American authors in explaining work-group behavior. Conceived and executed in the tradition of social anthropology in England, this research provides a detailed analysis of the influence of technological, administrative, and social factors in explaining levels of individual and group output in an electrical engineering plant and in a garment assembly workshop. The author worked and observed behavior as an operator for six months in each of the organizations. He openly acknowledges those personal biases that are bound to creep into research when one uses the "open participant observation" technique for gathering data. Most of his raw material comes from notes accumulated while he was employed in the two concerns.

In the study of each of the work groups he begins by analyzing the technological and

administrative controls imposed by management on work groups. The kinds of bureaucratic controls exercised by management have, according to most of our studies, resulted in various forms of informal output restriction where it is patently clear that the goals of workers are not congruent with the expectations of management. A modified Homans descriptive model is utilized in analyzing the social structure of the two workshops. His general conclusion is that rational economic motives play a more important part in explaining behavior than has been commonly thought by most sociologists.

In the garment factory, staffed mostly by women, he discovered that there was virtually no group norm controlling output. Workers tacitly accepted management's controls. In the electrical engineering plant strong norms had developed condoning the appropriateness of "fudging" figures submitted to management regarding the group's output. Interestingly enough lower management itself condoned the practice. Lupton implies that supervision and other management representatives recognized that attempts to control such practices would, in effect, only destroy other positive outputs of the informal social system. The climate of relationships was markedly similar to the "indulgency pattern" described by Gouldner in his *Patterns of Industrial Bureaucracy*.

The social structure of the two work groups was different. In the garment shop social groups were not structured by the work process, even though the process itself required a high degree of interdependence and interaction. At the electrical plant social groups were formed around the productive arrangements of the shop. From the findings of earlier studies, the author points out, one would expect that the garment workers, who were subject to elaborate managerial controls, would organize a strong informal system of resistance. Both the spatial arrangements and the nature of the process as well as the wage payment system would make this probable. Also job security was tenuous at best because of seasonal employment factors. Lupton makes a reasonably good case to prove that lack of social cohesion in resisting management cannot be explained by any unusual social characteristics of women as such. His own interpretation of the difference is somewhat confusing. He establishes marked differences in group structure and function but then tends to dismiss them as explanations.

The author does effectively demolish a long-standing assumption by management and by some researchers that workers will "cooperate" more fully with management in producing to the maximum when there exists a mutual "sense of belonging" between workers and management. In the electrical plant lower management acceded to a "soft" indulgency pattern with workers. The result: workers understandably exploited the relationship, making sure, of course, that they disciplined those members who tried to grab too large a piece of the economic pie. Lupton believes that one of the controlling variables in the situation was the company's favorable position in the external competitive market.

Unfortunately four of the crucial pages of conclusions, as well as half of the index, were missing from the reviewer's copy, owing to faulty printing. Such an error might be forgiven were it not for the fact that this is the third book printed in England and reviewed by this reviewer that has had serious printing mistakes. But all irritation aside, this study does serve as a good reminder to sociologists that individual "economic man" explanations of behavior, apart from the context of the immediate work group, should not be dismissed lightly.

ROBERT H. GUEST

Tuck School, Dartmouth College

The Rank-and-File Leader. By SIDNEY M. PECK. New Haven, Conn.: College and University Press, 1963. Pp. 398. $6.00.

Sidney M. Peck attempts to answer a persistent and troublesome question in the study of the American labor movement: What is the level of consciousness that characterizes the ideological outlook of the rank-and-file worker? Is it better described as job-consciousness, class-consciousness, or socialist-consciousness? For this purpose he elects to study the ideology reflected in comments on a number of selected social issues expressed by individuals within groups of shop stewards interviewed in Milwaukee. On the basis of comments that reflect the presence of a solidarity which encompasses the interests of the wider working class as a whole, the author concludes that among rank-and-file leaders there is present

a current of marked *class-consciousness*. This class-consciousness is distinguished from the more narrowly interest-group oriented *job-consciousness* often attributed to American workers. As described, it is clearly not interpretable as in any way a reflection of the presence of *socialist-consciousness*.

On some issues, particularly with respect to a report of what appears to be a structurally determined containment of anti-Negro sentiment accompanied by anti-Semitism, there clearly are new and provocative data in this book. However, the novelty of the work lies more in the area of its methodology. As already noted, the author depends upon a group technique rather than individual interviews. Even more interesting, analysis of data thus collected focuses upon the content rather than process of group interaction. The group-interview technique depending upon a projective stimulus is a modification of one originally devised by Theodor Adorno and his collaborators for the study of public opinion in Germany.

This reviewer strongly sympathizes with Peck and with the difficult task he set for himself when he decided to study the level of consciousness of unionized workers in a large American city. Yet there are some troublesome methodological problems in the design and data analysis of this work which cannot easily be overlooked. To this reviewer it seems unfortunate that while the author clearly is aware of these problems he does not indicate how bias may have been introduced into data on the basis of which he concludes the existence of class-consciousness. The author freely generalizes to the universe of stewards, and sometimes of workers, on the basis of a clearly highly selected sample of persons and organizations. That is, the group interview was administered essentially to steward volunteers within seventeen organizations selected on the basis of an exhibited willingness to participate in the study. In view of the fact that his sample of stewards and organizations was selected rather than taken at random, the author might have taken greater care to qualify his generalizations. Second, the report consists almost entirely of a presentation and interpretation of what the author refers to as "typical" individual responses across groups. The reader cannot feel certain about how representative these comments were for the groups as a whole. In other words, it is not clearly established that the findings of the study are generalizable. The reviewer must report that while Peck emphasizes that a group technique is his basic method of investigation, his book is a report of a psychological analysis of data which might have provided an interesting sociological study.

EDNA E. RAPHAEL

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Individualism and Big Business. By LEONARD R. SAYLES. New York: McGraw-Hill Book Co., 1963. Pp. xiv+200. $6.95.

Individualism and Big Business is put forth as an answer to critics, such as Argyris, Boulding, Mills, and William H. Whyte, who charge that big business or, more accurately, large-scale organization in all spheres stifles individualism. The book is made up of articles by twelve participants in a conference of scholars and businessmen that dealt with this problem. The articles were selected by Leonard R. Sayles, and his introduction, comments, and concluding chapters serve to integrate them and amply justify his nominal authorship.

The articles range from such predictable topics as the use of manipulatory techniques and psychological testing to an unpredictable and interesting article by Margaret Chandler on the implications of the increasing tendency for organizations to contract-out work that was once a part of their normal operations.

There is some attempt to meet the critics head-on, but as a whole the book is not just a defense of big business. In fact, a good deal of the book seems to give as much ground for worry to the friends of big business and/or individualism as the critics do. For example, it is observed in several articles that large organizations are not actually all rigid, highly routinized, monolithic structures, but that there is plenty of slippage between formal structure and actual operations, that the really big ones such as General Motors or the Defense Department are often more like feudal than totalitarian systems in their distribution of power, and that in general the complexity of modern organization creates both need and opportunity for individual initiative and responsibility. These observations might be given as evidence that big organizations are really not so bad. However, elsewhere in the

book it is suggested that these very characteristics may really be the source of a good deal of the trouble. Uncertainty and diffuseness of authority and responsibility may encourage initiative and creativity in a few sturdy people, but in many of the others, both managers and employees, they engender anxiety and frustration, which often lead to ritualistic behavior and stifling policies. Moreover, it is suggested that such individualistic organizational phenomena as managerial power struggles may be so disruptive as to lead many managements to decide that the best solution is simply to contract out the functions of departments, such as service departments, which are especially prone to involvement in these conflicts.

JOHN D. BREWER
University of Chicago

Computers and Thought. Edited by EDWARD A. FEIGENBAUM and JULIAN FELDMAN. New York: McGrawHill Book Co., 1963. Pp. 535. $7.95.

This is an excellent collection of articles. The title, however, is misleading. The papers do not carry broad general thoughts about computers. The editors concentrate specifically on papers that report on computer programs of artificial intelligence or simulations of human thought processes. The editors clearly state in their introductory remarks that this sharp focus on computer programs is intentional, and they have, in fact, limited the collection to working programs or papers that report on "results." While an anthology on computer programs seems highly uninteresting and extremely dull to anyone except a programmer, this is not the case here. The papers report on some highly creative research and are very stimulating. The progress made in programming computers to perform intellectual tasks, as reported in this volume, makes it "must" reading for every social scientist.

The editors have gone to great pains to make the collection useful as either a classroom supplement or a source of reference for the researcher. The volume is divided into four parts and several sections. Each part and section is prefaced with introductory remarks in which the editors attempt to clarify terms and arguments about the subject. In addition, virtually all the articles are definitive and are considered classic in the field. Of special value to the researcher is the descriptor-indexed bibliography prepared by Marvin Minsky (Part IV).

Part I is concerned with artificial intelligence, that is, computer programs that display intelligent behavior. The editors describe intelligent behavior as follows: "It appears that the clue to intelligent behavior, whether of men or machines, is *highly selective search*, the drastic pruning of the tree of possibilities explored. *For a computer program to behave intelligently, it must search problem mazes in a highly selective way, exploring paths relatively fertile with solutions and ignoring paths relative sterile*" (p. 6).

In attempting to make machines behave intelligently, the articles of Part I employ heuristic programs. According to the editors: "A *heuristic* (*heuristic rule*, *heuristic method*) is a rule of thumb, strategy, trick, simplification, or any other kind of device which drastically limits search for solutions in large problem spaces. Heuristics do not guarantee optimal solutions; in fact, they do not guarantee any solution at all; *all that can be said for a useful heuristic is that it offers solutions which are good enough most of the time.* A *heuristic program* is a program that employs heuristics in solving complex problems" (p. 6). Thus, Part I, presents articles that describe programs for playing and proving mathematical theorems. Also included are articles on a heuristic line-balancing procedure and a program for solving integration problems. While the games of chess and checkers and the applications to line-balancing and calculus may not be of general interest, the fact that programs have been developed that display intelligent behavior is of general significance.

In Parts I and IV, the editors and authors engage in some rather interesting (especially to the skeptic) academic discussions about the capabilities of computers. For example, the question of whether computing machines can think and learn is given extensive treatment. The clearest exposition is presented in the first article by the late A. M. Turing (pp. 11–35), a renowned mathematician and logician. If the reader is still wondering, the editors and authors in this volume believe that, while present results have been rudimentary, com-

puting machines are capable of analyzing and improving upon their own performance. The fascinating account of a checker program by A. L. Samuel (pp. 71–105) is an example of a learning routine. The performance of the program improves through game experience. In a match arranged by Samuel at the request of the editors (detailed at end of the article), the Samuel program actually bested a foremost checker champion.

While the works in Part I are concerned with producing programs that display intelligent behavior, the intelligence is artificial, that is, it was not necessarily the same as human intelligent behavior. In Part II, the articles are concerned with programs that perform tasks in the same way as humans. The level of explanation used in each of the reports is that the thought processes underlying human behavior display the characteristics of an information processing system.

> The basic premise of this approach is that complex thinking processes are built up of elementary symbol manipulation processes. A fundamental set of these elementary processes might be the following: read a symbol, write a symbol, copy a symbol, erase a symbol, and compare two symbols. Another basic facility that is required is the ability to take different courses of action depending on the outcome of the compare operation. If the two symbols are compared and found to be identical, the information processing system will take one course of action, i.e., execute one set of elementary processes. If the symbols are different, the system will execute another set of processes" [pp. 272–73].

The goal, of course, is to produce behavior that replicates the behavior of human beings when they are confronted with similar information. The articles of Part II report on the remarkable success achieved in creating models of elementary human behavior. The general problem-solving program of Newell and Simon (pp. 279–93) simulates behavior in solving logic problems. The articles presented in Section 2, one by Feigenbaum and the other by Hunt and Hovland, report programs that simulate elementary learning behavior.

Researchers have come closest to the "real world" in simulating the complexities of decision-making under uncertain conditions. The experiments of Feldman (pp. 329–46) resulted in program models of the processes underlying binary-choice behavior. Clarkson's program (pp. 347–71) of how an investment officer in a bank makes decisions on a portfolio of stocks has been most successful as a predictor of behavior.

Of greatest interest to the sociologist is the computer model of elementary social behavior developed by Gullahorn and Gullahorn (pp. 375–504). The model uses the propositions of George Homans in *Social Behavior* and employs the usual approach to simulated subjects, that is, treating them as information-processing organisms. In the context of social interaction, the information processing person must be able to receive stimuli, analyze them, commit them to memory; "he must be able to emit activities, differentiate reward and punishment, associate a stimulus situation with a response, and associate a response with a reinforcement; and, on the basis of past experience, he must be able to predict the probability of reward resulting from each response he contemplates. In social situations he must be able to differentiate among other members of a group, evaluate a social stimulus in terms of the specific person emitting it, and select his response accordingly so as to elicit a positive reaction in turn" (p. 376). The Gullahorns have a working program that simulates such a person in interaction with another person.

A broad overview of machine intelligence is provided in Part III by two excellent articles. The article by Armer (pp. 389–405) reviews and provides interesting discussion about attitudes toward intelligent machines. His examination not only covers Western attitudes but also provides some rare insights into Russian views about intelligent machines. The article by Minsky (pp. 406–50) gives a thorough analysis of artificial-intelligence research. He discusses the problems, shortcomings, and successes experienced in the field. His efforts provide clarity and undertanding in a most complex but stimulating subject.

The editors have done a thorough job in putting this collection together. Their efforts will satisfy a wide range of readers. There is enough introductory material for the beginning student, enough depth for the researcher, and enough discussion about the broad issues to satisfy the most critical of readers.

Rocco Carzo, Jr.

College of Business Administration
Pennsylvania State University

CURRENT BOOKS

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Social Class and Preferred Family Size in Peru[1]

J. Mayone Stycos

ABSTRACT

Data from a survey of currently mated women in Lima, Peru, and a highland Peruvian town indicate a positive relation between social class and number of children desired but a negative relation between class and previous thought or discussions of family size. Lower-class preferences for small families may fail to be activated because of various misperceptions of fertility and mortality. In particular, the lower classes tend to overestimate average fertility and average ideal family size and to view fertility and mortality as unchanging or increasing.

Until recently it was generally believed that the uneducated masses in underdeveloped areas desired large families. However, studies in a wide variety of countries within the past decade reveal that most lower-class women would prefer to have only three or four children, though they in fact have more. The older error of inferring attitudes from behavior has been replaced by what may be excessive optimism about the motivation of lower-class women to limit the size of their families.

The present investigation was designed, in part, to ascertain the existence and significance of differential family size preferences among various social classes in a Latin American country. In 1960 and 1961, with the financial assistance of the Population Council and the collaboration of the Peruvian School of Social Work, about 2,000 currently mated women in the city of Lima, Peru, and several hundred women in the highland town of Huaylas were administered a questionnaire on fertility and attitudes toward fertility.[2] Interviewers were instructed to categorize the respondent's social class in one of four ranked groups (from A, highest, through D, lowest), depending on a list of criteria provided by a Peruvian commercial research firm. Although the judgments were largely subjective, differences among the classes in education, expenditure, occupational distribution, etc., are marked. These subjectively determined categories will be used throughout the present analysis. Data on Huaylas occasionally will be introduced for contrast with Lima, but the small number of cases and skewed distribution

[1] Revision of a paper presented at the 1963 American Sociological Association meetings.

[2] Since no census had been taken in Peru since 1940, 100 blocks were chosen at random from recent maps of Lima, and twenty currently mated women between the ages of 20–44 were selected systematically from each block, yielding a total of about 2,000 interviews. For contrast with the capital city, the highland town of Huaylas was chosen; and 344 women, representing the total eligible population who could be contacted, were interviewed. For further detail concerning sampling and interviewing see J. Mayone Stycos, "Female Employment and Fertility in Lima, Peru," *Milbank Memorial Fund Quarterly*, January, 1965.

of social class there prevent detailed breakdowns.

Table 1 shows the distribution of responses to the question, "If you were to live your life over, how many children would you like to have in all?" Means are also given for responses to the question, "How many children would you like your daughter to have?" As might be expected, Huaylas stands out in the number of children considered ideal, with a quarter of the women wanting seven or more, and with a mean desired number of five. Within Lima, however, we see a *direct* relation between class and preferred number. Despite the fact that there is also a direct relation between preferred number and actual number of living children, standardizing for the number of children does not diminish the differences between classes in the desired number of children. A similar relation prevails with respect to the number desired for one's daughter. No less than 46 per cent of the women in the lowest class would like their daughters to have two or fewer children, whereas only 9 per cent of the upper class women prefer such a small number of children for their daughters.

Since lower-class women want fewer than the upper class but in fact have more, it is not surprising to find that a third of the lower-class women have already exceeded the number they consider ideal, as contrasted with only 12 per cent of the upper class.[8] If the respondents are consistent, we would then expect that the lower class would be less interested in having more children than they now have. This turns out to be true, as measured by

TABLE 1

NUMBER OF CHILDREN PREFERRED, BY SOCIAL CLASS AND RESIDENCE

No. of Children	Lima				Huaylas
	Class A	Class B	Class C	Class D	
0-2	8	17	29	31	22
3	17	18	13	15	11
4	37	30	24	18	12
5-6	26	24	22	21	21
7+	7	6	8	9	12
All God sends	4	3	3	4	13
No answer	1	2	1	2	9
Total	100	100	100	100	100
Mean no. preferred*	4.6	4.3	4.1	4.0	5.1
Mean no. preferred, standardized for no. of living children†	4.7	4.4	4.0	4.0	5.1
Median no. preferred for daughter	4.1	4 0	3.3	3.2	3.6
No. of cases	(253)	(490)	(757)	(495)	(344)

* Eight or more preferred children scored as 9; "all God sends" scored as 10.

† Standardized by the distribution of living children for the entire Lima sample.

[8] These figures are derived from direct cross-tabulations of the exact ideal number with the exact number living. The discrepancy is apparent at all parities; e.g., for women with less than four children, only 4 per cent of the lower-class but 13 per cent of the upper-class women have at least one more living child than they consider ideal. For women with five or more children the corresponding figures are 40 and 64 per cent. These findings are in marked contrast to those found for a group of several thousand Israeli maternity cases, where "less than 3 per cent admitted a preference for some number smaller than the number they already had" (R. Bachi and J. Matras, "Family Size Preferences of Jewish Maternity Cases in Israel," *Milbank Memorial Fund Quarterly*, XLII [April, 1964], 45).

a direct question on whether or not additional children are desired (Table 2). At every parity from one to six or more living children, the lower-class women are less likely to want additional children. Even when age and parity are simultaneously controlled, the class difference holds up in most instances. It is also of interest that age and parity are independently related to the desire for more children, controlling class. A similar pattern holds for the median preferred number of children (not shown), except that the relation to age is no longer a consistent one. In short, we find a persistent tendency for lower-class women to prefer fewer children than upper-class women.

This finding, while contrary to popular stereotypes about the lower classes in Peru, is entirely plausible. Urban lower-class aspirations for education and material acquisitions may not differ greatly from those of the upper classes, but their ability to achieve them is far less. Thus, 98 per cent of classes A and B said they wanted, and 96 per cent said they expected, their youngest female child to have a secondary-school or university education. Class C has about the same desire (93 per cent), but their expectation drops off sharply to 71 per cent. In Class D the discrepancy is even larger: 71 per cent desire secondary-school education but only 48 per cent expect it.[4] (In Huaylas, aspirations and expectations come together at a *low* level —*15* per cent and *12* per cent.) The gap between desired and *achieved* education for children is doubtless even greater. In such a situation, children are a greater disadvantage to the lower than to the upper classes.

While this may be so objectively, what evidence do we have that it is so perceived by the lower classes? To what extent are the class differences in desired family size due to the perception of the *economic* disadvantages of children? To answer this question we asked whether having an additional child would affect the family's economic situation, and if not, whether having three more would affect it. The same type of question was asked with respect to the effect an additional child would have on the respondent's health. ("Do you believe that having one more child would harm, improve, or not affect your health?")

TABLE 2

DESIRE FOR MORE CHILDREN, BY SOCIAL CLASS, AGE OF MOTHER, AND NUMBER OF LIVING CHILDREN, LIMA

AGE OF MOTHER	PER CENT WHO WANT NO MORE CHILDREN		BASES FOR PERCENTAGES	
	Classes			
	A, B	C, D	A, B	C, D
0–1 living children:				
20–29	2	14	123	155
30–34	7	21	28	34
35+	31	29	32	38
2–3 living children:				
20–29	15	32	114	260
30–34	30	33	96	99
35+	50	54	137	93
4+ living children:				
20–29	40	58	20	140
30–34	36	54	55	166
35+	62	73	138	265

For present purposes, the significant finding is that there is a marked difference between classes with respect to the question on economic impact but not with respect to health. When child parity is introduced, lower-class women with less than two children are more likely than upper-class women to indicate that an additional child would affect their health adversely. But for two children or more, there is no difference between the classes. On the other hand, for the effect on the economic condition of the family, at each parity from two to three times as high a proportion of lower-

[4] Figures for male children are virtually identical.

class as upper-class women answered the question affirmatively.[5]

Table 3 gives a more comprehensive summary of the responses to the economic question by combining information referring to both one more and three more children.

At each child parity the economic sensitivity of lower classes to additional children is higher than that of the upper classes.

TABLE 3

MEAN ECONOMIC SENSITIVITY SCORE, BY NUMBER OF LIVING CHILDREN AND SOCIAL CLASS,* LIMA

Living Children	Classes A–B	Classes C–D
0	1.6	2.2
1	1.9	2.6
2	2.4	2.7
3	2.3	2.9
4	2.4	2.9
5	2.5	2.8
6+	2.1	3.0

* Respondents were scored from one to four depending on combinations of responses: 1 = neither one nor three more children would affect the economic situation; 2 = three more children would affect it a little; 3 = three more children would affect it a lot; 4 = one more child would affect the economic situation.

Moreover, while the sensitivity increases with a fair degree of regularity as parity increases among lower-class women, the trend is less clear for upper-class women; those with six or more children are no more sensitive than those with two. In short, differences revealed between social classes in desired numbers of children are consistent with differences in concern over the *economic* impact of additional children.

But to what extent does the perception of economic and health disadvantages of additional children predict the actual desire for more children? People might believe children a financial and health disadvantage and still desire more because of other advantages. Or they might feel no economic or health disadvantages and still want no more children. Table 4 shows that there is indeed a relation. If an additional child is felt to harm both the economic and health situation, then in nine out of ten instances in Lima the woman will want no more children. However, the absence of such feelings does not guarantee that a woman desires more children; half of those who feel no economic or health disadvantages still do not want any more children. Put another way, 46 per cent of those not wanting more children feel that neither their health nor economic situation would be affected by having an additional child. This might suggest that motivations other than these are of considerable importance, for example, the amount of energy required to rear children, or a sense of inappropriateness about having "too many" children. Since 95 per cent and 61 per cent of the two upper classes but less than 5 per cent of the lower classes have servants, it is clear that the former are far less concerned with energy loss due to additional children than are the lower classes.

TABLE 4

PER CENT WHO WANT NO MORE CHILDREN, BY OPINION ABOUT IMPACT OF AN ADDITIONAL CHILD ON HEALTH AND ECONOMIC SITUATION, LIMA

Economic Situation	Health Would Be Harmed	Health Would Not Be Harmed
Would be harmed	90	73
Would not be harmed	64	48

We have seen that women in the lower classes want fewer children than the upper classes, but in fact have more. There is, of course, a long and complex chain between

[5] Only 14 per cent of upper-class women with two living children think an additional child would affect their economic situation, and this figure does not vary as the number of children increases. On the other hand, 23 per cent of the lower-class women with two living children report that an additional child would affect their economic situation, and the proportion increases steadily to 39 per cent of those with six or more children.

felt desire for a particular number of children and its achievement, and we obviously cannot expect to discover all the intervening mechanisms in this particular analysis. However, the attitudes toward numbers of children that we have been discussing can be of various kinds. For example, when a middle-class American woman says she want no more children or that an additional child would cause economic difficulties, the likelihood is that she has given the matter a good deal of thought, has discussed it with her husband, knows what to do about it, and knows what her friends are doing about it, etc. We believe that the same statement from a lower-class Peruvian woman does not generally imply any of these things, and may imply only a *latent* interest in controlling family size which at the moment of the interview may represent little more than wishful thinking.

Several types of data support this conclusion. Thus, when women are asked, "Have you ever thought about the number of children that you would like to have, or haven't you thought about it?" the proportion who answer positively drops markedly with social class: 73 per cent in the top class, 62 per cent in class B, 42 per cent in class C, and 35 per cent in the lowest class. In Huaylas the comparable figures are 23 per cent and 16 per cent for the upper and lower classes. Moreover, among those who have thought about it, lower-class women are much less likely than upper-class women to have discussed it with their husbands, the percentage dropping from 87 in class A to 57 in class D and 49 in Huaylas.[6] Notwithstanding the fact that lower-class women readily responded to our questions on desired numbers of children and wanted fewer children than upper-class women, only a minority say they have ever thought about it or discussed it with their husbands. Moreover, in Lima the more children a woman has the less likely she is to report ever having thought about it. Both class and parity independently affect the relation, so that as many as 90 per cent of the class A women with one birth or none, but only 29 per cent of the class D women with five or more births ever thought about the number of children they wanted. While it might be thought

TABLE 5

PER CENT WHO HAVE EVER THOUGHT ABOUT NUMBER OF CHILDREN WANTED, BY SOCIAL CLASS, NUMBER OF LIVING CHILDREN, AND AGE OF MOTHER, LIMA

AGE OF MOTHER	NO. OF LIVING CHILDREN		
	0–1	2–3	4+
Classes A and B:			
20–29	79	71	65
30–34	86	69	56
35+	77	64	49
Classes C and D:			
20–29	56	40	33
30–34	41	44	34
35+	47	54	29

that the parity relation in fact conceals a relation with age, Table 5 shows that in most instances women under 30 are no more likely to have thought about it than women 35 and over.[7] Class and parity, however, still maintain their relation to thought about ideal size.

Since we did not collect data on contraceptive practices, we cannot test the most plausible hypothesis: that those who think about family size are more likely to attempt to control it.[8] Of special interest

[6] This does not impede women from specifying their husbands' desires. For example, in the lowest class, despite the fact that only a fifth of the women have ever discussed the number of children they and their spouses want, 93 per cent answered a question asking how many more children the husband wanted.

[7] In the Israeli study, where only 40 per cent reported ever having considered the desired number of children, a marked negative relation to parity and little relation to age were also found (Bachi and Matras, *op. cit.*, p. 40).

[8] This proved to be the case in Puerto Rico and Israel. See R. Hill, J. M. Stycos, and K. W. Back, *The Family and Population Control* (Chapel Hill:

here, however, is the fact that unlike our previous attitude-items on family size, thought and discussion about family size show a positive relation to social class and a negative relation to fertility.

Thus we noted that desire for more children decreased and sensitivity to the economic implications of additional children increased with greater number of children, but thought about the matter diminishes. In fact, there is no relation between expressed attitude toward the number of children and whether or not the matter has been discussed or thought about (Table 6).

TABLE 6

ATTITUDES TOWARD FAMILY SIZE BY PREVIOUS THOUGHT AND DISCUSSION. CLASSES C AND D,* LIMA

	PER CENT		
	Three More Children Would Not Affect Economic Situation	Three More Children Would Not Affect Health	0–3 Children Preferred
Discussed number desired	17	40	42
Thought about but didn't discuss number desired	15	36	42
Have not thought about number desired	18	38	45

* The same absence of relation exists among the two upper classes.

The fact that most lower-class women have not thought about family size and that there is no relation between desire for few children and thought or discussion about it leads to questions concerning the meaning of expressed attitudes on family size. The consistent preference among the lower classes for smaller families regardless of the measure utilized and regardless of age or parity suggests that the responses are not random. However, we believe that, because the degree of factual information about fertility and birth control is so limited and distorted for the lower classes, attitudes favorable to the small family remain in a latent stage, perhaps largely unrealized by the respondent until articulated to an interviewer.

Such topics are not discussed in the mass media in Peru nor are they a part of public health services. Since the lower classes have little access to private physicians or scientific literature, we wished to assess their knowledge of general facts concerning fertility and mortality. As we can see from Table 7, it is clear that most lower-class women in Lima do recognize the existence of differential fertility in Peru—that rural and poor women have more children than do urban and rich women. Most of the Huaylas women, on the other hand, are ignorant of differential fertility and of changes in fertility and mortality. In Lima, with the exception of differential fertility, there are significant proportions misperceiving important social facts, and this misperception is inversely related to social class. Thus, as we move toward the lower class, increasing proportions believe there has been no change in fertility in the past generation; and of those who do see a change, over a third of the lower-class women believe fertility has *increased*. It might be thought that this is the result of perceptions of reduced infant mortality which make contemporary families seem larger. However, we see that lower-class women are also less likely to believe there has been a change in mortality, and, among those who do, over half of the two lower classes believe there has been an increase in mortality.[9] Moreover, those who say fertility has increased are somewhat less likely to say that mortality has declined.

University of North Carolina Press, 1959), pp. 317–18; and Bachi and Matras, *op. cit.*, p. 48.

[9] According to pre-test data, this is because they feel that the air, food, and life in general were more healthy in the "good old days."

Another interesting finding is that lower-class women believe the average woman bears ten or eleven children. In our own sample, currently married women aged 40–44 in the lowest-class group had an average of 7.2 live births. Census data for 1940 recorded only 6.1 live births for mothers 40–44 living outside the Lima-Callao area. Thus, lower-class women are greatly overestimating the fertility of the average woman.

TABLE 7

PERCEPTIONS CONCERNING FERTILITY AND MORTALITY

	Lima				Huaylas
	Class A	Class B	Class C	Class D	
Per cent who say poor have more children than rich*	75	88	94	89	29
Per cent who say rural women have more children than urban†	90	92	82	74	21
Per cent who see no generational change in fertility‡	16	11	22	36	64
Per cent of remainder who believe fertility has increased‡	5	7	18	38	47
Per cent who see no generational change in mortality§	19	18	22	29	66
Per cent of remainder who believe mortality has increased§	20	37	56	59	30
Median births by age 55 attributed to other women‖	6.5	8.2	10	11.4	10.1
Per cent who believe other women want same number of children as respondent#	53	47	37	37	51
Per cent of remainder who believe other women want more children than respondent#	14	20	34	35	54

* Do rich women have more children, fewer children, or the same number of children as poor women?

† Do women from the country have more, fewer, or the same number of children as women from Lima?

‡ In your parents' generation, did women have more, fewer, or the same number of births as nowadays?

§ In your parents' generation, did more, fewer, or the same number of children die as nowadays?

‖ How many births have most women had by the time they are 55 years old?

\# Do other women want to have more, fewer, or the same number of children as you?

Finally, lower-class women are less likely than upper-class women to believe other women desire the same number of children as they themselves do. Of those who see a difference, the lower classes are much more likely to think other women want *more* children than they. Thus lower-class women are more likely to believe that other women want more children and have more children than they do.

CONCLUSIONS

We initially raised questions about lower-class motivation with respect to family size. It was found that lower-class Lima women desire fewer children and are more sensitive to the economic implications of additional children than are upper-class women with the same number of children. Moreover, in absolute terms, preferred family size is moderate; half of the women want no additional children after they have had two.

Further questions were raised about the significance of verbal statements of preference in a social context where the topic is not generally a public one and where the society does not support in a normative or technological sense the realization of such ideas and preferences. It was found that only a minority of lower-class women report they had ever thought about the number of children they desired, and even fewer had ever discussed it with their husbands. The preference for only a few children is probably a latent one which is brought out by the interviewer's questions but which seems to have little significance for behavior.

In the absence of a social context in which information on family size and

family-size goals is shared, a number of erroneous beliefs are current which further serve to inactivate small-family preferences. Lower-class women greatly overestimate the average fertility of other women, suggesting that they must think of their own fertility as quite low. Similarly, much more so than is true for women in the upper classes, they believe other women desire more children than they do. They are also much more likely to believe that both fertility and mortality have increased since their parents' generation. Lower-class women are, however, generally aware that differential fertility exists between social classes and residential groups. Most of the generalizations hold in an even more exaggerated form for the rural women.

The implications for applied programs are that, while lower-class urban women do not have to be convinced of the general desirability of having a moderate number of children, they need to have their latent inclinations activated by shifting discussion of the whole matter into socially approved channels. In addition to technological information, they must learn that other women want small families, that their own fertility is not lower than that of others, and that mortality as well as fertility has declined. Most of all, they must be encouraged to think about and articulate private opinions about family size until public opinion is created. A program that emphasizes not the creation of new goals but the social implementation of latent ones should have unusual appeal in urban Latin American societies that are typically conservative about public programs of fertility control.

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Age Distribution as a Challenge to Development

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ABSTRACT

The 1961 census of Indonesia suggests that the number of entrants into the labor force will double during the next five years and that their literacy rate will be 75 per cent. Literate citizens with high expectations will be impatient with the minute division of labor and product in village agriculture; even those willing to work along traditional lines will find no land remaining. A large movement to the cities can be anticipated, but it will encounter the difficulty that the generation of potential managers is relatively small, for demographic and other reasons. These facts can be expected to modify Indonesian life sharply, the direction of modification depending on whether or not the crisis is interpreted as an issue of production in relation to population.

Much attention is being given in the new countries to the creation of new men; the most malleable material out of which to make national citizens is the young adult population. The demographic fact of a relative surplus of young adults combines with the molding of them by national civilian and military agencies and the educational system to create directions of economic and social pressure whose effects are becoming visible in certain countries. In this paper the age distribution of Indonesia will be presented in some detail; comparison will be made with other countries of Asia; finally some of the economic and social consequences of the changing distribution will be traced out.

1961 AGE DISTRIBUTION OF INDONESIA

The Indonesian census of 1961 has now been tabulated on a 1 per cent sample, of which the classification by age and sex is reproduced below as Table 1. Following down the column for both sexes together we note the drop from over 15 million in each of the first two age groups to about 8 million subsequently, and the fact that the age distribution is very nearly flat from ten to thirty-five. (Ages from twenty-five onward must be divided by two because they are in ten-year age groups.) The flatness or emptiness of these ages has been cited as a disadvantage under which Indonesia suffered in the 1950's;[1] the percentage of the population in the labor force was smaller than it would be with a more normal distribution.

The empty ages, as well as the over-full ages below ten in 1961, fit with well-known

TABLE 1

POPULATION OF STATED AGE BY SEX, INDONESIA, 1961

Age	Male	Female	Total
0- 4....	8,462,000	8,580,000	17,042,000
5- 9	7,684,000	7,639,000	15,323,000
10-14....	4,318,000	3,861,000	8,179,000
15-19....	3,834,000	3,874,000	7,708,000
20-24. . .	3,452,000	4,339,000	7,791,000
25-34...	7,334,000	8,542,000	15,876,000
35-44....	5,720,000	5,363,000	11,083,000
45-54 .	3,559,000	3,483,000	7,042,000
55-64. .	1,898,000	1,850,000	3,748,000
65-74...	796,000	829,000	1,625,000
75+.....	377,000	407,000	784,000
Total...	47,434,000	48,767,000	96,201,000

Source: Central Statistical Office, *1961 Census, 1% Sample* (Djakarta, 1964).

historical facts. The decade of the 1950's was relatively healthy and prosperous. Malaria had been in considerable part eliminated; food was far more available than in the 1940's, according to the recollections of those who passed through these periods

[1] K. Horstmann, *Ekonomi dan Keuangan Indonesia*, 1959.

(unfortunately, one of the evils of war and revolution, although by no means the greatest, was the fact that no one had the time to collect statistics). One can surmise that births as well as deaths differed between the 1940's and the 1950's; certainly during the occupation and the revolution marriages were delayed, and this, as well as separation of married couples, would have brought the birth rate down.

ACCURACY OF THE DATA

Could there be some error in the enumeration of the census sufficiently gross to account for the notch at age ten? Fortunately some evidence on the accuracy of the census is at hand, covering this particular point on which the entire succeeding argument will be based. We have a separate and independent labor force survey, taken in 1958, whose results for the crucial ages are shown in Table 2. We also have a demographic survey of December, 1962; although this survey was not entirely independent of the census, the figures are worth examining. The three sets of data are available most conveniently for rural Java and Madura, and it is to this important part of Indonesia that Table 2 is confined. Since the main phenomenon we are concerned with is common to both rural and urban parts, according to the census, we will be satisfied if the census is confirmed for rural Java and Madura.

TABLE 2

POPULATION OF RURAL JAVA AND MADURA AT THREE DATES

Age	Labor Force Survey, 1958	Census, 1961	Demographic Survey, 1962
0– 4........	7,901,000	9,332,000	9,068,000
5– 9.... ...	6,919,000	8,492,000	8,518,000
10–14........	3,958,000	4,229,000	4,828,000
15–19........	3,914,000	3,868,000	3,714,000
Total, all ages.	48,339,000	53,186,000	54,782,000
0–9 as percentage of total..	30.7	33 5	32.1
5–9 as multiple of 10–14....	1.75	2.01	1.76
0–10 as multiple of 10–19.	1.88	2 20	2.06

In its main lines it is indeed amply confirmed. We see that the numbers in the first two age groups are in all cases much larger than the numbers in the next two. Moreover, the ratio of the 5–9 to the 10–14 age group is higher for the census date, 1961, than for either of the other two dates. This is what one would expect if the break between the reproduction of the war and revolution on one side and of the peaceful period of the republic occurred exactly in the period 1950–51. For this would make the 5–9 age group in 1958 somewhat short because it contains about two years of the low 1940's; it would make the 10–14 age group of 1962 somewhat high because it contains one or two years of the high 1950's. The 0–4 of the labor force survey is not quite as high as the census would have us think; the discrepancy is of the order of 10 per cent. But the study of the three sets of data both confirms the census and permits us to locate the moment of change as centered on the year 1950.

TABLE 3

POPULATION AGED 0-19 PAKISTAN, 1961

0– 4........	15,722,021
5– 9........	16,001,151
10–14........	8,455,346
15–19..	7,438,928

It is of interest to see whether another country with a similar history of difficulties in the 1940's and relative security in the 1950's shows a similar age distribution. Pakistan meets the requirement of approximate similarity of conditions, and Table 3 shows for the first twenty years of age somewhat the same profile as does Indonesia. However, it appears that the Pakistani enumerator was required to fill an extra labor force form for those over ten, and this could have resulted in some distortion. I have no evidence on which to judge this point.

The censuses are certainly not exact; in

fact those who have been closest to the operations speak with the greatest modesty about the accuracy they have succeeded in attaining. Further work on the tabulations and perhaps further surveys or even the next census will have to be awaited before the last word can be said. But one does not need this last word to be certain that a considerable difference exists between the number of persons under ten and those aged 10–19. Even if the figures have departed from the reality by being 10 per cent in excess for the younger group and 10 per cent short for the older, the statements concerning the pending increase would still be more than two-thirds true, and this would suffice for the present discussion.

THE IMPENDING EXPANSION

We can probably do better than this on estimating the moment of change from the regime of low increase to that of high increase. For we also have at our disposal the census figures by single years of age; these are given in Table 4. The original census figures do not move smoothly from age to age; the strongest champion of the census must admit the possibility, for example, that many children of age eleven were given as age twelve. In order to get around this, one can perform a graduation with the use of Sprague multipliers[2] and obtain the results shown in the third column of Table 4. The reader who prefers some other graduation, who perhaps wishes to do his own by graphical means, is advised to do so; he can hardly reach conclusions very different from those that follow.

The striking feature of the table is the drop from over 3 million at age seven to under 1½ million by age twelve or thirteen. But to call this a drop is correct only when the table is read from top to bottom. In the book of real history this table is read from the bottom upwards. The successive ages are successive classes or cohorts, and they file past the grandstand from the bottom of our list to the top. Those who were age fourteen in 1961 are nineteen in 1966 and twenty-four in 1971. It is convenient to take one single age as the point at which to stand in reviewing the march-past of the cohorts, and we chose age seventeen. This is not the earliest that Indonesians start to work, especially in the villages, but one can say that by that age the large majority have left school and are in the labor market. The table shows that at the present time, as Table 1 shows for the past, the number that reach seventeen year by year is less than 1½ million; in 1966 it is still less than 1½ million. But then it takes a sudden jump—on the graduated figures it goes to 1,742,000 in 1967. By 1968 it has passed the 2 million mark; in 1969 it is close to 2½ million; in 1970 it is approaching 3 million, and in 1971 it has passed that figure. The witness of this march-past of the generations may well watch with breathless suspense. Is the increase going

TABLE 4

POPULATION AGED 0–14 AS REPORTED IN CENSUS AND AS SMOOTHED BY SPRAGUE MULTIPLIERS,* INDONESIA, 1961

Age in 1961	Census	Smoothed	Year in Which Attain Age 17
– 1...	3,171,000	....	1978
1...	3,009,000	....	1977
2 ..	3,516,000	....	1976
3. .	3,769,000	. ...	1975
4 ..	3,578,000	.. ..	1974
5...	3,296,000	....	1973
6 ..	3,073,000	3,360,000	1972
7 ..	3,327,000	3,108,000	1971
8 ..	2,884,000	2,812,000	1970
9...	2,743,000	2,492,000	1969
10 ..	2,381,000	2,139,000	1968
11...	1,341,000	1,742,000	1967
12 ..	1,912,000	1,466,000	1966
13 ..	1,344,000	1,389,000	1965
14	1,200,000	1,444,000	1964

* See text, n. 2.

Source: *1961 Census, 1% Sample.*

[2] See U.S. Bureau of the Census, *Handbook of Statistical Methods for Demographers* (Washington, D.C.: Government Printing Office, 1951), pp. 95, 96.

to continue? We cannot be sure, but it will probably not surpass 3½ million for a considerable period, perhaps not until the 1990's. Some uncertainty on this score arises as the result of the smaller numbers below two years of age; whether these reflect a real drop in births, the omission of infants in the census, or the overstatement of their ages is not possible at the moment to say.

TABLE 5

INDEX OF DEMOGRAPHIC CHANGE BETWEEN COHORTS FOLLOWING 1950 AND THOSE PRECEDING 1950, DERIVED FROM CENSUSES ABOUT 1960

	Population 0–9 ÷ Total Population (Per Cent)	Population 0–9 ÷ Population 10–19 (Ratio)
Cambodia, 1959	33.0	1.54
Indonesia, 1961	33.6	2.04
Rural Java and Madura, 1961	33.5	2.20
Malaya, 1957	33.0	1.60
Pakistan, 1961	35.1	2.00
Philippines, 1960	33.0	1.43
Singapore, 1957	33.4	1.78
United States, 1960	21.8	1.30

A NUMERICAL MEASURE OF SUDDENNESS OF CHANGE

The phenomenon of sudden increase in the number of young adults noted for Indonesia and Pakistan ought to be measurable in terms of some single index. In casting about for such an index one thinks first of the percentage of the population under age ten. But this does not measure what we have in mind, for we are thinking of a phenomenon of transition from high to low death rates rather than of any possible stable condition, and it is easy to find instances of stable populations with as high a percentage under ten years of age (33–35 per cent) as is shown in Indonesia and Pakistan. If it is the rapidity of change between the 0–9 and the 10–19 age groups which is the essence of the situation in these two countries, then we ought to consider the ratio of the second of these to the first. Both of these measures are given in Table 5 for a number of the countries of South Asia and for the United States.

The Asian countries shown are uniform in their percentage under ten years of age, and all are half as high again as the United States figure of 21.8 per cent. But the countries of Asia differ considerably from one another in the steepness of declines of numbers as one proceeds up the pyramid of ages. Indonesia and Pakistan are uniquely steep; they show twice as many individuals under ten years of age as between ten and nineteen. Here again we must be aware of possible enumeration error.

EDUCATION

The entrants into the labor force in all new countries from now on will have more formal education than the entrants of the past. There is abundant evidence of this fact as far as Indonesia is concerned in the 1961 census; some figures of simple literacy are reproduced in Table 6. The ability to read (in Roman, Arabic, or other characters) is sharply differentiated by age, sex, and rural-urban residence; but the differential that seems to stand out most is that by age. The enormous success of the educational process in reaching the young appears especially in the fact that 70 per

TABLE 6

LITERACY BY AGE, INDONESIA, 1961 (PER CENT)

Age	Males		Females		Total
	Urban	Rural	Urban	Rural	
10–14	89.8	73.5	84.7	64.0	72.1
15–19	92.3	72.9	79.9	54.3	67.7
20–24	89.0	70.2	64 4	40.1	57 5
25–34	79.2	58.5	47.8	26.9	44.6
35–44	74.3	49.4	37.1	18.2	37.4
45–54	65.1	38.2	24.2	9.4	26 6
55–64	57.7	29.0	17.5	6.1	19.9
65–74	50 6	20 6	12.2	5.0	14.9
75+	40.7	21.3	9.5	6.0	14.7

Source: *1961 Census, 1% Sample.*

cent of rural males under twenty-five are literate, as against only 20 per cent of those over sixty-five. The investment in human capital, especially for the cohorts born in 1930 and onward, is impressive by any standards. Considerable effort is now being made to expand the educational system in accord with the increase in numbers of children, but little statistical evidence is available on its exact extent. Aside from the self-judged literacy declared to the census taker of Table 6, nothing is in print regarding the effect of the educational system on productive competence. But there can be no question regarding its effectiveness in inculcating a national outlook and attitude.

ENTRY INTO THE LABOR FORCE

The number of persons entering the age groups at which the labor force is recruited, now about 1½ million annually, will rise to over 3 million per year very early in the 1970's. The evidence for this is essentially what we saw in Table 1 for ages 10–14, where some 8 million in 1961 are replaced by 15 million in 1966. We are here using census figures for the ages 5–14 in 1961 without deducting deaths; the justification for this is that deaths at these ages are low everywhere, and they seem to be particularly low with the level of health that has been attained in Indonesia.

What ordinarily happens is that an age cohort moves up through the labor force each year; people are promoted, retire, or die, and their places are taken by the young entrants who come in at the bottom of the hierarchy. This is how most of the 1½ million have been absorbed in Indonesia year by year up to now and will continue to be absorbed up to 1966. But by the beginning of the new decade the annual number of potential entrants will be double the number to which the country is now accustomed. Each entrant will literally bring his brother along, and the brother will want a job as well. What sort of jobs will they want?

For the countryside the answer depends on the degree to which such youth, with training often including middle school, will be willing to share in the minute division of labor and of the crop which characterizes village life in Central and East Java, for instance, and whether there will be places for them. This question applies even today to the smaller numbers who are finishing their schooling and going back to the village. They are having trouble locating themselves in the economic circuit. The census throws light on this problem: Table 7 shows unemployment data for rural areas. We see that unemployment is predominantly a problem of those under twenty-five; the men beyond that age are evidently located on the land in one way or another. For better or worse, their place in society is determined; even though in a bad season they may face starvation, they do not describe themselves as unemployed to the census enumerator. The census figures indicate that, on the other hand, either there is no work for one-fifth of the young, or else they are not willing to undertake the tasks that sufficed for their parents. This superfluity or dissatisfaction of the young appeared even in 1961, when they constituted an "empty" age group, abnormally depleted by the war and revolution.

TABLE 7

MALE LABOR FORCE UNEMPLOYED IN RURAL AREAS, BY AGE, 1961 (PER CENT)

Age	Indonesia	Java and Madura
10–14	22.4	24.3
15–19	15.6	18.4
20–24	7.5	8.9
25–34	1.9	2 0
35–44	0.8	0.7
45–54	0.8	0 6
55–64	1.0	0.8
65–74	1.9	1.6
75+	2.6	2.7

Source: *1961 Census, 1% Sample.*

MIGRATION

What about the enlarged age groups that are expected from 1967 onward? They will clearly be tempted to migrate, and to this subject we now turn our attention. The census shows how much net migration there has been, particularly in the rural-urban direction, during the past thirty years. Its figures will suggest the movement likely to occur during the late 1960's and 1970's.

Growth of cities has been rapid during the thirty-one years preceding the census. Djakarta Raya increased from 533,000 residents in 1930 to 2,973,000 in 1961, a growth of five and a half times. Bandung increased at an even higher rate, coming close to the million mark. Surabaya multiplied by three to exceed one million. These figures are impressive, especially when one bears in mind that the ages at which migration takes place, say fifteen to twenty-five, were underrepresented in the Indonesian pyramid during the period in question.

The doubling that is in prospect at these ages therefore permits the safe prediction that cityward migration will shortly be intensified. The youth who are being turned out by the rural and small-town schools, educated and upwardly mobile, will move where opportunity seems to beckon. The idea that there is opportunity in the cities is inevitably given by the construction which has taken place during the past few years. New boulevards, stadiums, hotels, government buildings, especially in Djakarta, all suggest to the young man of the *desa* that the city is the place to come for the opportunity to introduce oneself into the economic circuit of production and consumption.

COMPLEMENTARITY OF AGES IN PRODUCTION

Clearly the doubling of the youthful cohorts within the five-year period 1966–71 represents an enormous opportunity for development. The major question will be the availability of organizing skill and capital to put it to work productively. One cannot expect that people of seventeen years of age will provide their own opportunities. A kind of complementarity of ages operates in production, which requires certain proportions of skilled and experienced people along with the young. This applies whether enterprise is private or governmental; the organizing of production is never an easy matter. Indonesia is handicapped at this stage of its development by the fact that the older generation continues to be relatively small in numbers, as has been stressed above; its proportion literate is relatively small and its proportion with higher education, especially higher technical education, is smaller yet. The smaller old generation will have to guide the productive effort of the large new generation. Needless to say, if a spirit of enterprise proves to be sufficient, the great increase of youth, its education and determination to make something of life whether in agriculture or the city are nothing but advantages for the country.

THE ELITE AND ITS OBJECTIVES

An elite that has come into existence through political struggle is likely to see its continuing success in terms of the qualities and actions that brought it to the forefront to begin with. The giving of authority to owners and managers on the basis of skill in production would constitute a major change. An elite may develop an ideology which is especially adverse to industry in proportion as its career has consisted in argument or warfare with foreigners whose claim to domination was their superiority in managing production. Such a history may bring about an incapacity to perceive the role of management or the fact of talent for productive organization. We are all partially blind, and people choose what they will and will not perceive in accord with social need. It requires especially strong pressures and very patent needs for a class or nation to perceive the role of management in production.

It is frequently assumed that the most urgent social need facing a governing elite in a poor country is to alleviate the poverty and suffering of its people, especially of its peasants. But this assumption underestimates the capacity of rulers everywhere to tolerate misery in their subjects, a capacity which the whole course of history demonstrates. Sensitivity in rulers may lead to expressions of sympathy for the poor, and to symbolic acts of charity, but hardly to the neglect of all other values in the interest of development. We must reflect that the measures necessary for development attack poverty only indirectly and show results not immediately but after a period of years. Mere sympathy with the poor is more likely to show itself in the distribution of alms, either in old-fashioned style or as social security measures and other such immediate alleviation of misery, which is often quite the opposite of a long-term policy that will lead to development—as seen in nineteenth-century England, where people were urged not to give alms so that able-bodied beggars would be forced to seek factory work.

Thus while poverty is always in some sense a challenge to government, it is not one to which a positive response at the level of effective action is inevitable. That poverty which arises from the high density and rapid growth of population would seem to be as tolerable as any other. What sort of challenge can find a response in development? Presumably one that somehow enmeshes the self-interest of powerful groups or individuals. Thus a military threat from the outside world was accepted as a challenge to Japan's existence by samurai bureaucrats after the Meiji restoration.

If poor, aging peasants can be left to misery or even starvation without serious political consequences, the case is different with the young generation whose numerical importance in the coming years is revealed by the census figures. The literacy, mobility, and incipient organization of these young people make them politically important, capable of exercising a pressure for goods which government will find it hard to deny.

POPULATION AS AN OBSTACLE AND A CHALLENGE

The relation of population growth to economic development has been studied extensively and deeply. Coale and Hoover, in what is the classic work on the subject,[3] show the very different consequences for Indian development of different rates of population growth, these rates operating through their effect on the process of capital accumulation and in other ways. Any increase in income which a country struggles to attain must be rapid so that population growth may be outdistanced and reversion to stability avoided, as Leibenstein has shown.[4] But the somewhat opposite possibility that population growth might provide a healthy challenge, or otherwise awaken people to reality, is not entirely absent from the literature. Hirschman develops this point, citing a series of suggestions from Malthus (the incentive effect of having to provide for one's wife and children), from Schumpeter (that population may have an energizing effect), and from other writers. He suggests that "the possibility of a strong reaction is greater if the population increase comes as a sudden shock."[5] The present writer is less optimistic about the chances that population pressure in general will arouse the necessary action toward development, largely because it can only lower per capita incomes at the rate of 3 per cent or so per year in the country as a whole. This is

[3] A. J. Coale and E. M. Hoover, *Population Growth and Economic Development in Low-Income Countries* (Princeton, N.J.: Princeton University Press, 1955).

[4] Harvey Leibenstein, *Economic Backwardness and Economic Growth* (New York: John Wiley & Sons, 1957).

[5] A. O. Hirschman, *The Strategy of Economic Development* (New Haven, Conn.: Yale University Press, 1956), pp. 176–83.

too gradual for people to have a point at which to take a stand. If there are any circumstances in which population can offer a shock treatment, they should be in the situation where a critical age group—that of the entrants into the labor force—doubles in numbers within a five-year period.

YOUTH AND NATION-BUILDING

The significance of the advent of a host of young people in Indonesia arises not only from their numbers but from the circumstances of political, economic, and social life. First among these is the nation-building effort of the authorities. Nations are very different social groups from those governed by tradition. To establish a nation is not merely to legitimate the indigenous culture as against that imposed by a now-rejected colonial power. In many of its aspects, indeed, the underdeveloped nation stands in even sharper opposition to the precolonial culture and society than it does to the colonial.

Nation-building is a frequent theme of Indonesian public speeches.[6] The set of ideas unfolded under this theme includes the importance of getting away from the variegated indigenous culture (except on such points as Islam, which can help unify a modern Republic); Indonesian nation-building is seen to depend on the Javanese ceasing to be Javanese, the Minangkebau ceasing to be Minangkebau. Even intermarriage among the several *sukubangsa* or cultures of which Indonesia is made up is counseled. All formal political discourse is in the neutral Indonesian language which is the new national speech, created out of antecedents in a traders' *lingua franca* throughout the archipelago and as far away as Madagascar. This language has the advantage for a modern state of dispensing with the inflections for indicating respect that are integral features of the more deeply rooted local languages; it is thus suited to be a means of communication among nominally equal citizens recently liberated from their ancient fixed societies. It is spreading with the very rapid spread of education at the primary level and upward, for school education stresses the national language at the expense of the local languages. This education includes a much larger measure than is usual in older countries of what we would call "civics," an inculcation of the themes which make up the official national belief system—invariably referred to as the Indonesian Revolution. Aside from the social homogenization which is represented by the playing-down of regional cultures and their social hierarchies, there is also a more visible homogenization in the military-style training which hundreds of thousands, perhaps millions, of volunteers for the enterprise of crushing Malaysia are now undergoing. Leaders are much in favor of physical fitness, of citizens taking daily exercise individually and in groups.

It is hardly necessary to stress how alien to the adult peasant are all these elements. The peasant can no more appreciate the importance of training in civics than he can that of doing physical jerks. Only his children of an age to have attended school after the mid-1940's can even understand the national language. He has no national preoccupations; in the expression commonly used in Indonesia today, he is wholly "feudal." To make him a national product is the aim of much intense effort, but one that can hardly succeed in any considerable measure for individuals already at middle age or beyond.

It is largely the young who are susceptible of being formed in the national mold through the educational and political system. It is the young who have the ambition to rise within the new social systems being formed of political parties, trade unions, army groups, in which the pressure of upward mobility is so strong. The old, at least those in the countryside, are mentally located within a hierarchical system, and

[6] For example, that given by General Nasution at the University of Indonesia in May, 1964.

materially they have some greater or lesser claim to the land. The claim may be only the privilege of sowing and weeding an area of half a hectare, for which they obtain one-quarter of the crop; but this is enough in general to keep them alive, and they are not easily alerted to the possibility of improving their position by leaving their claim and going elsewhere.

RURAL YOUTH AND PRESSURE ON THE LAND

If we remember that Java is about three-quarters rural; that the number of rural young people reaching age seventeen every year will rise from about 800,000 to about 1,600,000 during the next six years; that the attitudes of the young, determined by their education, must include impatience with the fragmentation of labor and of the crop which is the best that the Javanese village can offer them; and that even this kind of entrance into economic life will not be available to most of them since the land is already wholly taken up in most places, then it seems safe to predict that there will be movement to the cities. We reflect that the cities have been growing by over 200,000 persons per year during the period in which those reaching the age of entrance into economic life in rural Java numbered only 800,000 annually and the villages were less crowded than they have now come to be. It becomes plain that the cities will grow very fast indeed with the doubling of the numbers coming to labor-force-entry age, which is everywhere the age of greatest propensity to migrate.

A SYSTEM WHOSE OUTPUT IS SPIRITUAL RATHER THAN MATERIAL

What will the city offer the new immigrants? Is the activity of training for the invasion of west Irian of three years ago and for guerilla action in north Borneo today the ideal occupation? From some points of view it is; it puts the youth into green uniforms which well represent the cultural homogeneity so much desired by the authorities of the Republic; they learn the civic virtues now being inculcated as constituting the permanent Indonesian Revolution; they maintain a high degree of physical fitness. For Marx, men are made what they are by the relations of production into which they enter; for guided democracy they are shaped by military-style exercises, by mass rallies, by mass organizations, by the school system.

There is of course one difficulty with these last-named means: although they may be analogous to a productive system in making new men, as well as in the various kinds of input they use, they differ in having no material output. The spiritual output or satisfaction is not to be disregarded, but it is a complement to, rather than a substitute for, material goods. However much individuals may think when they are gathered on the drill ground that the satisfactions of the activity itself suffice for them, there will certainly be moments when they will individually observe the shortage of food, clothing, and other necessities.

A CHALLENGE BECOMES SUCH ONLY WHEN SUITABLY INTERPRETED

Whether this essentially unemployed younger generation is a difficulty or an opportunity for the republic will depend on how the increase in numbers is interpreted. For the first time since 1950 Indonesia will face a genuine problem—the demands of a new and self-conscious group, insistent in a way that an excess population consisting of illiterate peasants could never be. Attempts will certainly be made to interpret the problem as one of division of the product rather than of its insufficiency in total. On the other hand the pressure may be so great as to bring a moment of truth: that the republic's troubles are not to be solved by the rectification of unequal or unjust division of the national product, but by increasing it. Some few Indonesians have pointed this out, circumspectly and under the banner of guided democracy and its

need to crush Malaysia, but the attention of the public and the authorities is not yet effectively focused on it. A widespread recognition of the solution through production, both agricultural and industrial, would force recognition of the need for management and organization. It would lead to a selection of talent among the young away from political and toward economic organization. It could accentuate and unconditionally support the many constructive activities now going on throughout the country. But any historical prediction based on pressure and a challenge must face the fact that in human society a challenge becomes such only when appropriately interpreted. Whether the educated youth descending on the cities will impose a production-mindedness—not hitherto called for—depends on the interpretations and ideologies with which that youth is received. A first sign that the population crisis has forced the country into constructive action will be the initiation of energetic measures to bring down the birth rate.

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Some Measurements of Achievement Orientation[1]

Joseph A. Kahl

ABSTRACT

A series of interrelated studies of achievement orientation, stemming from theoretical work by Kluckhohn and Parsons, are compared. It is found that this orientation consists of at least four separate components: (1) "activism" or "mastery"; (2) "trust"; (3) "independence of family"; and (4) "occupational primacy" or "accomplishment." The first three are positively correlated with one another and with socioeconomic status; the fourth is negatively correlated with the others and with status. The findings are interpreted in the light of theoretical problems about achievement orientation, social mobility, and economic development.

Ever since Max Weber wrote *The Protestant Ethic and the Spirit of Capitalism,* the concept of achievement orientation has been important to sociologists concerned with the social-class system and to economists concerned with economic development. The sociologists usually state that the middle class is especially devoted to values stressing accomplishment, striving, and ambition to do better. Furthermore, it is thought that a small minority of the working class become committed to this value system as adolescents, and thereupon begin to behave in ways that are likely to lead to social mobility.

The economists are concerned with the achievement syndrome as crucial to the entrepreneurial behavior that leads a society from stagnation to growth. They have variously speculated that the small number of men who become successful entrepreneurs are likely to be Protestants, immigrants, or members of ethnic minorities whose overt values stress ambition, or to have had parents who behaved in ways that produced in them a high subconscious need for achievement.[2] Weber himself said that, once it is institutionalized, achievement can become a widespread value that needs no special explanation; he was interested in the "Protestant ethic" as a factor of importance at the very beginning of modern capitalism when the weight of cultural tradition inhibited economic innovation.

Despite its theoretical significance, achievement orientation has seldom been measured by standardized instruments. True, McClelland and associates have reported on an extensive series of studies dealing with the subconscious need for achievement, a motivation that was operationalized through the coding of a projective test composed of stories created in response to TAT cards.[3] Sociologists, however, are likely to prefer an instrument that taps conscious belief systems. Our notion of the concept "value" implies a verbal system, an official ideology that is deliberately taught to new members of given groups or strata.[4] The existing evidence is contra-

[1] The preparation of this paper was aided by NSF grant G24222 to Joseph A. Kahl via the Social Science Institute, and NSF grant G22296 to the Computer Center, both at Washington University. A critical reading by Professor Robert L. Hamblin was most helpful; John E. Lyman provided efficient statistical assistance.

[2] See, e g., the recent book by Everett E. Hagen, *On the Theory of Social Change* (Homewood, Ill.: Dorsey Press, 1962).

[3] David C. McClelland, *The Achieving Society* (New York: D. Van Nostrand, 1961).

[4] With reference to class values, see Talcott Parsons, "An Analytical Approach to the Theory of Social Stratification," *American Journal of So-*

dictory but seems to suggest that there is a positive but low correlation between measures of motivation and measures of value, and that they predict different aspects of achieving behavior.

The most direct way to measure value systems is through use of sets of attitude scales. Fortunately, there is one series of researches on verbal achievement orientation that are indirectly linked to one another and thus provide an unusual opportunity to compare related measures for different samples. Not all of the data have yet been published, but nevertheless it is possible to present the relevant comparisons here. They show that "achievement orientation" is a global concept, not a single dimension, and that its separable components do not necessarily vary in the same way among different social-class levels.

THE THEORETICAL STANCE

The particular theoretical position that stimulated the empirical work reported here was elaborated primarily by Florence Rockwood Kluckhohn, and secondarily by Talcott Parsons. In the early 1950's, some students of Kluckhohn and Parsons, and in turn, their students and associates, began to operationalize aspects of achievement orientation in connection with various researches on social stratification and mobility in Cambridge, Massachusetts, and New Haven, Connecticut. The concepts they used were not always precisely those of the formal schemes of Kluckhohn and Parsons, but were suggested by them. Later studies collected comparable data in St. Louis, Missouri, in Brazil, and in Mexico.

Earlier writers on values had tended to picture the world in terms of "ideal types," or clusters of characteristics that together described the beliefs of specified groups of people. These ranged from Weber's "Protestant ethic" through Ruth Benedict's "Apollonian" and "Dionysian" to the Wirth-Redfield contrast between "folk" and "urban" cultures. The logic of the ideal type stated that there was a limited number of relevant attributes, and when *all* of them fit together in a specified way they defined a type. Often (but not always) ideal types were bipolar, so that the same attributes in their opposite form defined the alternative type. For example, Redfield considered a culture that was sacred, collective, and organized to be "folk," and a culture that was secular, individualistic, and disorganized to be "urban." A combination of attributes from both poles—such as secular, collective, and organized—was a residual annoyance, a non-type.

More recent approaches divide the ideal type into its component attributes and conceive of them as varying independently of one another. Thus instead of a type with its polar opposite, these approaches describe a set of "profiles," each of which is defined by a particular combination of attributes.

For instance, Kluckhohn posits five basic questions and indicates that there can be three possible answers to each one. The three answers to a given question are not different positions on a single scale of increasing intensity but qualitatively different possibilities. Thus, if we were to consider only the preferred answer to each question, the various combinations would define 243 theoretical types (3 to the fifth power). Since she considers not only the preferred or dominant answers, but also the ranking of the second- and third-order alternatives, the number of theoretically (not empirically) possible types is even greater.[5]

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ciology, XLV (May, 1940), 841–62; Florence Rockwood Kluckhohn, "Dominant and Substitute Profiles of Cultural Orientations: Their Significance for Social Stratification," *Social Forces*, XXVIII (May, 1950), 376–93; and Joseph A. Kahl, *The American Class Structure* (New York: Rinehart & Co., 1957), chap. vii.

[5] Florence Rockwood Kluckhohn and Fred L. Strodtbeck, *Variations in Value Orientations* (Evanston, Ill.: Row, Peterson, 1961). Kluckhohn and her associates have evolved an ingenious series

To illustrate Kluckhohn's system, we can note that she pictures middle-class American culture as emphasizing these preferred choices: (1) human nature: mixture of good and evil; (2) man-nature: mastery over nature; (3) time: future; (4) activity: doing; (5) relations: individualistic.

Parsons' pattern variables also describe five attributes that can vary independently of one another; however, each is defined as a dichotomy, a forced choice, and thus only thirty-two possible types are defined by the system.[6] His view of middle-class culture emphasizes universalism, specificity, affective neutrality, self-orientation, and achievement.

Operationalizing a system with so many possible types is difficult; the mind gets confused, and the standard statistical devices do not readily serve. However, if one attempts to treat each component as a single variable, a characteristic that can range from none, through a little, to more, to a lot, then he has standard tools at hand. He need not cling to a series of many hypothesized "types," but instead can deal with a limited number of relevant variables. He can study their covariation; he can identify central tendencies, statistical (not ideal) types of characteristics that tend to cling together in patterns; and he can study interesting deviations from these central tendencies, either persons of one level of society (say, working class) who display values usually associated with another level (say, middle class), or persons who are high on the variable (say, individualism) and low on another (say, mastery), when those two variables usually have positive correlations.

The field studies reported below began with the theoretical view shared by Kluckhohn and Parsons that emphasizes the profile-like nature of values. They decided to measure several key components of achievement orientation: activism or mastery over the environment; planning for the future; individualism, even to the point where family ties are weakened; desire for occupational accomplishment; trust in people. They invented sets of attitude items to measure each dimension *separately* and treat it as a variable rather than an attribute; but then they often combined those items into a *single scale* of achievement orientation. The central point of the present review is to show that they obscured the facts by so doing, for achievement orientation is not a single dimension, and its components do not vary in the same way among different social strata. The point can be made by a chronological review of the field studies.

THE FIELD STUDIES

In 1953, Kahl and Davis interviewed 209 adult men in Cambridge, Massachusetts.[7] Included in the interview was a scale called "Occupational Primacy" which aimed at a measure of achievement values that put occupational success ahead of alternative possibilities (the full scale is presented in the Appendix). A later factor analysis of the data with the co-operation of Hamblin showed that the six items in the scale formed a single dimension. To the surprise of most persons associated with the research, it turned out that this scale was *negatively* associated with social-class rank; there was a correlation of —.44 with occupation, and —.56 with education.[8]

of questions to operationalize her concepts, but they are not directly comparable to the sets of items discussed in this article.

[6] Talcott Parsons, *The Social System* (Glencoe, Ill.: Free Press, 1951), chap. ii.

[7] Joseph A. Kahl and James A. Davis, "A Comparison of Indexes of Socio-economic Status," *American Sociological Review,* XX (June, 1955), 317–25.

[8] Joseph A. Kahl and Robert L. Hamblin, "Socioeconomic Status and Ideological Attitudes: A Nonlinear Pattern" (unpublished manuscript, 1961). The correlations reported in the text are product-moment coefficients. "Occupational Primacy" was scored as shown in the note to Table 1; occupation was scored by assigning a score of 1 to service workers and laborers, 2 to operatives, up to 7

Closer inspection revealed that the relationship was curvilinear when status was measured by occupation, but not when it was measured by education, as shown in Table 1.

Post hoc interpretation led to this attempt to explain the negative correlation:

Successful men probably take their success somewhat for granted and turn their attention to "the higher things of life." Automatically assuming that their sons will have good positions, the fathers advise the sons to learn to play the piano or enjoy tennis and not concentrate all their attention on preparation for their careers. This does not mean that upper-status men disapprove of business success; rather, it means they do not have to think about it all the time. But men who have achieved less success in a society which puts great emphasis on it are inclined to keep pushing, both for themselves and their sons.[9]

TABLE 1

OCCUPATIONAL PRIMACY IN CAMBRIDGE, BY OCCUPATION AND EDUCATION

	Mean Score on Occupational Primacy*	N
Occupational level:		
Professional and technical workers	1.64	39
Proprietors, managers, and officials	1.88	24
Sales workers	1 79	19
Clerical workers	2.33	15
Craftsmen and foremen	2.37	51
Operatives	2 19	36
Service workers and laborers	2.04	25
Total	2 00	209
Educational level:		
Post-college	1.19	31
College graduate	1.46	24
Part college or post-high school	2.20	35
High-school or trade-school graduate	2.24	54
Part high school or trade school	2.20	40
Grammar school only	2.50	25
Total	2.00	209

* The sample was divided into equal thirds on "Occupational Primacy"; those in the lowest third were assigned a score of 1, the middle third a score of 2, and the upper third a score of 3.

One might add that the curvilinear pattern (by occupation) can perhaps be interpreted as an expression of ambition in the middle of the hierarchy where success is feasible, of relaxed and secure acceptance of their position by those at the top,[10] and of apathetic withdrawal among those at the bottom who feel that striving will not lead to success and therefore is not worth the effort. The existence of a high score on occupational primacy for those in the middle of the occupational, but not the educational, hierarchy might be influenced by a tendency for men of low education who have high achievement values to climb up a bit through hard work and success on the job.

At about the same time as the Cambridge research, but independently of it, Strodtbeck was developing a "V-scale" to measure values associated with achievement among a thousand high-school boys in New Haven, Connecticut.[11] He found that eight items had predictive value in indicating those boys who achieved in school (as measured by their course grades) at a level higher than would be expected from their

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for professionals. Similarly, the educational ranks were scored from 1 for grammar school to 6 for post-college. Other correlations reported later in the text were computed in a parallel manner. The correlation between education and occupation in Cambridge, and also in Brazil and Mexico, was approximately .70.

[9] *Ibid.*, p. 8.

[10] See Charles McArthur, "Personality Differences between Middle and Upper Classes," *Journal of Abnormal and Social Psychology*, L (March, 1955), 247–55.

[11] Fred L. Strodtbeck, "Family Interaction, Values and Achievement," in David C. McClelland *et al., Talent and Society* (New York: D. Van Nostrand, 1958). Strodtbeck's article includes a cogent discussion of the relation of achievement values to classical theory in both sociology and psychology. His scale was useful to Carolina Martuscelli Bori in research on Brazillian mobility; see her chap. x in B. Hutchinson, *Mobilidade e Trabalho* (Rio de Janeiro, 1960).

scores on IQ and aptitude tests. To this degree, then, all eight items predicted actual achievement behavior. Yet, a later factor analysis of the items showed at least two separate dimensions (see Appendix): Factor I, called "Mastery," and Factor II, called "Independence of Family." The first emphasizes planning for a controllable future, and the second stresses loyalty to self and career instead of to parents. The low loading on the seventh item suggests that it is independent of the others, and perhaps belongs to a dimension of "Individualism," and the last item is also independent, but its meaning is not clear. Despite this indication of two or three dimensions, the items were treated as a single scale, for they all predicted course grades.

TABLE 2

ACHIEVEMENT VALUES OF FATHERS IN NEW HAVEN, BY OCCUPATION

Occupation	Mean Score on Strodtbeck V-Scale	N
Large businessmen and professionals	6.8	16
Small businessmen, clerks, and foremen	6.4	16
Skilled and unskilled workers	5.5	16
Total	6 0	48

Strodtbeck administered his instrument to forty-eight of the fathers of his high-school boys and found that there was a *positive* relation between social status and achievement orientation as measured by the V-scale. Here the results, shown in Table 2, may appear to contradict those from Cambridge. Yet the items in the scales are different, which suggests that the global concept of achievement orientation may have separate subdimensions, and that they do not relate in the same way to status level.

Rosen, who was associated with the Strodtbeck research, went on to do a replicating study.[12] He too used a sample of high-school boys in New Haven, and administered a scale that is a shortened version of the V-scale. It has three "Mastery" items, one "Independence of Family" item, and one new item. He found that for 120 boys, achievement values varied positively by social-class background (measured by father's occupation, education, and area of residence) (see Table 3). He also used the McClelland projective

TABLE 3

ACHIEVEMENT VALUES OF BOYS IN NEW HAVEN, BY SOCIAL CLASS OF FATHERS

Fathers' Social Class (Hollingshead Index)	Per Cent on Rosen Achievement Scale			N
	High	Low	Total	
I and II (high)	77	23	100	30
III	70	30	100	30
IV	33	67	100	30
V (low)	17	83	100	30
Total	50	50	100	120

measure of "need-Achievement," and found that it too was positively associated with social class. He further found that need-Achievement was predictive of school marks, whereas achievement values were predictive of aspiration toward college attendance—even when social-class background was controlled. (The correlation between achievement values and need-Achievement was not explored.)

In 1960, Kahl pursued the notion of the multidimensionality of value systems in a field study in Brazil, and three years later repeated the same instrument in Mexico.[13]

[12] Bernard C. Rosen, "The Achievement Syndrome: A Psychocultural Dimension of Social Stratification," *American Sociological Review*, XXI (April, 1956), 203–11 (also see his "Race, Ethnicity and the Achievement Syndrome," *ibid.*, XXIV [February, 1959], 47–61).

[13] Joseph A. Kahl, "Urbanização e Mudanças Ocupacionais no Brasil," *América Latina*, V (Outdez de 1962), 21–30; a monograph comparing Brazil and Mexico is being prepared for publication.

Both samples consisted of adult men, with over six hundred in Brazil and over seven hundred in Mexico. Persons living in the capital cities of their countries, as well as those from small towns in the interior, were included. Four scales relevant to the present discussion were used:

1. "Trust," with some items taken from earlier (and yet unpublished) work of Alvin W. Gouldner, and some new items.
2. "Activism," which contained two items borrowed from Strodtbeck's "Mastery," plus five others of similar content.
3. "Occupational Primacy," which contained four items out of the six used previously in Cambridge.
4. "Integration with Relatives"—the opposite of Strodtbeck's scale of "Independence of Family." One item was taken from him, and the other two are similar in content.

The scale of "Trust" was included because it was believed that the theme of mastery over life chances depends upon an ancillary belief in the stability of life and the trustworthiness of people. Perhaps, as Strodtbeck has suggested, this concept is the modern equivalent of Calvin's transcendental God who controls nature and man through immutable law rather than miraculous intervention. Certainly if one lacks faith in human relationships, he will find it harder to plan carefully for his future through orderly commitment to successive steps in career advancement.

As indicated in the Appendix, the scales used in Brazil and Mexico were almost the same, and can be taken to be measures of identical dimensions. In a few instances, a given item was used in one country but not the other, or an item had a loading that fell below .35 in one of the countries and thus was not used in the computation of the scale score, but these minor differences should not affect the comparability of the scales.

A series of factor analyses of the Brazilian and Mexican data showed that each of these four scales is a separable dimension of belief, despite the fact that three of them (all except "Occupational Primacy") are significantly associated with one another; technically speaking, they are oblique axes that can be identified out of a general factor.

When each of the four scales was correlated with an index of socioeconomic status (based on occupation, education, and self-identification), the coefficients were as follows:

	Brazil	Mexico
Trust	.30	.26
Activism	.42	.49
Occupational primacy	−.20	−.09
Integration with relatives	−.30	−.46

We must reverse the sign of the last of the four scales in order to make it comparable to the measure of "Independence from Family" used in New Haven. With that adjustment, all the scales behave about as expected: "Trust," "Activism," and "Independence from Family" are positively related to socioeconomic status, and "Occupational Primacy" is either negatively related or shows such a low correlation as to be unimportant.

In Tables 4 and 5, the mean scores for each scale are given for separate strata of occupation and education. They show one major departure from linearity: in Brazil, as in Cambridge, foremen have the highest scores on "Occupational Primacy." (There is also a smaller departure from linearity: in both countries, low-level white-collar workers show less independence from their families than do high-level white-collar workers.)

Finally, we report the St. Louis data collected by Cox in 1961 from a sample of 234 mothers of children attending nursery schools.[14] Both Negroes and whites were

[14] Henrietta Cox, "Study of Social Class Variations in Value Orientations in Selected Areas of Mother-Child Behavior" (unpublished doctoral dissertation, Washington University, St. Louis, 1964).

TABLE 4

VALUES IN BRAZIL, BY OCCUPATION AND EDUCATION

	Mean Scores*				N
	Trust	Activism	Occupational Primacy	Integration with Relatives	
Occupation:					
High white-collar	2.7	3.1	1.7	2.0	72
Low white-collar	2.8	2.9	1.9	2.3	202
Foremen	2.1	2.4	2.5	2.0	37
Skilled workers	2.2	2.3	2.1	2.7	152
Semi- and unskilled workers	2.1	2.0	2.1	2.9	164
Total	2.4	2.5	2.0	2.5	627
Education:					
Post-secondary	3.1	3.0	1.7	2.1	50
Secondary graduate	2.8	3.1	1.8	2.2	105
Part secondary	2.6	2.8	2.0	2.3	108
Primary graduate	2.3	2.4	2.1	2.6	168
Part primary	2.1	2.0	2.2	2.8	196
Total	2.4	2.5	2.0	2.5	627

* The sample was divided into equal thirds on "Occupational Primacy"; those in the lowest third were assigned a score of 1 the middle third a score of 2, and the upper third a score of 3. The other scales were approximations to quartile scores.

TABLE 5

VALUES IN MEXICO, BY OCCUPATION AND EDUCATION

	Mean Scores				N
	Trust	Activism	Occupational Primacy	Integration with Relatives	
Occupation:					
High white-collar	3.0	3.4	1.9	1.9	100
Low white-collar	2.7	2.8	2.0	2.3	188
Foremen	2.4	2.6	2.0	2.1	60
Skilled workers	2.3	2.2	2.0	2.8	238
Semi- and unskilled workers	2.2	1.9	2.2	3.0	154
Total	2.5	2.5	2.0	2.5	740
Education:					
Post-secondary	3.0	3.7	1.9	1.9	108
Secondary graduate	2.7	3.0	2.2	2.4	64
Part secondary	2.6	2.9	2.0	2.1	160
Primary graduate	2.3	2.3	2.1	2.5	159
Part primary	2.3	1.9	2.1	3.2	249
Total	2.5	2.5	2.0	2.5	740

included. Her items were phrased as values that should be taught by mothers to children—thus, their format was different from the earlier scales which referred to the personal beliefs of the respondents. She dealt with the deliberate teaching of values, whereas the earlier investigators dealt with internalized values. This difference heightens our interest in Cox's results, for they indicate that some variation in the form of items is not sufficient to disturb the expected pattern of response.

TABLE 6

VALUES IN ST. LOUIS, BY SOCIAL CLASS*

Social Class, Hollingshead Index	Mean Scores			N
	Trust	Activism	Accomplishment	
All women:				
II (high)	3.09	3.07	2.00	57
III and IV	2 69	2.73	2.40	101
V (low)	1.89	1.87	3.09	76
Total	2.53	2.53	2.53	234
White women:				
II	3.17	3.10	1.80	41
III and IV	2.87	2.60	2.26	53
V	2.11	2.05	2.92	37
Negro women:				
II	2.63	3.25	2.88	8
III and IV	2.29	2.91	2.54	35
V	1.69	1.69	3.26	35

* Scale scores represent approximations to quartile scores. Twenty-five women did not indicate their race on the questionnaire.

Cox's scale called "Accomplishment" parallels Kahl's "Occupational Primacy." Her scales of "Activism" and "Trust" are similar to those of the same names used in Brazil and Mexico. Factor analysis indicated that all the items could be considered as reflecting a general underlying dimension (they all had significant loadings on a principle axis factor), but that each of the three scales could readily be separated out as an oblique subdimension. (Cox's results showed a closer, albeit negative, relation between "Accomplishment" and the other two scales than did Kahl's results in Latin America.)

In St. Louis, the respondents were given socioeconomic status scores based on the occupation and education of their husbands (or themselves, if presently unmarried). Each of the value scales was correlated with status, providing these coefficients:

Trust	.48
Activism	.41
Accomplishment	—.37

Once again, the correlations fit our expectations, for two of the scales were positively associated with status, whereas one of them, that which most openly expressed the desire to be a "winner," was negatively associated with status.

All of these relationships were linear for the total sample and for white women taken separately. However, among Negro women, "Accomplishment" was lowest in the middle-status group. This particular deviation from linearity is not the same as those observed in the earlier studies; the data are shown in Table 6.

DISCUSSION

Each of the field studies reported here had its own purpose, and accordingly devised its own measurements. Samples included high-school boys, adult men, and adult women. Yet the studies all came from the same theoretical stance, and their results proved to be comparable and parallel. Simple attitude items were devised to measure abstract values about certain social relationships and behaviors connected with achievement orientation, and they produced scales that "worked" in two senses: the same items clung together to define given scales, despite the distances of time, geography, and language that divided the field studies; and the correlations of the scales with an outside variable, socioeconomic status, were stable.[15]

[15] Unfortunately, most of the items in these scales (all of which follow a Likert format) are phrased in such a way that a person of low socioeconomic

These results add weight to the theoretical guidelines which stimulated the field work. They show the possibilities of cross-cultural research using standardized measurements. And they strengthen Inkeles' argument that "men's environment, as expressed in the institutional patterns they adopt or have introduced to them, shapes their experience, and through this their perceptions, attitudes and values, in standardized ways which are manifest from country to country, despite the countervailing randomizing influence of traditional cultural patterns."[16]

Finally, these field studies should make it clear that, if we are interested in understanding a fundamental value system like achievement orientation, we must conceive of it as containing more than one component. If that had not been done, the interesting subdimensions would not have appeared—subdimensions that were intercorrelated, but only moderately (the positive coefficients of correlation between "Trust," "Activism," and "Independence of Family" ranged from about .20 to about .55). More importantly, the negative relation between "Occupational Primacy" or "Accomplishment" and the other scales would not have been discovered (the coefficients ranged from —.07 to —.22 in Brazil and Mexico, and from —.27 to —.43 in St. Louis). This negative relation contradicted expectations, since all the scales were originally conceived as positive components of achievement orientation. Let us explore the meaning of this negative relationship.

Apparently open and frank avowal of success, "determination and driving ambition," to use the phrasing of one of the items, is most characteristic of a person in the lower-middle section of the status hierarchy, especially one who has achieved some occupational success as a skilled worker or foreman, but did not have much educational preparation. It is the expressed attitude of a "striver."

Those with more education strive less openly and vigorously, but they have other values that in fact aid them in reaching or maintaining a level of occupational success higher than that obtained by the "striver." They trust in people and are thus able to develop long-term relationships that aid their careers, particularly in bureaucratic structures where the judgments of peers are so crucial. They believe that planning for the future is possible and fruitful; thus not only people but "destiny," can be relied upon. They are willing to move away from their parents in order to accept career advancement, and in general put efficiency ahead of nepotism. These values are more subtle than open striving, yet are more directly connected with success.

A recent study by Mizruchi adds weight to this interpretation.[17] Reporting on a sample of slightly more than two hundred persons from a small city in upstate New York, he says that most respondents told him that it was important to them "to get ahead in life," but that it was somewhat more important to persons in the working class than to those in the middle class. Then he asked them to indicate the symbols of success that they had in mind when they talked about getting ahead. The middle

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status would, according to the correlations reported in the text, tend to agree. Now, it may be that persons of low status are "yea-sayers" to an unusual degree, and tend to agree with almost anything. If so, acquiescence set would inflate the reported correlations. However, the presence of a a few items phrased so that low-status persons tend to disagree, plus additional data not reported here from the researches of Kahl and Cox that used forced-choice rather than agree-disagree items, shows that the results are not spurious. For studies of acquiesence set as a personality variable, see Arthur Couch and Kenneth Kenniston, "Yeasayers and Naysayers," *Journal of Abnormal and Social Psychology*, LX (March, 1960), 151-74.

[16] Alex Inkeles, "Industrial Man," *American Journal of Sociology*, LXVI (July, 1960), 2.

[17] Ephraim Harold Mizruchi, *Success and Opportunity* (New York: Free Press of Glencoe, 1964), chap. iv. The cross-cultural data reported by Inkeles, *op. cit.*, corroborate this view.

class preferred education as their first choice, whereas the working class indicated home ownership. Furthermore, education was viewed by the middle class as a good thing in itself, whereas to the working class it was valued only as an instrumental means to a decent job. Many studies have shown, in addition, that when a working-class man stresses the importance of education he is likely to be thinking of a high-school diploma, whereas the middle-class person has a college degree in mind. Thus a general goal, be it "success" or "education," has different concrete meaning to persons at different status levels.

Mizruchi concludes that in a general way the "success" goal is widespread throughout the class hierarchy, but that the particular content of this goal and the means by which it can be obtained vary significantly. The lower classes are more concrete in their aspirations: they want a job that provides money, security, and a home, and they sense that these goals are problematic and must be strived for. Risk-taking is dangerous and should be avoided. Aspirations that call for very high levels of education and skill involve risk. Thus, one must strive hard for limited goals.

Mizruchi's data on the middle and upper classes suggest that they view success more in terms of the intrinsic qualities of a job and in terms of their general status in the community. They are willing to take some risks in order to achieve big results; they are concerned with the interesting nature of the task and the challenge to one's abilities. Even when judging success by extrinsic rewards, they are more inclined to be concerned about reputation in the community, and less about material things. I suggest that these preferences of the middle and upper classes are possible only because the basic minimum of material success is taken for granted, and therefore it is possible to concentrate on the other aspects of career.

If this view is correct, then the psychological dimensions of need-Achievement and intelligence must be used within a specified context. The standard measure of achievement motivation deliberately abstracts from the particular goals involved; it is an index of a generalized motivation to do well, to excel in a variety of tasks. But success in the socioeconomic structure depends upon a desire and an ability to do well only in certain clearly defined spheres which center on work and on the personal relationships associated with the work role. One's motivation to write exquisite poetry or to express one's manliness through sexual conquest or physical aggression is either irrelevant or possibly even harmful. Members of a traditional rural upper class often emphasize poetry, and members of a lower class often emphasize manliness. Neither set of goals is likely to lead to business leadership, though both may involve very high need-Achievement.

Consequently, the appropriate theoretical framework seems to be this: first, goals must be defined in terms of group norms, and the means of reaching those goals must be defined in terms of the realities of the social structure. Then men who share given goals can be sorted into different categories according to their level of achievement motivation, and also their level of intelligence, in order to predict their success in meeting the defined goals. In general, it will take less motivation (and perhaps less intelligence) to maintain an inherited status than to climb to a higher one, for all sorts of subtle advantages accrue to the fellow who is already there.[18]

Insofar as goals and means differ at different levels of the class hierarchy, and also at different epochs in the process of

[18] For a study of achievement motivation that follows this approach see Harry J. Crockett, Jr., "The Achievement Motive and Differential Occupational Mobility," *American Sociological Review*, XXVII (April, 1962), 191–204. For a study of intelligence see C. Arnold Anderson *et al.*, "Intelligence and Occupational Mobility," *Journal of Political Economy*, LX (June, 1952), 218–39. I am still wating for a study that combines both intelligence and motivation within the context of social structure.

economic development, it seems naïve to predict either individual success or national economic development solely from abstract achievement motivation. But it is equally naïve to try to predict mobility within a given system without first controlling for the level of the system we are concerned with, and then showing how some individuals somehow learn values and display motivations and abilities that are sufficiently atypical to lead them toward another status.

Achievement orientation on the level of values is not a single dimension. We must deal with its components, and then tie them to the relevant psychological dimensions. Studies that depend on any one of these variables without reference to the others do not control enough variance in predicting behavior to contribute much to a theory of succession and mobility in the stratification system, to say nothing of a theory of entrepreneurship and economic progress, that will be both elegant and practical.

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ST. LOUIS

APPENDIX

SCALE STEMS WITH FACTOR LOADINGS

A. OCCUPATIONAL PRIMACY (Kahl, J. A. Davis; Cambridge)

+.74	The most important thing for a parent to do is to help his children get further ahead in the world than he did.
+.71	The most important qualities of a real man are determination and driving ambition.
+.68	The most important purpose of the public schools is to prepare people for occupational success.
+.57	The best way to judge a man is by his success in his occupation.
+.51	The job should come first, even if it means sacrificing time from recreation.
—.39	Americans put too much stress on occupational success.

B. V-SCALE (Strodtbeck; New Haven)

Factor I	Factor II	
—.64	.00	Planning only makes a person unhappy since your plans hardly ever work out anyhow.
—.49	—.28	When a man is born, the success he's going to have is already in the cards, so he might as well accept it and not fight against it.
—.58	—.15	Nowadays, with world conditions the way they are, the wise person lives for today and lets tomorrow take care of itself.
—.04	—.60	Even when teen-agers get married, their main loyalty still belongs to their fathers and mothers.
—.21	—.60	When the time comes for a boy to take a job, he should stay near his parents, even if it means giving up a good job opportunity.
—.29	—.68	Nothing in life is worth the sacrifice of moving away from your parents.
+.02	—.28	The best kind of job to have is one where you are part of an organization all working together even if you don't get individual credit.
+.05	.00	It's silly for a teen-ager to put money into a car when the money could be used to get started in business or for an education.

C. ACHIEVEMENT VALUES (Rosen; New Haven. All items scored negatively; factor weights unavailable.)

... All I want out of life in the way of a

career is a secure, not too difficult job, with enough pay to afford a nice car and eventually a home of my own.

... When a man is born the success he is going to have is already in the cards, so he might just as well accept it and not fight against it.

. . Planning only makes a person unhappy since your plans hardly ever work out anyway.

Nowadays, with world conditions the way they are, the wise person lives for today and lets tomorrow take care of itself.

. Nothing in life is worth the sacrifice of moving away from your parents.

D. Trust (Kahl; Mexico and Brazil)

Mexico	*Brazil*	
—.66	—.78	It is not good to let your relatives know everything about your life, for they might take advantage of you.
—.71	—.74	It is not good to let your friends know everything about your life, for they might take advantage of you.
—.67	—.55	Most people will repay your kindness with ingratitude.
+.38		Most people are fair and do not try to get away with something.
—.62		People help persons who have helped them not so much because it is right but because it is good business.
—.40		You can only trust people whom you know well.

E. Activism (Kahl; Mexico and Brazil)

—.63	—.74	Making plans only brings unhappiness because the plans are hard to fulfil.
—.58	—.65	It doesn't make much difference if the people elect one or another candidate for nothing will change.
—.67	—.63	With things as they are today, an intelligent person ought to think only about the present, without worrying about what is going to happen tomorrow.
—.54	—.57	We Brazilians (Mexicans) dream big dreams, but in reality we are inefficient with modern industry.
—.61	—.47	The secret of happiness is not expecting too much out of life, and being content with what comes your way.
+.46		It is important to make plans for one's life and not just accept what comes.
+.41		How important is it to know clearly in advance your plans for the future? (*Very important* is coded positively.)

F. Occupational Primacy (Kahl; Mexico and Brazil)

+.59	+.69	The job should come first, even if it means sacrificing time from recreation.
+.64	+.59	The best way to judge a man is by his success in his occupation.
+.80	+.62	The most important qualities of a real man are determination and driving ambition.
	+.46	The most important thing for a parent to do is to help his children get further ahead in the world than he did.

G. Integration with Relatives (Kahl; Mexico and Brazil)

+.73	+.76	When looking for a job, a person ought to find a position in a place located near his parents, even if that means losing a good opportunity elsewhere.
+.78	+.75	When you are in trouble, only a relative can be

depended upon to help you out.

+.65 +.64 If you have the chance to hire an assistant in your work, it is always better to hire a relative than a stranger.

H. ACTIVISM (Cox; St. Louis)

—.70 Children should learn that planning only makes a person unhappy since your plans hardly ever work out anyway.

—.80 Children should learn early there isn't much you can do about the way things are going to turn out in life.

—.66 Nowadays the wise parent will teach the child to live for today and let tomorrow take care of itself.

—.77 Children should be taught not to expect too much out of life so they won't be disappointed.

—.67 Children should be taught that when a man is born the success he's going to have is already in the cards, so he might as well accept it and not fight against it.

I. ACCOMPLISHMENT (Cox; St. Louis)

+.66 Above all, parents ought to try to help their children get further ahead than they were able to get.

+.67 A child should be taught from infancy to take the greatest pride in doing things well.

+.74 Children ought to learn to try hard to come out on top in games and sports.

+.56 A mother ought to teach her child to try to do everything he does better than anyone else.

+.47 Children should be taught that the job comes first, even if it means giving up most of the fun.

J. TRUST (Cox; St. Louis)

—.86 Children should be taught that in these days a person really doesn't know whom he can count on.

—.72 Children should learn that if you don't look out for yourself people will take advantage of you.

+.72 Children should learn that most people can be trusted.

Note: Except for the V-scale, all factor weights are based on a principle-axis analysis of only the items contained in the scale.

Occupational Segregation in a Metropolitan School System[1]

Albert Lewis Rhodes, Albert J. Reiss, Jr., and Otis Dudley Duncan

ABSTRACT

The relationship between socioeconomic levels of close friends, the extent to which this relationship may be a consequence of socioeconomic segregation of schools, and the extent to which school segregation is a consequence of residential segregation in the larger community are examined for the Nashville, Tennessee, Standard Metropolitan Statistical Area male population in grades 7–12. The fact that boys tend to choose their best friends from their own occupational level is shown to be compatible with a theory of urban structure in which a combination of economic and social factors is said to segregate people into areas homogeneous in selected socioeconomic characteristics. Nevertheless, the amount of segregation by occupation in the white community and its secondary schools is not great enough to preclude a relatively high degree of accessibility between persons at different occupational levels.

The continuing interest of the sociologist in the relationship between the spatial pattern and the moral order of the city[2] is reflected in recent studies on the effects of school climate. Wilson, Coleman, and others have studied how educational aspiration and performance relate to the socioeconomic composition of schools.[3] Wilson concludes that "The *de facto* segregation brought about by concentrations of social classes in cities results in schools with unequal moral climates which likewise affect the motivation of the child, not necessarily by inculcating a sense of inferiority, but rather by creating a different ethos in which to perceive values."[4] Recent studies of schools in large urban communities call attention to schools in which students are drawn from extremes of the social-class system.[5] These studies deal with the consequences of segregation, and the authors purposely select particular schools so as to emphasize the effects of *de facto* socioeconomic segregation. Although the results are instructive, this procedure does not lend itself to measuring the *amount* of class segregation in the total school system of a city or the *magnitude* of the effects of class segregation.

Two assumptions are explicit in the Wilson study and implicit in other, similar studies: (1) Attitudes and values are transmitted to the individual by his peer group; and (2) the socioeconomic-status composition of peer groups within a school reflects the socioeconomic-status composition of the whole school. It would seem that these assumptions are generally involved

[1] The data for schools were gathered under Project 507, U.S. Office of Education. Albert Lewis Rhodes held a Social Science Research Council Postdoctoral Fellowship at the University of Michigan when the present analysis was made. He takes responsibility for the calculations reported in this paper; Albert J. Reiss, Jr., was principal investigator for Project 507; and Otis Dudley Duncan worked on methodological problems encountered in the present analysis.

[2] Robert E. Park, "The City: Suggestions for the Investigation of Human Behavior in the Urban Environment," *American Journal of Sociology*, XX (1915), 581–87.

[3] Alan B. Wilson, "Class Segregation and Aspirations of Youth," *American Sociological Review* XXIV (1959), 836–45; James S. Coleman, *The Adolescent Society* (New York: Free Press of Glencoe, 1961); J. Richard Udry, "The Importance of Social Class in a Suburban School," *Journal of Educational Sociology*, XXXIII (1960), 307–10; Lewis Rhodes, "Anomia, Aspiration and Status," *Social Forces*, XLII (1964), 434–40.

[4] Wilson, *op. cit.*, p. 845.

[5] *Ibid.*, p. 837; Coleman, *op. cit.*, pp. 280–84; James Bryant Conant, *Slums and Suburbs* (New York: McGraw-Hill Book Co., 1961).

when authors conclude that attitudes and aspirations of students from diverse socioeconomic levels in a given school tend to reflect those of levels that are predominant in that school.[6] Given the sparse literature with divergent findings concerning socioeconomic level and peer-group structure in the metropolitan setting, it may be worthwhile to test the validity of the second assumption.[7]

That contemporary American cities are differentiated residentially according to racial, ethnic, and economic characteristics has been demonstrated in a number of studies.[8] Wilson and Udry suggest that the class composition of the urban school is determined by the composition of the district it serves.[9] At first glance, this connection seems so obvious that social scientists may have been led to overstate the case for *de facto* segregation in the schools. In the large city, it is quite possible to find individual schools with extreme overrepresentation of lower-class or upper-class students. However, students of *de facto* segregation have failed to describe how the class composition of the school varies with the class composition of the neighborhood throughout a whole metropolitan area. It would be helpful, therefore, to know how much correspondence there is between the pattern of school segregation and the pattern of residential segregation in the metropolitan community.

The present investigation is directed toward measuring three things: (1) the relationship between socioeconomic levels of close friends; (2) the extent to which this relationship may be a consequence of socioeconomic segregation in the schools; and (3) the extent to which school segregation is a consequence of residential segregation in the larger community.

DATA AND METHOD

Given the scope of this investigation, it is necessary to resort to some rather drastic simplifications in order to reduce the problem to manageable size. Ours is a case study of one metropolitan area. Occupation is used as the sole indicator of socioeconomic status. The sociometric data are limited to a small sample of white male pupils, and our measure of socioeconomic-status segregation by residence depends upon census tract data.

The Nashville, Tennessee, SMSA adolescent population attending grades 7–12 of the public and private schools in 1957 was the universe surveyed for the study. This definition of the universe includes about 95 per cent of all boys and girls aged twelve to fifteen in the metropolitan area, but only 81 per cent of the sixteen- and seventeen-year-olds.[10] It is impossible to estimate precisely the total loss of cases of adolescent age from the popula-

[6] Wilson, *op. cit.*, pp. 836–37, 844; Coleman, *op. cit.*, pp. 68–80.

[7] Udry, *op. cit.*, p. 309; Coleman, *op. cit.*, pp. 70–71. See H. Otto Dahlke, *Values in Culture and Classroom* (New York: Harper & Bros., 1958), pp. 330–51, and Norman E. Gronlund, *Sociometry in the Classroom* (New York: Harper & Bros., 1959), pp. 206–21, for reviews of earlier studies.

[8] Otis Dudley Duncan and Beverly Duncan, "Residential Distribution and Occupational Stratification," *American Journal of Sociology*, LX (March, 1955), 493–503; Arthur H. Wilkins, "The Residential Distribution of Occupational Groups in Eight Middle-sized Cities of the United States in 1950" (unpublished Ph.D. dissertation, University of Chicago, 1956); Otis Dudley Duncan and Beverly Duncan, *The Negro Population of Chicago: A Study of Residential Succession* (Chicago: University of Chicago Press, 1957); Otis Dudley Duncan and Stanley Lieberson, "Ethnic Segregation and Assimilation," *American Journal of Sociology*, LXIV (January, 1959), 364–74; Charles Tilly, "Occupational Rank and Grade of Residence in a Metropolis," *American Journal of Sociology*, LXVII (November, 1961), 232–330; Stanley Lieberson, "Suburbs and Ethnic Residential Patterns," *American Journal of Sociology*, LXVII (May, 1962), 673–81; Eugene S. Uyeki, "Residential Distribution and Stratification," *American Journal of Sociology*, LXIX (March, 1964), 491–98.

[9] Wilson, *op. cit.*, p. 837; Udry, *op. cit.*, p. 310.

[10] U.S. Bureau of the Census, *1960 U.S. Census of Population*, Vol. I, "Characteristics of the Population," Table 73 (Washington, D.C.: Government Printing Office, 1961).

tion due to such factors as excused absences, truancy, and non-response, as well as to drop out, but a rough guess puts it somewhere between 12 and 17 per cent of persons twelve to eighteen years of age. Given differential dropout rates, there are proportionately fewer of the expected males, and particularly Negro males.[11]

It is assumed that the socioeconomic status of a student is determined by the socioeconomic status of his family and that occupation of the head (main breadwinner) of the family is as good a single indicator of the family's socioeconomic status as any other. It will facilitate the presentation of results and the following discussion if we follow the practice of referring to the occupational level of the student's family as the student's "occupational level."

Information about the subject's occupational level was obtained by coding responses to four questionnaire items on parent's work and/or the occupation of parent or guardian listed on the student's cumulative record into census occupation categories.[12] Father's or stepfather's occupation was coded for 88 per cent of the cases and occupations for parent substitutes (grandparent, etc.) were coded for the remaining cases if occupational information was available. For lack of a better expedient, "occupation not reported" is sometimes treated simply as an additional occupation category.

In the analysis for schools, occupations were grouped according to major census classifications with the following exceptions: Professional, technical and kindred workers were subdivided into two categories—"old and new professions," which include the traditional professions of law, medicine, religion, and the sciences, plus teachers, engineers, and accountants; and "quasi- and semi-professions," which include the remainder of the professions, including pharmacists, funeral directors, medical technicians, draftsmen, "engineers" with no college training (except that railroad engineers and building engineers are included in the appropriate categories), reporters, and so on. The category of managers, officials, and proprietors is subdivided into salaried and self-employed. Service workers were subdivided into "protective service workers" and "other service workers." The comparison of school segregation with residential segregation relies on census tract data for the latter.[13] It was not possible to subdivide the major occupational categories for these comparisons so the number of occupational categories is reduced from twelve to nine. In any case, the socioeconomic-status distribution for schools and for residential areas will usually differ even when the data are for the same geographic area, since the measures of occupation segregation for schools and residential areas are different. The measure for school segregation is based on the aggregation of occupation data for head of household for each child enrolled in school while that for residence is based on aggregated data for employed male workers of all ages.

Occupational levels of students were tabulated for grades 7 and 8 and/or grades 9–12 for each white or Negro school in the racially segregated secondary-school system (the secondary system was still legally segregated at the time of this study). There are thirty-seven white junior high schools, seventeen white senior high schools, twelve Negro junior high schools, and three Negro senior high schools. Two private senior high schools for white boys are also included in the study. Junior high

[11] Albert J. Reiss, Jr., and Lewis Rhodes, *A Sociopsychological Study of Conforming and Deviating Behavior among Adolescents* (Washington, D.C.: U.S. Office of Education Project 507, 1959), p. 13.

[12] U.S. Bureau of the Census, *Alphabetical Index of Occupations and Industries: 1950* (Washington, D.C.: Government Printing Office, 1950).

[13] U.S. Bureau of the Census, *U.S. Censuses of Population and Housing, 1960 Census Tracts,* Final Report PHC(1)-99 (Washington, D.C.: Government Printing Office, 1961).

schools are treated separately, since they represent smaller territorial units. Some schools in the system have grades 7–12, and separate tabulations were made for the junior and senior parts of these schools because the junior part serves students from a smaller area.[14] In the interest of brevity, the presentation of results and discussion are largely confined to data for junior high schools. Patterns for white junior and senior high schools are very similar. The correlation between the respective sets of indexes of dissimilarity, for example, is .94 (see Table 7).

Sociometric data were obtained from an approximately 2 per cent systematic sample of white boys ages eleven through sixteen in our population. Each of the 153 boys was asked to name his closest friend. If this closest friend was in school, he was interviewed. The original subject's occupational level is cross-tabulated with the occupation of closest friend.

No sociometric data are available for Negroes, and they are omitted from the main analysis. Some observations on school and residential segregation patterns of Negroes are, however, offered subsequently.

RESULTS

What is the relationship between socioeconomic levels of close friends? Previous studies of the homogeneity of adolescent peer groups have produced mixed results.[15] However, the relevant theory would lead us to expect friends to have similar status.[16]

Our data tend to support the theory, in that a boy in our sample is more likely to choose a friend whose family's occupational level is similar to his own level. Product-moment correlation coefficients are computed using two sets of scores for occupation groups, one set based on nine major census categories of occupation and the other set based on refinements of the census categories (Table 1). The correlations are .50 and .45. As in other research,[17] there is also a slight tendency for subjects to choose friends from an occupation category which is higher than their own. The mean socioeconomic index for choosers ($\bar{X}$) is 33.7; the mean index for their friends ($\bar{Y}$) is 35.1 using the major census categories, and the regression equation is $\hat{Y} = 18.9 + .48x$. If the refined categories are used, $\bar{X} = 34.5$, $\bar{Y} = 35.5$, and the regression equation is $\hat{Y} = 21.2 + .41X$. Neither of the mean differences, however, is as large as its standard error.

To what extent is the positive relationship between occupational levels of subjects and friends a consequence of socioeconomic segregation in the school system? Sociologists have often noted the relationship between spatial distance and social distance. Numerous studies report the relationship between residential propinquity and marriage, social participation, and so on.[18] Could segregation by occupation in

[14] Five white junior high schools include ninth-graders. These 808 ninth-graders are excluded from the tabulations in order to avoid the overlapping of school districts. It is unlikely that exclusion of 8 per cent of the total for senior high schools will markedly affect these findings.

[15] See n. 10; J. J. Gallagher, "Social Status of Children Related to Intelligence, Propinquity, and Social Perception," *Elementary School Journal*, LIX (1958), 225–31; Morton B. King, Jr., "Socio-economic Status and Sociometric Choice," *Social Forces*, XXXIX (1961), 199–206.

[16] Theodore M. Newcomb, "The Study of Consensus," and Leonard Broom, "Social Differentiation and Stratification," in Robert K. Merton, Leonard Broom, and Leonard Cottrell (eds.), *Sociology Today* (New York: Basic Books, 1959), pp. 277–92; Henry W. Riecken and George C. Homans, "Psychological Aspects of Social Structure," in Gardner Lindzey (ed.), *Handbook of Social Psychology*, II (Cambridge, Mass.: Addison-Wesley Publishing Co., 1954), 791–93; Bernard Barber, *Social Stratification* (New York: Harcourt, Brace & Co., 1957), p. 122.

[17] King, *op. cit.*

[18] A. C. Clarke, "An Examination of the Operation of Residential Propinquity as a Factor in Mate Selection," *American Sociological Review*, XVII (February, 1952), 17–22; William F. Whyte, *Street Corner Society* (Chicago: University of Chicago Press, 1937); Otis Dudley Duncan and J. W. Artis,

the schools serve to account for some of the observed homogeneity of occupational levels in adolescent friendship groups? If we assume that friendship choices are confined exclusively to schoolmates and there is differential accessibility (or availability) of schoolmates in various occupational levels, then it follows that indivdiuals are more likely to choose friends from occupational levels which are overrepresented in the school where the choosing takes place.

Actually, data for the sample of 153 white boys support the assumption that most of the interaction with peers occurs in the same school. Eighty-four per cent of the sample chose a friend from the same school. An additional 7 per cent chose a friend from another school in the same school district (e.g., a junior high school boy may have chosen a friend from a grade school or high school in the same district). Five per cent chose a friend from a school in an adjacent area (boys living on the boundary of a school district are

"Some Problems of Stratification Research," *Rural Sociology*, XVI (1951), 17–29; William H. Form, Joel Smith, Gregory P. Stone, and James Cowhig, "Approaches to the Delimitation of Sub-Areas," *American Sociological Review*, XIX (August, 1954), 437; Wendel Bell and Marion Boat, "Urban Neighborhoods and Informal Social Relations," *American Journal of Sociology*, LXII (January, 1957), 391–98; James H. Williams, "Close Friendship Relations of Housewives Residing in an Urban Community," *Social Forces*, XXXVI (May, 1958), 358–62; Rudolf Heberle, "The Normative Element in Neighborhood Relations," *Pacific Sociological Review*, III (Spring, 1960), 7; Albert K. Cohen and Harold M. Hodges, "Characteristics of the Lower Blue Collar-Class," *Social Problems*, X (Spring, 1963), 311–15.

TABLE 1

SUMMARY MEASURES OF OCCUPATIONAL SEGREGATION BY TRACTS, SCHOOLS, AND FRIENDSHIP PAIRS

Tracts, Schools, and Friendship Pairs	r_b	Product-Moment Correlation
Major occupation categories:*		
Tracts (white employed males)	.045	.19
Schools (white grades 7 and 8)	.045	.22
Friendship pairs (white boys)	....	.50
Refined Occupation Categories:*		
Schools (white grades 7 and 8)	.043	.24
Schools (white grades 9–12)	.040	.19
Friendship pairs (white boys)	....	.45

* Occupation categories were assigned scores according to the "socioeconomic index" computed by Otis Dudley Duncan, "Properties and Characteristics of the Socioeconomic Index," chap. vii, in Albert J. Reiss, Jr., *Occupations and Social Status* (New York: Free Press of Glencoe, 1962), pp. 155, 263–75. Scores for subdivisions of major groups are weighted averages of scores for detailed occupations, using as weights the frequencies of white employed males in detailed occupation categories reported in U.S. Bureau of the Census, *U.S. Census of Population, 1950*, Vol. IV: *Special Reports*, Part I, chap. b, "Occupational Characteristics" (Washington, D.C., Government Printing Office, 1956).

The SES scores for major occupation groups and subdivisions are:

Professional, technical and kindred workers	75
Old and new professions	(79)
Quasi- and semi-professions	(66)
Managers, officials and proprietors	57
Salaried	(66)
Self-employed	(48)
Sales workers	49
Clerical and kindred workers	45
Craftsmen, foremen and kindred workers	31
Operatives and kindred workers	18
Service workers	17
Protective service	*(29)*
Other service	*(14)*
Laborers	7
Occupation not reported	19

likely to choose across it—at this point neighborhood contact competes with school contact). Only 2 per cent (three boys) chose someone in a different area of the city.

To assess the effect of school segregation, we consider a model which assumes (1) that interaction occurs within school districts and (2) that no other selective factors are operating.

TABLE 2

FATHER'S OCCUPATION

School Group	Professional	White Collar	Self-employed	Manual	Total
A......	92	174	68	39	373
B......	39	138	90	140	407
C......	11	111	37	221	380
Total	142	423	195	400	1,160

a given occupational category will interact with another individual in each occupational category, given that both are in the same district with the chances of interaction based on the occupational distribution for the metropolitan area as a whole. For simplicity, the model assumes sampling with replacement, that is, a student might occasionally choose himself.

A compact example (Table 2) may serve to illustrate the procedure for determining the effect of school segregation. The data for this illustration are taken from the Wilson study.[19] Expected frequencies based on total occupational distribution are shown in Table 3. The expected frequencies based on occupational distributions within individual schools are obtained by computing the null model for each school and then summing expected frequencies for each combination of occupational categories over

TABLE 3

FATHER'S OCCUPATION

Father's Occupation	Professional	White Collar	Self-employed	Manual	Total
Professional......	17.38	51.78	23.87	48.97	142
White collar......	51.78	154 25	71.11	145.86	423
Self-employed....	23.87	71.11	32 78	67.24	195
Manual..........	48.97	145 86	67.24	137.93	400
Total........	142	423	195	400	1,160

Each school serves only a small fraction of the total area of the city unless it has some specialized function (e.g., the school for retarded students or the trade school). Given the fact of territorial organization of schools, are the boundaries of school districts arranged so that occupational levels represented in the schools are heterogeneous or homogeneous? The "yardstick" is the total distribution of occupations for the school system. If the distribution of occupations within school districts is known, then it is possible to compare the chances that an individual in

all schools. The calculation is facilitated by matrix multiplication on an electronic computer. Convert the occupation distributions for individual schools to proportions. Premultiply the matrix of proportions by the transpose of the matrix of frequencies. The sums of expected frequencies based on school segregation in this example are given in Table 4. The τ_b statistic of Goodman and Kruskal is one way to summarize the result.[20] It

[19] Wilson, *op. cit.*, Table 3, p. 839.

[20] Leo A. Goodman and William Kruskal, "Measures of Association for Cross Classifications," *Jour-*

involves a comparison of the proportion choosing a friend of the same occupation level as the chooser when the choice is at random from the school marginal distribution with the proportion obtained when the choice is at random from the total distribution for all schools. In terms of the procedure illustrated here, τ_b is the difference between the sum of the frequencies on the diagonal expected from the school distributions and the sum of diagonal frequencies expected from the total distribution, divided by N minus the latter sum. For Wilson's data with a threefold school classification and four occupation groups, τ_b is .075.

Only the relative proportion of like-occupation choices is considered in the calculation of τ_b. If one wants to look at the effect of segregation on the several possible combinations of distinct occupations, it is instructive to compute the ratios of the choice frequencies expected on the assumption of random choice within schools to the frequencies expected on the assumption of random choice from the entire school population. Here are the ratios for Wilson's data, which show not only that homogeneous pairs are disproportionately frequent, but also that such pairings as "professional" with "self-employed" are made more likely by the segregation pattern (Table 5).

Similarly computed ratios for the Nashville school and tract data appear in Table 6. It is easy to see that school segregation places some constraint on potential friendship choices. School segregation operates to increase somewhat the chances that an individual will interact with someone in the same occupation category: τ_b in Table 1 is .045 or .043, depending on which occupation classification is used. With some minor distortion, the pattern of ratios in Table 2 may be summarized by stating that segregation reduces potential interaction between two occupation categories in proportion to the "social distance" separating them. In the school data (and with one exception in the tract data) ratios exceeding unity occur not only on the diagonal, but for all combinations of two white-collar or two manual occupations, while ratios below unity occur for all combinations of a manual with a white-collar occupation. It is important to note that none of the ratios is zero—even though

TABLE 4

FATHER'S OCCUPATION

Father's Occupation	Professional	White Collar	Self-employed	Manual	Total
Professional	26.75	59.35	26.47	29.43	142
White collar	59.35	160.38	73.05	130.22	423
Self-employed	26.47	73.05	35.90	59.59	195
Manual	29.43	130.22	59.59	180.76	400
Total	142	423	195	400	1,160

TABLE 5

FATHER'S OCCUPATION

Father's Occupation	Professional	White Collar	Self-employed	Manual
Professional	1.54	1.15	1.11	0.60
White collar	....	1.04	1.03	0.89
Self-employed	....	....	1.10	0.89
Manual	....	....	....	1.31

nal of the American Statistical Association, XLIX (1954), 759–60.

there is segregation by occupation in the schools, we do find children from families of professional workers and laborers attending the same school. Hence these data show that, while segregation by occupation exists in this school system, schools are by no means completely homogeneous in occupational composition.

Having studied the pattern of school segregation, we are now in a position to see whether this pattern accounts for the relationship between occupational levels of subjects and their friends. We compare the correlation coefficients that would obtain if each individual chose a friend at random within his own school with the coefficients in actual sociometric data (see Table 1). The coefficients due to school segregation are only about one-half as large using either set of occupation categories. This comparison leads us to conclude that school segregation accounts for a significant part, but nevertheless only a part of the correspondence between occupational levels of friends. If one prefers the viewpoint on which τ_b is computed, we may reach the same conclusion in the following way. With random choices from the entire city, 15 per cent of the junior high school students would select a friend in the same refined occupation category as theirs; with random choice within schools, this figure would be 19 per cent. Actual sociometric data show 27 per cent of the choices to involve a friend with the same occupation level. School segregation

TABLE 6

RATIOS OF EXPECTED FREQUENCIES OF CHOICES AT RANDOM WITHIN SCHOOLS OR TRACTS TO EXPECTED FREQUENCIES OF CHOICES AT RANDOM FROM THE TOTAL OCCUPATION DISTRIBUTION, BY MAJOR OCCUPATION GROUP CLASSIFICATION OF MEMBERS OF THE FRIENDSHIP PAIR

Occupation Group	N	Professional and Technical	Managers, Officials, Proprietors	Sales Workers	Clerical Workers	Craftsmen, Foremen, etc.	Operatives, Kindred Workers	Service Workers	Laborers	Occupation Not Reported
Schools—white grades 7 and 8	7,818									
Professional and technical	674	2.02	1.64	1.53	1.05	0.72	0.58	0.68	0.51	0.85
Managers, officials, proprietors	838		1.51	1.39	1.02	0.81	0.71	0.82	0.72	0.84
Sales workers	797			1.40	1.11	0.83	0.73	0.79	0.68	0.91
Clerical workers	304				1.24	0.98	0.93	0.94	0.80	1.03
Craftsmen, foremen, etc.	1,862					1.10	1.12	1.08	1.15	1.04
Operatives, kindred workers	1,811						1.22	1 17	1.23	1.04
Service workers	253							1.37	1.20	0.94
Laborers	297								1.58	0.97
Occupation not reported	982									1.18
Tracts—employed white males	82,078									
Professional and technical	9,403	1.53	1.34	1.24	1.07	0.78	0.66	0.81	0.65	0.91
Managers, officials, proprietors	11,998		1.38	1.20	0.99	0.84	0.72	0.81	0.74	0.66
Sales workers	8,533			1.21	1.06	0.88	0.80	0.86	0.77	0.92
Clerical workers	7,347				1.13	0.98	0.94	0.94	0.85	1.02
Craftsmen, foremen, etc.	17,540					1.12	1.14	1.04	1.12	1.03
Operatives, kindred workers	15,723						1.29	1.12	1.25	1.04
Service workers	3,814							1.56	1.14	1.04
Laborers	4,316								1.62	1.09
Occupation not reported										1.37

accounts for (19 — 15) / (27 — 15) = 33 per cent of the sociometric bias.

Does the pattern of occupational segregation in the schools reflect the pattern of residential segregation by occupation in the larger community? Results of two different tests indicate that patterns of school and residential segregation by occupation are quite similar. Indexes of dissimilarity between each pair of occupations were computed from the school and tract data.[21] There is a very close relationship between the two sets of indexes (see Table 7). The patterns for both white junior and white senior high schools fit the patterns for white employed males equally well. The main difference is that the indexes for senior high schools are slightly lower (see negative intercept). There are more tracts than schools, so differences in the number of areal units as well as in the population included may account for the minor differences in the pattern. On the basis of this test, however, it seems reasonable to conclude that the patterns of indexes are highly similar. (The proportion of cases with occupation not reported is higher in the school data than in the tract data. The segregation of this category may slightly inflate the value of τ_b.

The τ_b statistic (Table 1) was about .04 for both the tract data and the junior high school data. The product-moment correlation based on tract data is .19, which is similar to the .22 correlation based on school data. The similarity of the two segregation patterns is also evident in the comparison of the two sets of ratios in Table 6. The residential segregation pattern clearly is reflected in—indeed, is somewhat magnified by—the school-segregation pattern (note the slopes somewhat over unity in the first two regressions in Table 7).

Does the pattern of occupational segregation in the schools reflect a pattern of segregation which is unique to the Nashville SMSA? Since the school pattern is similar to the residential pattern, it may be worthwhile to see if the residential pattern is peculiar to this particular metropolitan area. There are data on occupational segregation in eight other middle-sized cities in 1950.[22] Indexes of dissimilarity for these cities were computed on the basis of tract data for all employed males. The correlation between indexes of dissimilarity for all employed males in the Nashville SMSA and the average indexes for the eight cities is .97 (Table 7). The main difference is that the indexes are generally higher for Nashville than for the average of eight cities (notice the positive intercept in the regression equation), in conformity with Wilkins' observation that occupational segregation for all males was somewhat greater in the cities with large numbers of Negroes. The Nashville residential pattern clearly is not unique. While there is no specific basis for generalizing from the Nashville school data, we should expect similar findings in a city of comparable size where the schools, as in Nashville, are organized on a primarily territorial basis.

OCCUPATIONAL SEGREGATION IN THE NEGRO COMMUNITY

Negro and white patterns of residential segregation by occupation are somewhat different, as we might expect from the dissimilarity of their occupation distributions and the pattern of residential segregation by race.[23] The census-tract index of dissimilarity between all whites and all Negroes is .77 indicating the high degree of *de facto* racial segregation. In Nashville,

[21] Duncan and Duncan, "Residential Distribution . . . ," *op. cit.*, pp. 494–95.

[22] Wilkins, *op. cit.* The average indexes of dissimilarity for the eight medium-sized cities appear in Otis Dudley Duncan, "Population Distribution and Community Structure," *Cold Spring Harbor Symposia on Quantitative Biology*, XXII (1957), 361.

[23] See total occupational distributions for whites and non-whites in Tables 3 and 4 of U.S. Bureau of the Census, Final Report PHC(1)-99, pp. 38–47.

as elsewhere, Negroes are largely confined to the low-prestige and low-paying jobs.[24] Four out of nine white employed males have white-collar jobs; two out of nine are craftsmen, foremen, etc. Only one out of seven Negro males has a white-collar job; only one in ten is in the craftsman-foreman category. The correlation between the white and non-white patterns of indexes of dissimilarity by census tracts is only .50 (see line 8, Table 7).

Although the residential pattern of occupational segregation for Negroes does bear some resemblance to that of whites, this pattern is not preserved in the occupational segregation of Negroes within their separate pattern of Nashville Negroes, for that pattern correlates .63 with the one observed in Chicago[25] (Table 7, line 7), and the indexes of dissimilarity between occupations generally run higher for Nashville than for Chicago.

Residential segregation by occupation, therefore, was not reflected in school segregation by occupation in the Negro system as it was organized in 1957. Yet we can indicate a significant feature of the association between class and color which turns up in both the residential and the school data. In the 1960 employed labor force of the Nashville area, 16 per cent of the male workers were Negroes. Negroes were underrepresented in both white-collar and higher manual occupations, overrepresented in lower manual occupations.

Now, let us imagine a sociometric process in which each white chooses a friend at random from the white occupation distribution of the entire metropolitan area and each Negro similarly chooses at random from the Negro occupation distribution. The result, shown in the second panel of Table 8, would be that less than 2 per cent of all pairs in which both members

TABLE 7

REGRESSION EQUATIONS AND CORRELATION COEFFICIENTS RELATING SELECTED SETS OF INDEXES OF DISSIMILARITY

Dependent Variable Y	Intercept a	Regression Coefficient b	Independent Variable X	Correlation Coefficient r
White schools:				
Grades 7 and 8	− 1.00	1.11	White employed males (tracts)	+.96
Grades 9–12	− 5 65	1.15	White employed males (tracts)	+.97
Grades 9–12	− 2.36	.97	White schools: grades 7 and 8	+.94
All employed males, Nashville SMSA	4.92	.99	All employed males, 8-city average	+.97
Negro schools:				
Grades 7 and 8	16.42	− .10	Negro employed males (tracts)	−.19
Grades 9–12	16.60	− .09	Negro employed males (tracts)	−.14
Negro employed males, Nashville SMSA	6.05	1 11	Negro employed males, Chicago	+.63
Negro employed males, Nashville SMSA	14.14	.38	White employed males, Nashville	+.50

NOTE: Each regression is based on twenty-eight observations, one for each pair of the eight census major occupation groups, omitting "occupation not reported."

[24] L. D. Zeidberg and Paul H. Geisel, *1960 Population Handbook, Nashville Standard Metropolitan Statistical Area* (Nashville: Census Tract Committee, 1963), pp. 32, 35, 97. See also Karl E. Taeuber, "Negro Residential Segregation, 1940–1960: Changing Trends in the Large Cities of the United States" (paper read at the 1962 meetings of the American Sociological Association, Washington, D.C.).

[25] Duncan and Duncan, *The Negro Population of Chicago*, p. 295.

are white-collar workers are Negro, while nearly one-half of all homogeneous lower-manual pairings would be Negro. Quite apart from any tendency of actual choices to be socioeconomically homogeneous, the marginal distributions themselves tend to produce a school system. In Table 9 one can detect a certain resemblance between the Negro and the white segregation patterns by tracts. (The tract data for "Negroes" actually are limited to figures for non-whites in census tracts with four hundred or more non-white inhabitants.) The Negro school data, however, show little trace of occupational segregation. The explanation no doubt lies in the peculiar territorial organization of Negro schools: Fifty-one per cent of all Negro seventh-

TABLE 8

PERCENTAGE OF NEGROES BY OCCUPATION AND BY OCCUPATION CLASSIFICATION OF SOCIOMETRIC PAIRS FORMED ON TWO HYPOTHESES ABOUT INTERACTION PATTERNS, FOR EMPLOYED MALES AND STUDENTS IN GRADES 7 AND 8

Occupation Group	Employed Males			Seventh- and Eighth-Grade Students		
	White Collar	Craftsmen, Foremen	Other Manual*	White Collar	Craftsmen, Foremen	Other Manual*
Total occupation distribution....	5.2	7.5	28.3	8.2	15.4	27.0
Friendship pairs formed at random from total distribution:						
White collar..................	1.7	2.5	11.1	3.6	6.8	11.8
Craftsmen, foremen...........	....	3.6	15.5	....	12.7	22 1
Other manual*...............	....	....	47.4	....	....	38.8
Pairs formed at random within tracts or schools:						
White collar..................	1.7	2.9	13.0	2.5	8.2	15.6
Craftsmen, foremen...........	....	3.6	14.0	....	12.3	20 7
Other manual*...............	....	....	43.0	....	....	34.5

* Includes "occupation not reported."

TABLE 9

RATIOS OF EXPECTED FREQUENCIES OF CHOICES AT RANDOM WITHIN SCHOOLS OR TRACTS TO EXPECTED FREQUENCIES OF CHOICES AT RANDOM FROM THE TOTAL OCCUPATION DISTRIBUTION, BY BROAD OCCUPATION GROUP, BY RACE

Occupation Group	Race and Occupation Group					
	White			Negro		
	White Collar	Craftsmen, Foreman	Other Manual*	White Collar	Craftsmen, Foremen	Other Manual*
Schools—grades 7 and 8:						
White collar..................	1.44	0.81	0.76	1.07	1.02	0.98
Craftsmen, foremen...........	....	1.10	1.09	....	1.06	0.98
Other manual*...............	....	....	1.13	....	....	1.01
Tracts—employed males:						
White collar..................	1.21	0.86	0.80	1.24	1.01	0.95
Craftsmen, foremen	....	1.12	1.11	....	1.11	0.98
Other manual*...............	....	....	1.21	....	....	1.01

* Includes "occupation not reported."

and eighth-graders go to one school in this district and 49 per cent of all Negro pupils in grades 9–12 attend one Negro high school in this district. Another school serves one-fourth of the seventh- and eighth-graders. The rest of the junior high school pupils attend ten smaller schools, five of which are one-room schools in the urban fringe. The other half of the senior high school pupils are evenly divided between two schools, one in the city system and one which serves all pupils outside the central city. This arrangement of school districts all but eliminates segregation by occupation within the Negro system.[26] Thus the correlations in Table 3 between indexes of dissimlarity based on the school data and the residential data for Negroes are actually negative, though very low in absolute value. It seems unlikely that this result is due to a peculiarity of the residential highly disproportionate number of Negroes among lower-class peer groups. Much the same result obtains for schools. Moreover, if we require the random choices to be within tracts or within schools the picture is only slightly altered (bottom panel of Table 8).

DISCUSSION

When the boys in our sample were asked to name their best friend, they tended to choose boys from their own occupational level. There are several ways of interpreting the fact. A social psychologist might say that it fits the general theory that people prefer to interact with others like themselves.[27] However, the fact is also compatible with a theory of urban structure: In the metropolis, a combination of economic and social factors operates to sort people into areas which are homogeneous with regard to certain socioeconomic characteristics. If the schools of a city are territorially organized, they will reflect this segregation, and one may, therefore, attribute the observed homogeneity of occupational levels to the pattern of residential and school segregation. One might argue that, if working-class boys are more likely to choose each other, this is largely a consequence of living in a neighborhood and going to a school where there are many working-class boys and few middle-class boys.

Our data show that there is socioeconomic segregation in the white secondary-school system. This secondary-school system reflects the pattern of residential segregation for whites in the metropolitan community. The similarity between occupational levels of friends is, however, rather greater than that predicted from the school-segregation pattern. Therefore, it seems likely that segregation by occupation in the junior and senior high schools contributes to, but does not fully account for, this relationship.

Perhaps the most important finding is that the amount of segregation by occupation in the white community and its secondary schools is not great enough to preclude a relatively high degree of accessibility between persons at different occupational levels. The data do not support those who imply that secondary-school systems are made up solely of "blackboard jungles" and "country-club schools."[28] Neither do the data support the generalization from small-town studies that the individual high school mirrors the status structure of the total community.[29]

The city investigated for this study is in a border state, so the percentage of Negroes (19.1 per cent) is predictably high. There is a relatively high degree of

[26] Tau_b is .01 for both junior and senior high schools regardless of whether one uses nine or twelve occupational categories.

[27] Riecken and Homans, *op. cit.*, p. 791.

[28] Conant, *op. cit.*

[29] August B. Hollingshead, *Elmtown's Youth* (New York: John Wiley & Sons, 1949), pp. 163–68; Robert J. Havighurst, Paul H. Bowman, Gordon P. Liddle, Charles V. Mathews, and James V. Pierce, *Growing Up in River City* (New York: John Wiley & Sons, 1962), pp. 9–15, 172.

residential segregation by race and the secondary-school system is racially segregated. (The grade-a-year plan for desegregation does not yet affect the high schools.) It is probably unnecessary to note that racial segregation, in contrast to occupational segregation, determines the racial composition of adolescent peer groups. Not one of the white boys named a Negro as best friend. Even though the white boys were asked to name additional friends, none of them named Negroes.

There are constraints on choices of occupation and residence for adult Negroes which result in a high degree of segregation by occupation within the Negro community. In the secondary schools, however, the plan for Negro school districts serves to virtually eliminate the segregation by occupation that exists for adults.

It is obvious that there is a relationship between the degree of occupational segregation in secondary schools and city size. Consider two extreme types: The first type is the secondary school that reflects the occupational composition of the total community, and the second type is the one-class school. It is easily apparent that all single-secondary-school communities have schools of the first type, for example, "Elmtown," "Millburg," and "Executive Heights."[30] It is important to remember, however, that even though small communities can't afford more than one secondary school, there is no perfect relationship between the population of a community and the number of secondary schools. A medium-sized city may be served by only one or two senior high schools (e.g., Little Rock, Arkansas).

We suspect that if the second type of secondary school exists, it exists only in the very largest of our central cities, small suburbs, or under exceptional circumstances. We fail to observe a one-occupation secondary school in this SMSA (although the one-room, urban-fringe school for Negroes approaches this ideal type). It seems unlikely that one-class secondary schools can be very prevalent in metropolitan areas, given the continuous processes of invasion, succession, and urban renewal in American cities and trends in secondary-school financing and organization.

These remarks should be sufficient to emphasize the need for further studies to assess the effects on school composition of city size, region, racial composition, occupational composition, and amount of residential segregation by occupation and by race, both *de jure* and *de facto*.

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[30] Coleman, *op. cit.*, pp. ix, 58–68.

An Illustration of the Use of Analytical Theory in Sociology

The Application of the Economic Theory of Choice to Non-economic Variables[1]

Eugene V. Schneider and Sherman Krupp

ABSTRACT

Recent developments in the philosophy of science have made it increasingly clear that scientific inquiry may employ various formal languages. Sociology, in the main, is committed to the language of functionalism, but other languages might be used. Furthermore, there are certain inherent limitations in the functional approach which confine analysis to a small number of possible states of systems. Analytical theories, taking as a basic axiom an incremental ranking system for all concepts, would permit discussion of ranges of values of variables and of varying relationships between them, and thus allow for the treatment of different types of systems and of various states of one system. The use of analytic theory in sociology is illustrated by the application of the economic theory of choice to a system of non-economic variables.

I

The aim of this paper is to illustrate the applicability of a deductive-analytic mode of reasoning to sociology. This framework, commonly used in the natural sciences and in economics, requires that a few variables be related in such a way that a change in one variable or its surrounding conditions will have predictable consequences for the other variables. For this to be accomplished, the variables must be assumed to be continuous over some designated range and their relations must be expressible in the simple laws of interdependence. We propose to adapt a deductive technique of this kind, worked out and highly developed in abstract economic reasoning, to sociological variables.

In sociological thinking the main deductive-analytical language is that of functionalism. The fact that functionalism is one particular language has been obscured by the circumstance that today almost all systematic sociological thinking is functional thinking.[2] In the light of recent developments in the philosophy of science, which have made it increasingly clear that scientific inquiry may employ various formal language systems, there is no inherent reason why other modes of scientific explanation cannot be usefully employed in sociological inquiry.[3]

Functionalism has undoubtedly proven fruitful in sociological inquiry. But functionalism, like any formal language, has certain inherent limitations. Since functional theory fails to incorporate a systematic incremental ranking system for each of the variables, it is difficult to dis-

[1] Revision of a paper read at the annual meeting of the American Sociological Association, August, 1964.

[2] See, e.g., Kingsley Davis, "The Myth of Functional Analysis as a Special Method in Sociology and Anthropology," *American Sociological Review*, XXIV (December, 1959), 757–72. Davis, in fact, argues that functional analysis has become identical with sociological inquiry.

[3] See Ernest Nagel, *The Structure of Science, Problems in the Logic of Scientific Explanation* (London: Routledge & K. Paul, 1961); Gilbert Ryle, *Dilemmas* (New York: Cambridge University Press, 1954); Norwood R. Hanson, *Patterns of Discovery* (New York: Cambridge University Press, 1958); Abraham Kaplan, *The Conduct of Inquiry Methodology for Behavioral Science* (San Francisco: Chandler Publishing Co., 1964), pp. 327 ff.

tinguish (1) a given range of a particular variable, (2) a particular value of a variable within its specified range and (3) the limits within which a given set of relationships between variables will hold. These characteristics of functional theory lead to certain difficulties in social analysis. In particular, functional theory has tended to deal with properties or attributes of aggregates without recognizing that the aggregates are composed of units that may be ordered independently. Consequently, the attempt to predict an outcome from any given change in conditions is limited to a description of gross changes in the properties of the aggregate. For example, it is possible to describe a social system as a structure of status, as a system of coercion and cohesion, as an administrative hierarchy, as a structure of authority, etc. However, the functional framework does not lend itself to the discussion of social systems in terms of possible levels of status, coercion, or authority. As a result, it does not permit discrimination among the levels of status, let us say, that result from changes in the values of the other variables. It is also extremely difficult to make any but gross comparisons of various states within a system or between systems.

Finally, functional analysis has treated variables such as status, coercion, cohesion and hierarchy mainly as they contribute to the goals of a system. This has constrained analysis to a narrow sector of possible relationships and has resulted in preoccupation with the "system-maintaining" solution of social and organizational behavior.[4]

A more general theory would permit a discussion not only of the structural conditions of a system but of the range and value that the variables will assume within this structure as well. It need not assume that the variables are essentially either functional or dysfunctional in their relationship to the system. By allowing for changes in the structural conditions and by locating the behavioral solutions which are produced through interrelationships among the variables, a wider variety of social behavior may be dealt with systematically. This allows the analysis not only of different social systems, but what is probably even more significant, of different states of a given system. Thus it should be possible to avoid reification; that is, it is not possible, using this language, to identify any one particular state of the system as the only state of the system.

But equally important, an analytical system permits a wide range of implications to be drawn from relatively few postulates. In a deductive framework, large classes of particulars can be deduced from relatively small sets of logically related variables and structural conditions. This enables us to proceed from the simple to the complex through a chain of deductive reasoning. An analytical theory may thereby organize seemingly disparate observations and categories by subsuming them under a common set of primitive concepts.

II

Theories with the above characteristics are common in the biological and physical sciences; they are less common in the social sciences. But economics provides some striking examples. In order to demonstrate the possibility of applying analytical theory to sociology, we shall make use of the economic theory of choice.[5] The eco-

[4] For a discussion of the limits of functionalism relative to other types of equilibrating systems see Sherman Krupp, *Pattern in Organizational Analysis* (Philadelphia: Chilton Co., 1961), pp. 32–52.

[5] The classical presentation of the "indifference technique" will be found in John R. Hicks, *Value and Capital* (Oxford: Clarendon Press, 1939). Subsequently, this method has been explored exhaustively. A typical treatment will be found in Erich Schneider, *Pricing and Equilibrium* (New York: MacMillan Co., 1962). In a recent book Kenneth Boulding has applied indifference analysis to the problems of conflict in a non-economic framework (see *Conflict and Defense: A General Theory* [New York: Harper & Row, 1962], chap. i).

nomic theory of choice, it must be stressed, is one among many possible analytical approaches. This theory is selected because of its relatively high state of development in economics.

The theory of choice treats the problem of goal fulfilment in relation to limited resources and the necessity of allocating these resources among competing ends. It introduces a ranking system as its basic axiom. The logic of maximization of economic variables has been thoroughly treated within this framework.

The analytical power of such an approach may be demonstrated by an analysis of non-economic variables in organizational behavior. Let us consider authority and conflict reduction to be two important organizational goals. An analysis of this kind requires that authority and conflict be considered capable of varying within some designated range. This permits the location of authority and conflict on an ordinal scale; that is, authority and conflict may be ranked in terms of "more" or "less."

Let us define authority as the ability of individuals or groups occupying statuses to control the actions of other individuals and groups who have varying potentialities for compliance or resistance. This definition implies that, given a set of external conditions, A and B have control over each other to varying degrees. It follows that a relative increase in the authority of A implies a relative decrease in the authority of B. This definition enables us to discuss different levels of authority that can be reached as the levels of resistance change. Authority may be likened to a scarce resource, available in principle to all members of an organization, but whose distribution will be determined by the conditions and forces that operate in any given situation.

Conflict may be expected to occur where the control of A over B is not well structured, that is, in the region where authority is imperfectly defined. An example is provided by the recent displacement of areas traditionally classified as "management prerogatives" into the area of negotiations. Any attempt to alter the distribution of authority (i.e., shift the limit of conventional control) will change the level of conflict. Under a specified set of conditions, a particular level of conflict would be an outcome of attempts by both A and B to determine in their own favor the value that the variables will assume in the disputed region.

The means available for goal achievement are limited; they constitute a fund or budget to be made available for the achievement of ends. That is, management or labor has only limited expendable resources of time, efforts, techniques, or money to be allocated for all organizational goals. Given this resource constraint, the achievement of one set of goals requires the relative neglect of other goals.

The amount of resources that must be expended will be determined by the level of resistance to the attainment of these goals. By resistance we mean the collection of obstacles that must be overcome in order to attain goals. A given level of resistance may be identified by a particular set of obstacles. A change in the level of resistance means either a change in the intensity or number of obstacles in that set, or the introduction of obstacles of a different kind.

It should be noted that we are not defining "resistance" as a simple psychological state of compliance or non-compliance. The obstacles that determine the level of B's resistance to A include the physical, biological, and psychological limitations in B which A must overcome if he is to achieve any given level of authority relative to B. Thus, the debilitation following a long and costly strike may cause labor to place a higher premium on peace than before the strike, although it retains the same distaste for managerial authority. A change in the ethnic or racial composition of a plant work force may shift

the level of compliance to managerial authority. Improved channels of communication may permit management to increase its level of authority without increasing its expenditure of resources.

It is possible to analyze relationships between the goals of authority and peace, the resources that are available for this purpose, the resource expenditure required to overcome resistances, and the

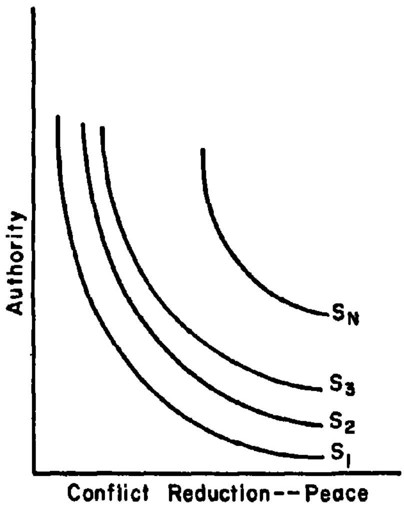


FIG. 1

levels of conflict reduction and authority that are attainable under these constraints. These relationships may be expressed diagrammatically.[6]

Suppose management desires both authority and peace, and these are substitutable goals. These relative preferences of management may be expressed as a family of curves convex to the origin. That is, each curve represents combinations of peace and authority which are equally acceptable to management. Along each curve authority may be traded off for a reduction in conflict; by accepting less peace, authority may be increased. A point on any one curve represents a state of equal preference with any other point on that curve for varying combinations of peace and authority. That is, each point represents the same amount of satisfaction but varying combinations of goal realization. In Figure 1, S_1, S_2, S_3 . . . , S_n represent a family of preference curves, ranked in terms of ascending order of goal achievement. Any point on S_3 represents a higher level of goal satisfaction than any point on S_1 or S_2. Any two points on the same curve represent equal amounts of goal satisfaction but with different combinations of peace and authority. Authority is decreased as we move down the curve and

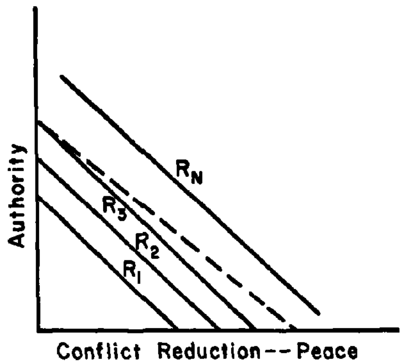


FIG. 2

out along the abscissa, but each diminution in authority is compensated by an increasing amount of conflict reduction. Management will attempt to reach the highest curve possible.

However, the highest realizable curve that management can attain, given a state of resistances, is determined by the available resources. This resource constraint will take the form of a straight line when each additional unit of peace or authority requires the expenditure of equal increments of resources, the highest line R_3 representing the largest total expenditure of re-

[6] An early example of the use of diagrammatically expressed relationships, which resembles our own treatment, will be found in Walter Firey, "Informal Organization and the Theory of Schism," *American Sociological Review*, XIII (February, 1948), 15–24.

sources (see Fig. 2). If there is a change in the state of resistances to the achievement of a goal, then the amount of resources that must be expended will also change. This will be represented on the diagram by a change in the slope of the curve representing the resource constraint (see dotted line, Fig. 2). Ordinarily, the resource constraint curve will not be a straight line (see Fig. 3, R_1, R_2). A straight line implies that the increase of peace or authority attained by the expenditure of resources is proportional to that expenditure. In reality, given a state of resistances, each additional degree of authority may require disproportionate resource expenditure. That is, if management wants more authority, it is attainable only by an increasing rate of effort expended. In this case, the resource constraint curve may be concave to the origin. As in the case of the straight line, a change in the slope of this curve reflects a change in the state of resistances.

The highest degree of goal attainment consistent with a resource constraint is represented by the point of tangency between the curve expressing this constraint and the curve expressing the substitutability of goals. (see. Fig. 3). Assume now that the resources available to management are represented by a curve designated by R. Any point above the point of tangency with S_1, such as G, is not attainable given the resources available; any point below means that management is not attaining the goals it could attain with the same resources. The point of tangency, T_1, is the point where management would act to achieve its ends if it adopted the most effective strategy in the use of its resources.

III

Various system behaviors may be explained within this framework as the outcome of conditions and forces that operate in any particular context. Changes in constraints—in management preferences, resource availabilities, or resistance to goal attainment—will result in different behavioral outcomes.

Consider a change in the relative preference of management for authority and peace. This would be represented by a new family of goal-substitution curves. If the resource and resistance constraints remain the same, there will now be new points of tangency. For example, a change in the climate of opinion prevalent in England after World War II seems to have resulted

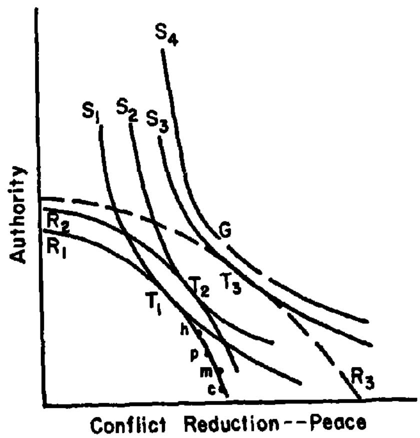


FIG. 3

in a reduction of the relative preference of management for authority.[7] At the same time there has been no apparent change in management's distaste for conflict. Management's relative preference for these goals would now be represented by new families of curves and the points of tangency would shift along the resource constraint curve.

It is also possible to rank management strategies in terms of their effectiveness in goal fulfilment. Let us assume that we can delineate alternative management strategies which stress: (1) human relations

[7] See J. A. Banks, *Industrial Participation* (Liverpool: Liverpool University Press, 1963).

(k), or (2) paternalism (p), or (3) money inducements (m), or (4) coercion (c) (see Fig. 3). If all resources are expended on one strategy, the level of goal attainment would be expressed as a point on the resource constraint curve. Different strategies may be ranked in terms of their distance from the point of tangency. Even though it may not be possible for management to adopt the optimal strategy, there may still be a system of ranking among those that are available. A change in management's preferences may result in a reordering of the relative effectiveness of the various strategies.

Now suppose there is a change in the resources available to management, that is, a change in the total possible effort that management can apply toward the attainment of authority and conflict reduction so that resource availability shifts from R_1 to R_2. For example, should this involve a larger money expenditure management may be able to increase the closeness of supervision in order to attain authority or raise the salaries of disaffected workers to reduce conflict. If the new resource is in the form of a more energetic, driving management, it is possible to increase the closeness of supervision and at the same time to reduce conflict by taking into account the workers' psychological and social needs. *In either case there will be a new solution involving a higher fulfilment of* management goals. This may be represented on the diagram by a movement from a point on S_1 to a point on S_2 (T_2).

New management behavior will shift the levels of "resistance-compliance" of the individuals and groups that management is trying to control. In Figure 3 a change in resistance is represented by a shift from R_2 to R_3, and the new point of tangency is T_3. The state of the compliance to management's drive for peace and authority will determine the level of goals that management can achieve, given a level of resources. Thus a reduction in the militancy of labor leadership in the society might result in new and more favorable solutions for management because it would decrease resistance to the attainment of management authority. Analogously the patriotic belief that labor conflict is incompatible with national unity might increase the importance of industrial peace to the worker, altering the solutions which management can attain with any given application of resources.

IV

Just as organization can be viewed as a context within which management seeks to attain its goals, limited by certain constraints, so is it possible to discuss the achievement of the goals of peace and authority in terms of labor's peace-authority preferences, their resource constraint, and management's resistance or compliance to the attainment of these goals. Thus labor will have a desire to control the actions of management (e.g., a desire for control over the work rules) as well as a desire to reduce conflict (e.g., a desire for more satisfying social relations). The attainment of labor's goals will also be limited by its resources (e.g., the state of its organization, the degree of worker militancy, the capacity to sacrifice wages). Finally, there are the resistance or compliance variables (e.g., the state of management's ability to achieve its goals, the costs of operating a union, the status labor may lose in a community by being militant).

V

The limits which labor and management impose upon each other in the attainment of their respective goals can be analyzed within a common framework.[8] In Figure 4, the desire for authority and peace on the part of labor is expressed by the S_L curves, while management's goals are represented by the S_M curves. S_{L1} represents a state of goal attainment that is the minimum labor

[8] Schneider, *op. cit.*, pp. 299–313.

is willing to accept in a given situation. S_{M1} expresses the minimum acceptable goal attainment for management in the same situation. Areas I and II represent goal achievement regions which might exist in other contexts, but do not apply to this situation. Both labor and management are assumed to have resources which will enable them to achieve at least the minimum, S_{L1} or S_{M1}. The area between curves—the shaded area in Figure 4—represents the region of all possible compromises between labor and management. It is the range within which bargaining will occur between both parties. The given compromise, which is a bargained solution to a situation of conflict, will be at a point in the shaded area. The particular compromise will be determined by the relative bargaining power of labor and management. Relative bargaining power, in turn, is determined by a variety of behavioral factors external to this context such as negotiating skill, public opinion, and market situations.

If we conjecturally extend both management and labor curves into the shaded area, a set of points of tangency can be traced, represented by the line D. It might seem that one of these points would represent an optimal solution for both parties and therefore would be the "best" compromise. But this is not the case. Either party may hope to gain by moving to another point along D, and this can only occur at the other's expense. Therefore, any point on D is a compromise which can be considered inferior from one party's point of view but not from the point of view of both. Suppose, in contrast, that the point of compromise is some point off D, such as Z; then the position of one party or both may be simultaneously improved by a movement from Z to a position on D. That is, there

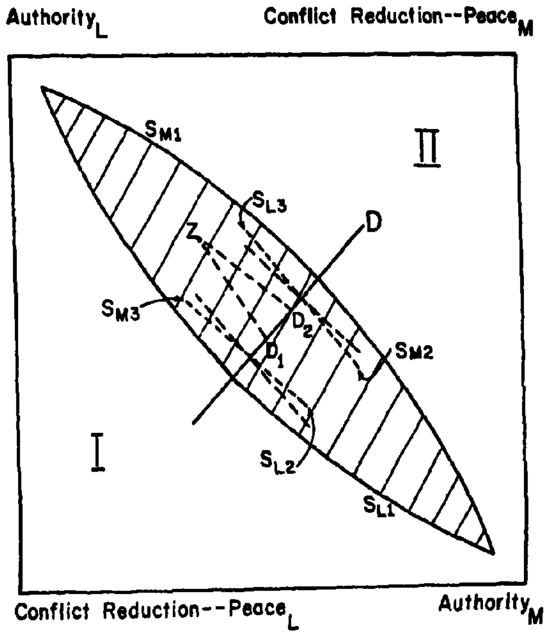


FIG. 4

is a point on D such as D_1 which represents a point of satisfaction for labor equal to Z, but which at the same time represents an improvement in management's position, or point D_2 at which management is equally satisfied as at Z but at which labor's position is improved. Any point between D_1 and D_2 improves the position of both management and labor relative to Z. Consequently, there is always a point on D which will yield a preferred position to at least one of the parties without loss to the other. In short, a point on D is optimal relative to Z, but not optimal relative to any other point on D.

These interrelationships enable us to show the areas where conflict and harmony may occur in the organizational context. If a solution has been achieved at Z which is off the curve designated by D, there is a situation where management or labor or both may gain without loss to the other. This is the context within which the "human relations school" can be effective. The further Z is from D, the more effective the human relations program may be; but as Z approaches D this effectiveness decreases. The ability to move along D, in contrast, will depend on the power that either side can exert on the other. If human relations programs, for example, are applied on D, they can only benefit one side at the expense of the other.

One may speculate about the possibility that structural conditions (i.e., constraints) change as a result of internal relationships. For example, if the power of management or labor is great enough to achieve solutions at one extremity or another of the bargaining situation, then the structure of the situation may become precarious. In this situation there are various possible outcomes, of which three may be mentioned: (1) If solutions occur near the extremities but mechanisms exist which propel the solutions toward the center, then the general structure of the situation will be preserved. Many theories of stable social systems depict this state of affairs (e.g., the Parsonian functional theory). (2) If solutions cluster at either extremity over time, forcing some modifications in the structural conditions but providing sufficient time for the structural conditions to reorder themselves, then a new relatively stable situation may emerge (e.g., Merton's revision of functionalism). (3) If solutions achieved at an extremity create rapid change in the structural conditions which in turn lead to new but unstable internal situations, the result may be total disruption. This is basic to the revolutionary stage of Marxian theory.

The system may also change as a result of external conditions. Goals, resources, and resistances may be altered by forces located outside the system (e.g., a change in public opinion, legislation, political regimes, race relations). These changes in external contexts will change the structural conditions and thereby modify the internal conditions of the organization. Change in external conditions may be compensated for, may create large changes, or may totally disrupt the system.

VI

This analysis (or other analyses of a similar kind) seems to provide one way to overcome some of the weaknesses of traditional sociological theory. All the concepts with which we have dealt have been defined in such a way as to allow for variation in their values. Thus it has been possible to avoid reification; it is not possible, using this language, to identify any one particular state of the system as *the* necessary state of the system. Instead, our method makes it mandatory to speak of different kinds of systems, different states of systems within some context, and the effect of contextual changes on these states.

Furthermore, our method allows for an analysis of change in social systems. In the system we have outlined, for example, changes may be introduced at one or several points: in goal achievement, resource level, or resistance level, or in any com-

bination of these. The result will be a shift in the system to a new level. Changes which occur either as the result of an internal process or as the result of external factors can be treated by this method. Finally, the formal language that we have employed enables us to discuss the limits within which behavioral solutions or outcomes will occur. These solutions do not involve such teleological elements as "system-maintenance"; they are the outcome of the relations of external constraints and endogenous forces affecting behavior.

By using this kind of formal language system, we have necessarily had to modify certain assumptions that sociologists often make about their subject matter. Traditional sociology frequently assumes that its categories do not conform to an incremental ranking system. However, we should like to point out that this is true only insofar as sociology confines itself to "lumpy" type-part concepts and considers them as collections. For example, institutions, groups, roles, etc., are usually treated in this way. As we have attempted to show in this paper, there is no logical reason for not employing another kind of language which takes an incremental ranking system as a basic axiom for its concepts. The choice of a language, we would argue, should not be based on some supposed inherent attributes of the subject matter, but on the over-all utility that a given formal language can be shown to possess.

The practical utility of the formal language we have been employing in this paper, as opposed to its logical practicability, depends on our correctness in assuming that concepts such as authority, conflict reduction, and resource allocation in fact can be considered in terms of "more or less." Here we may call upon the common experience that there are degrees of authority, that a situation may be more or less peaceful, that people possess greater or less resources.

There is also the possibiilty we can make use of the experience in scaling and measurement that has accumulated through the work of such people as Guttman and Lazarsfeld. Whether we assume an underlying continuum of which the visible points are the observed data, or whether we assume that the data themselves may be arranged in ordinal scales, the basic conditions for incremental ranking are being met.[9] To the extent this is true, there is the possibility of operationalizing the system we have been dealing with, though perhaps crudely. Considering the power that analytical systems in various fields have been shown to possess, it would seem desirable for sociology to experiment with such language systems, even before more refined instruments of scaling are developed.

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[9] For a discussion of theoretical problems in scaling, see C. West Churchman and Philburn Ratoosh, *Measurement: Definitions and Theories* (New York: John Wiley & Sons, 1959).

Local-Cosmopolitan: Unidimensional or Multidimensional?[1]

INTRODUCTION

Since Merton's study of community influentials,[2] the terms "locals" and "cosmopolitans" and related hyphenated neologisms such as "professionals-organizationals," "specialists-institutionalists," and "science oriented-company oriented" have been used by sociologists to characterize the differential attachments, loyalties, reference-group orientations, and values that organizational personnel may have. A basic assumption commonly employed by researchers in their use of these terms is that the concepts refer to competing, antithetical, or bipolar syndromes. Consequently, people in organizations can be classified as either locals *or* cosmopolitans (if qualitative measurement is assumed), or they can be ordered along a unidimensional attitude scale according to their degree of "localness" or "cosmopolitanness" (if at least an ordinal-measurement model is assumed).

On the one hand, there are the cosmopolitans (or professionals, etc.) who are oriented toward seeking status within their professional group, who have a deep commitment to their specialty, who are strongly committed to their distinctive professional ideology, and who seek the approval and recognition of peers outside the organization as well as of those within it. On the other hand, there are the locals (or organizationals, etc.) whose primary loyalty is to the organization for which they work, who seek advancement up the managerial hierarchy, who identify with organizational goals and values, and who seek recognition primarily from their organizational superiors.

This kind of conceptualization has been common, until very recently, in theoretical and empirical work concerned with the orientations to be found within organizations in general[3] and within industrial research and development laboratories in particular. For example, such writers as Herbert A. Shepard,[4] Simon Marcson,[5] and Hollis Peter[6] have viewed the goals

[1] This research was completed under a National Science Foundation grant while the authors were working on a study of idea flow and project selection in industrial laboratories. This study is presently being conducted in the Department of Industrial Engineering and Management Science at Northwestern University. We would like to express our debt to Dr. James A. Davis of the University of Chicago and Dr. Robert Gordon of the Johns Hopkins University for a critical reading of an earlier draft of this paper, and to the Northwestern University Computing Center, which generously provided free computing time for the factor analyses.

[2] Robert K. Merton, *Social Theory and Social Structure* (2d ed.; Glencoe, Ill.: Free Press, 1957).

[3] E.g., James A. Davis, "Locals and Cosmopolitans in American Graduate Schools," *International Journal of Comparative Sociology*, II (1962), 212–23; Alvin W. Gouldner, "Cosmopolitans and Locals: Towards an Analysis of Latent Social Roles," *Administrative Science Quarterly*, II (1957–58), 281–306. For excellent general discussions and reviews of the literature see Peter M. Blau and W. Richard Scott, *Formal Organizations* (San Francisco: Chandler Publishing Co., 1962), and William Kornhauser and Warren Hagstrom, *Scientists in Industry: Conflict and Accommodation* (Berkeley: University of California Press, 1962).

[4] "Nine Dilemmas in Industrial Research," *Administrative Science Quarterly* (1956), p. 298.

[5] *The Scientist in American Industry* (Princeton, N.J.: Industrial Relations Section, Princeton University, 1960).

[6] "Human Factors in Research Administration," in Rensis Likert and Samuel P. Hayes (eds.), *Some Applications of Behavioral Research* ("Science and

and expectations of professionally oriented and organizationally oriented research as standing in sharp contrast to each other and have considered research staffs as likely to be internally divided between those persons who are interested in management promotion and those who are interested in research achievements that will bring professional recognition.[7]

There has been, however, a growing body of evidence[8] to indicate that a bimodal distribution of orientations may not necessarily be one of the "givens" of research laboratories and that a researcher's orientation toward his profession can be independent of his orientation toward the organization for which he works. Barney Glaser,[9] for one, has written about the high motivation of the scientist who has *both* a local and a cosmopolitan orientation; while Robert Avery[10] has suggested that pure-type locals and pure-type cosmopolitans, felt by some writers to be the dominant orientations, are perhaps rare in laboratories, with most research and development personnel manifesting local as well as cosmopolitan tendencies in their efforts to extract advantages from both organizational and professional sources.

METHODOLOGY OF THE PRESENT STUDY

In a recent study of the factors that went into decision-making on technical ideas in one industrial research and development laboratory, questionnaire data were collected which were relevant to the existence of local and cosmopolitan orientations in laboratories and the question of whether these orientations could be considered as empirically bipolar. The data to be discussed here are the ratings or weightings that researchers and managers said they gave to various criteria in evaluating research ideas. Before discussing the details of the questionnaire and its administration, including the assumptions underlying the technique, it will be helpful to present a brief description of the context in which the present research was conducted.

A. THE RESEARCH SETTING

The study was carried out in the research and development laboratory of a leading company in the field of material. The company is "vertically integrated," directly controlling all phases in the processing and merchandising of the basic raw material. In terms of organizational structure, the laboratory is divided into four product divisions and one small basic-research department. Altogether, 153 persons were working in the laboratory at the time of the study, including twenty-one managers, seventy-three professionals, and thirty-nine non-professional or supporting personnel. In Table 1 is shown a breakdown of the educational backgrounds of

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Society Series") (Paris: UNESCO, 1957), pp. 140–42.

[7] This point of view is well summed up in the following statement by Shepard: "The research staff itself is likely to be divided into cosmopolitans and locals. The former are oriented toward success as members of their profession, and their interest in the company is limited to its adequacy as a provider of facilities for them to pursue their professional work. Since they are productive, they may be valuable to the company, but such value is an almost accidental by-product of their work. The locals are good company men, but their interest is likely to be less in their work than in their advancement in the company" (*op. cit.*, p. 300).

[8] E.g., Donald Pelz, "Some Social Factors Related to Performance in a Research Organization," *Administrative Science Quarterly*, I (1956), 310–12. Data collected on role orientations in other organizations have indicated that a person's professional orientation may be relatively independent of his organizational orientation and that a multidimensional conceptualization of the phenomena covered by the terms local-cosmopolitan may be more useful than a unidimensional one. E.g., see Leonard Reissman, "A Study of Role Conceptions in Bureaucracy," *Social Forces*, XXVII (1949), 305–10; and Alvin W. Gouldner, *op. cit.*, pp. 444–80.

[9] See "The Local-Cosmopolitan Scientist," *American Journal of Sociology*, LXIX (1963), 249–60.

[10] "Enculturation in Industrial Research," *IRE Trans-Actions on Engineering Management*, VII (1960), 20.

the chemists, engineers, and managers for whom data were available.

The company does not depend very much on basic science for accomplishing its research and development goals, which at present are primarily product development, support of the manufacturing operation, and technical service. One might consider it typical of many whose tightly controlled applied-research and development activities are considerably more common in industry than the "blue-sky," long-range, exploratory type of research operations.

TABLE 1

COLLEGE DEGREES OF MANAGERS AND PROFESSIONALS*

Percentage Distribution by Major Field	Per Cent
Chemistry	38
Chemical engineering	27
Other technical fields (ceramic and mechanical engineering, biology, etc.)	20
Non-technical field (education, business, etc.)	9
No college degree	5
Total	99
Percentage Distribution by Amount of Education	
B.S. or B.A.	80
M.S. or M.A.	13
Ph.D.	2
No degree	5
Total	100

* $N = 81$.

B. THE QUESTIONNAIRE

As part of our research program in this company, a confidential survey questionnaire was administered to the managerial and professional personnel. The question dealing with the criteria used in evaluating technical ideas for projects was stated in this manner:

When you evaluate your own ideas, the ones you thought up, how much importance do you usually give the following considerations in deciding whether or not the idea is a good one? (If you are a person whose job does not usually involve originating ideas, answer the items below in terms of the amount of importance you give them when you evaluate other people's ideas.)

This question was followed by a list of thirty-six criteria in a five-point response format:

5 = of utmost importance, always considered;
4 = usually important, often considered;
3 = occasionally important, sometimes considered;
2 = rather unimportant, rarely considered;
1 = not important at all, never considered.

The professional criteria[11] which the respondent was asked to rate included such items as "enhance my reputation in my special technical field," "interest and approval of colleagues and co-workers," "theoretical relevance for the state of existing knowledge," and "improve the reputation of the company R&D program in the eyes of the scientific and engineering professions." Among the items assumed to be organizational criteria were such items as "fits into product line," "increase my chances for a promotion," "will help achieve company goals," and "improve or maintain company status in the eyes of its customers."

The question was asked because it seemed relevant to the problem of localism versus cosmopolitanism, for it was felt that the criteria that would or would not be emphasized in the evaluation of technical ideas would reflect basic orientations of laboratory personnel toward their organizational and professional roles. Two assumptions are important here. The first is the methodological one that the criteria that individuals emphasize—or fail to emphasize—can be considered as projective indicators of decision-making perspectives. The second assumption is that among the crucial decisions made in industrial laboratories are those concerned with technical ideas for research and development projects—whether to submit, communicate,

[11] Of the thirty-six criteria, twelve were designated, on the basis of their face content, as professional-scientific criteria. These items are identified with an asterisk in Table 2.

modify, accept, or reject them—and that these decisions typically bring forth expressions of broad role orientations of which a person's perspective on technical ideas is one component. This latter assumption is consistent with Merton's definition of an orientation as a "theme . . . [which] finds expression in each of the complex of social roles in which the individual is implicated."[12]

C. ADMINISTRATION AND COLLECTION OF QUESTIONNAIRES

The questionnaire was administered in the laboratory in November, 1962. Each professional and manager was handed a sealed envelope containing a questionnaire which was picked up from him a few days later. Overall, eighty-one persons or 86 per cent of the total managers and professionals in the laboratory completed the questionnaire. To examine the possibility of a self-selection bias in the respondents, a categorization of the returns by organizational location, function, and rank was made. The results indicated that our sample, which included 80 per cent of the engineers, 94 per cent of the chemists, and 86 per cent of the managers, could be considered as representative of the entire organization.

DATA ANALYSIS

Does the structure of the correlations among the items indicate the existence of an organizational and professional component in the evaluation of technical ideas? From a technical standpoint, the question is whether the thirty-six criteria represented essentially different and independent considerations or whether they could be considered as manifestations of a smaller number of more basic evaluative dimensions or factors. The methodology most appropriate for this endeavor was a factor analysis[13] of the 36 $\times$ 36 correlation matrix formed by intercorrelating the raw scores of the eighty-one laboratory personnel on the thirty-six items.

[12] *Op. cit.*, p. 392.

Using the BIMD 17 program developed by Wilfred Dixon of UCLA, a principal-axis factor analysis was carried out. With squared multiple correlations placed in the diagonal of the 36 $\times$ 36 correlation matrix as the lower-bound estimate of the real communality of each variable, the program extracted one general factor and twenty-three bipolar factors in decreasing order of their contribution to the total common variance of the original thirty-six variables.

Rotation to a factorially invariant solution was then carried out via the subroutine for the Kaiser Varimax criterion and rotations for 6, 3, and 2 factors compared. The results indicated that a rotated two-factor orthogonal solution was the preferred factorial description of the structure of the correlation matrix. The reasons for this were as follows:

1) While some twenty-four common factors accounting for 70 per cent of the total unit variance were initially extracted by the principal-axis method, the first two were found to account for nearly one-half of this amount.

2) An examination of the incremental proportion of variance extracted by each factor showed a very sharp drop-off between factors II and III. This was the largest incremental change between any two successive factors. From factor III to factor XXIV there were only small incremental changes between successive factors, indicating essentially level contributions for these remaining twenty-two factors.

3) A comparison of the distribution of factor loadings for 6, 3, and 2 factors showed that (a) the two-factor solution had stronger bimodal properties for the distribution of items on factors and (b) the items themselves divided in a "cleaner" fashion for a two-factor solution—an item that was high on one factor was very low on the second, and vice versa.

[13] See Harry H. Harman, *Modern Factor Analysis* (Chicago: University of Chicago Press, 1960), for a comprehensive treatment of the subject.

INTERPRETATION OF THE FACTORS

The loadings of the thirty-six evaluative criteria on the two rotated orthogonal factors with the amount of common variance accounted for by the two-factor solution are shown in Table 2. The items have been grouped according to the factor with which they were most highly correlated and arranged in descending order of the size of their loadings on that factor. Altogether, sixteen items loaded higher on factor I than on factor II. Eleven of these had loadings on factor I which were greater than .55 and only 2 had loadings of less than .40. Of the twenty items that loaded higher on factor II than factor I, nine had loadings greater than .55 on factor II, and another five had factor coefficients between .40 and .50.

The most striking characteristic of the criteria that loaded higher on factor I than on factor II is that they include all twelve of the items originally selected as indicators of a professional-scientific orientation; and only two of these criteria had loadings of less than .55 on factor I. The three highest loading items, for instance,

TABLE 2

LOADINGS OF THE 36 EVALUATION CRITERIA ON THE TWO ROTATED ORTHOGONAL FACTORS

Item Name	Communality	Factor I	Factor II
*Interest and approval of colleagues and co-workers	.593	*.760*	−.046
*Enhance reputation in special technical field	.564	*.733*	−.166
*Improve reputation of company in eyes of the engineering and scientific professions	.496	*.699*	−.086
*Challenge presented to me by the idea	.477	*.688*	−.054
*Intrinsic interest I have in special technical field	.433	*.655*	.065
*Enjoyment I would have in working on the idea	.442	*.648*	.148
*Opportunity to do good research	.387	*.616*	−.083
Increase my chances for a promotion	.399	*.596*	−.208
*Theoretical relevance for existing knowledge	.338	*.580*	.043
*New breakthrough in theory, concepts, or methods	.316	*.561*	.036
*Originality (is it creative)	.310	*.556*	.030
*Could write an article on it for a technical or professional journal	.209	.456	.015
Preferences of my manager or superiors	.210	.437	−.135
Keep people who work under me happy	.265	.434	−.277
Improve product quality	.197	.392	−.208
*Would advance the state of the art	.051	.202	−.098
Amount of capital investment	.484	−.042	−*.695*
Number of man-hours or people	.433	.074	−*.654*
Equipment expenditure required	.409	−.084	−*.634*
Improve or maintain company status in the eyes of its customers	.393	.093	−*.620*
Protecting existing markets	.395	.013	−*.638*
Amount of risk involved	.383	.048	−*.617*
Potential net profit	.358	−.072	−*.594*
Fits into company product line	.429	−.283	−*.591*
Salability to other areas of the company	.382	.204	−*.584*
Length of time the project will take	.246	.087	−.489
Will help achieve company's goals	.227	.027	−.475
Opening new markets	.237	.144	−.465
How much R&D will cost	.219	.100	−.457
Technical feasibility from a production standpoint	.180	−.051	−.421
Consumer acceptance	.155	.076	−.386
Importance of other ideas that could not be worked on	.140	−.035	−.373
Furthering company growth	.160	.209	−.341
Attractiveness to higher management	.183	.275	−.327
Patentability	.137	.210	−.305
Practicability	.083	.054	−.284
Amount of Common Variance Accounted for by Two-Factor Solution	11.300	5.671	5.629

* See note 11.

might be interpreted as reflecting sensitivity and desire for status and respect from professionals inside and outside the laboratory, while the next four reflect an interest in doing technical work that is meaningful and enjoyable to the person. Three other items that loaded above .55 would seem to show concern with the contribution a person's ideas make in his particular field. There is thus a strong lure to label this factor the cosmopolitan or professional factor in the traditional meaning of these terms.

The surprisingly high positive loadings of the items "increase my chance for a promotion" and "preferences of my manager or superiors" on this factor prevent such a simplistic interpretation. These loadings have a theoretical significance, since, as was noted earlier, professional advancement and organizational advancement have been viewed by some writers as competing ends for industrial researchers, the person in the laboratory having to make a choice between "advancement in the company" and "success as a member of his profession." The attitudinal content of factor I is thus not entirely consistent with the traditional definition of a professional or cosmopolitan outlook. The data are, however, consistent with Avery's self-interest hypothesis, which views research personnel as desiring to extract advantages from professional and organizational sources. For this reason, factor I is perhaps best labeled a "professional self-gratification" factor.

In contrast to factor I, which had loaded on it all the items having a "self-oriented" character, factor II is distinguished by the complete absence of such items. The dominant theme to be inferred from the items correlating above .55 with factor II concerns the security and success of the company. The cost and investment aspects of the idea, the compatibility of an idea with company market and profit goals, and the general sensitivity to resource and organizational constraints on the work which is done in the laboratory are the predominant characteristics of the factor II items. This dimension may be referred to as reflecting a person's awareness of the organizational context in which the technical work must be done and his sense of responsibility for seeing that the idea meets the requirements of this context. For this reason, factor II may suitably be defined as the factor of "organizational responsibility in the evaluation of a technical idea.

As can be seen, the results of the factor analysis have led us so far to some questions concerning the empirical content of the attitudes which are usually blanketed by the terms "professional" and "organizational." The other way in which the factor analysis is theoretically important is that the orthogonality of the two factors supports a conceptualization that the "professional self-gratification" factor and the "organizational responsibility" factor are independent dimensions.

The theoretical consequence of this result is that a complete description of a person's orientation in this laboratory will require his categorization in a two-dimensional configuration. This implies a fourfold typology of orientations. In addition to those who rate the professional criteria considerably more important than organizational criteria, and vice-versa, there are two other types: those who have a "complex" orientation in the sense that they take into account the relevance of an idea for both their personal gratifications and the success of the company and those who seem to have an indifferent or disinterested orientation in the sense of not emphasizing consideration of either professional gratification or organizational responsibility. The latter group seems disinterested, of course, only in relation to those who scored high on one or both factors. Also, they may use other kinds of criteria, perhaps of a more idiosyncratic character, which were neglected by our questionnaire.

SUMMARY AND CONCLUSIONS

To summarize, data relevant to the problem of whether individuals in organi-

zations such as research and development laboratories can be divided into two groups (locals or cosmopolitans, professionals or organizationals, etc.), or ordered along a single attitude dimension, have been examined in this paper. The data consisted of the responses of individuals in one industrial research laboratory when asked to rate various professional and organizational criteria as to their importance in the evaluation of the worth of a technical idea. A factor analysis was used to determine the underlying dimensionality of the thirty-six criteria which were rated by the eighty-one managers and professionals. Two orthogonal dimensions were delineated as the best solution to the factor problem. The content of the items that loaded high on the factors led us to name one a "professional self-gratification" factor and the other an "organizational responsibility" factor. A theoretically significant finding of this solution was the high loading of the two items concerned with "advancement in the organization" and "pleasing organizational superiors" on the factor that included all the professional-scientific items. This was interpreted as meaning that the personnel in this laboratory did not choose between organizational and professional rewards, as has been suggested in the literature, but that they varied in the extent to which they sought after personal gratifications in general, whether these came from the organization or the profession.

There is, of course, the possibility that the organization we studied was a deviant case and that the view of laboratories as tending to be divided into two polarized camps is the correct one for most organizations. This is a question for further empirical work. Our own hunch is that sociologists might expect to find considerable variation among the population of research laboratories on the degree to which there is a cleavage between those who have a "professional orientation" and those who have an "organizational orientation." Some organizations may be similar to the one we have studied in which professional and organizational orientations seem to be essentially independent. Other laboratories may present a state of affairs more typical of that suggested by Shepard, Marcson, and Peter. If this is the case, however, then it should point up the need for theories that explain the causes and consequences of these variations.

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Age Norms, Age Constraints, and Adult Socialization[1]

In all societies, age is one of the bases for the ascription of status and one of the underlying dimensions by which social interaction is regulated. Anthropologists have studied age-grading in simple societies, and sociologists in the tradition of Mannheim have been interested in the relations between generations; but little systematic attention has been given to the ways in which age groups relate to each other in complex societies or to systems of norms which refer to age-appropriate behavior. A promising group of theoretical papers which appeared twenty or more years ago have now become classics,[2] but with the

[1] Adapted from the paper "Age Norms and Age Constraints in Adulthood," presented at the annual meeting of the American Sociological Association, September, 1963. This study has been financed by research grant No. 4200 from the National Institute of Mental Health (Bernice L. Neugarten, principal investigator). The authors are indebted to Mrs. Karol Weinstein for assistance in the collection and treatment of the data.

[2] Following the classic article by Karl Mannheim ("The Problem of Generations," *Essays on the Sociology of Knowledge* [New York: Oxford

exceptions of a major contribution by Eisenstadt and a provocative paper by Berger,[3] little theoretical or empirical work has been done in this area in the two decades that have intervened, and there has been little development of what might be called a sociology of age.

The present paper deals with two related issues: first, with the degree of constraint perceived with regard to age norms that operate in American society; second, with adult socialization to those norms.[4] Preliminary to presenting the data that bear upon these issues, however, a few comments regarding the age-norm system and certain illustrative observations gathered earlier may help to provide context for this study.

BACKGROUND CONCEPTS AND OBSERVATIONS

Expectations regarding age-appropriate behavior form an elaborated and pervasive system of norms governing behavior and interaction, a network of expectations that is imbedded throughout the cultural fabric of adult life. There exists what might be called a prescriptive timetable for the ordering of major life events: a time in the life span when men and women are expected to marry, a time to raise children, a time to retire. This normative pattern is adhered to, more or less consistently, by most persons in the society. Although the actual occurrences of major life events for both men and women are influenced by a variety of life contingencies, and although the norms themselves vary somewhat from one group of persons to another, it can easily be demonstrated that norms and actual occurrences are closely related. Age norms and age expectations operate as prods and brakes upon behavior, in some instances hastening an event, in others delaying it. Men and women are aware not only of the social clocks that operate in various areas of their lives, but they are aware also of their own timing and readily describe themselves as "early," "late," or "on time" with regard to family and occupational events.

Age norms operate also in many less clear-cut ways and in more peripheral areas of adult life as illustrated in such phrases as "He's too old to be working so hard" or "She's too young to wear that style of clothing" or "That's a strange thing for a man of his age to say." The concern over age-appropriate behavior is further illustrated by colloquialisms such as "Act your age!"—an exhortation made to the adult as well as to the child in this society.

Such norms, implicit or explicit, are supported by a wide variety of sanctions ranging from those, on the one hand, that relate directly to the physical health of the transgressor to those, on the other hand, that stress the deleterious effects of the transgression on other persons. For example, the fifty-year-old man who insists on a strenuous athletic life is chastised for inviting an impairment of his own health; a middle-aged woman who dresses like an adolescent brings into question her husband's good

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University Press, 1952], pp. 276–322), these include Ralph Linton's discussion in *The Study of Man* (New York: Appleton-Century, 1936); Ruth Benedict, "Continuities and Discontinuities in Culture Conditioning," *Psychiatry*, I (1938), 161–67; Kingsley Davis, "The Sociology of Parent-Youth Conflict," *American Sociological Review*, V (1940), 523–35; and Talcott Parsons, "Age and Sex in the Social Structure of the United States," *American Sociological Review*, VII (October, 1942), 604–16. Anthropological classics include Arnold Van Gennep (1908), *The Rites of Passage* (Chicago: University of Chicago Press, 1960); Robert H. Lowie (1920), *Primitive Society* (New York: Harper & Bros., 1961). More recently, A. H. J. Prins, *East African Age-Class Systems* (Groningen: J. B. Wolters, 1953) has presented a critical analysis of concepts and terms in use among anthropologists.

[3] S. N. Eisenstadt, *From Generation to Generation* (Glencoe, Ill.: Free Press, 1956); and Bennett M. Berger, "How Long Is a Generation?" *British Journal of Sociology*, XI (1960), 10–23.

[4] With some exceptions, such as the work of Robert K. Merton, *Social Theory and Social Structure* (Glencoe, Ill.: Free Press, 1957), sociologists have as yet given little attention to the broader problem of adult socialization.

judgment as well as her own; a middle-aged couple who decide to have another child are criticized because of the presumed embarrassment to their adolescent or married children. Whether affecting the self or others, age norms and accompanying sanctions are relevant to a great variety of adult behaviors; they are both systematic and pervasive in American society.

Despite the diversity of value patterns, life styles, and reference groups that influence attitudes, a high degree of consensus can be demonstrated with regard to age-appropriate and age-linked behaviors as illustrated by data shown in Table 1. The table shows how responses were distributed when a representative sample of middle-class men and women aged forty to seventy[5] were asked such questions as: "What do you think is the best age for a man to marry? . . . to finish school?" "What age comes to your mind when you think of a 'young' man? . . . an 'old' man?" "At what age do you think a man has the most responsibilities? . . . accomplishes the most?"[6]

The consensus indicated in the table is not limited to persons residing in a particular region of the United States or to middle-aged persons. Responses to the same set of questions were obtained from other middle-class groups: one group of fifty men and women aged twenty to thirty re-

TABLE 1

CONSENSUS IN A MIDDLE-CLASS MIDDLE-AGED SAMPLE REGARDING VARIOUS AGE-RELATED CHARACTERISTICS

	Age Range Designated as Appropriate or Expected	Per Cent Who Concur	
		Men (N = 50)	Women (N = 43)
Best age for a man to marry	20–25	80	90
Best age for a woman to marry	19–24	85	90
When most people should become grandparents	45–50	84	79
Best age for most people to finish school and go to work	20–22	86	82
When most men should be settled on a career	24–26	74	64
When most men hold their top jobs	45–50	71	58
When most people should be ready to retire	60–65	83	86
A young man	18–22	84	83
A middle-aged man	40–50	86	75
An old man	65–75	75	57
A young woman	18–24	89	88
A middle-aged woman	40–50	87	77
An old woman	60–75	83	87
When a man has the most responsibilities	35–50	79	75
When a man accomplishes most	40–50	82	71
The prime of life for a man	35–50	86	80
When a woman has the most responsibilities	25–40	93	91
When a woman accomplishes most	30–45	94	92
A good-looking woman	20–35	92	82

[5] The sample was drawn by area-probability methods (a 2 per cent listing of households in randomly selected census tracts) with the resulting pool of cases then stratified by age, sex, and socio-economic status. Using the indexes of occupation, level of education, house type, and area of residence, these respondents were all middle class. The data were gathered in connection with the Kansas City Studies of Adult Life, a research program carried out over a period of years under the direction of Robert J. Havighurst, William E. Henry, Bernice L. Neugarten, and other members of the Committee on Human Development, University of Chicago.

[6] For each item in the table, the percentages that appear in the third and fourth columns obviously vary directly with the breadth of the age span shown for that item. The age span shown was, in

siding in a second midwestern city, a group of sixty Negro men and women aged forty to sixty in a third midwestern city, and a group of forty persons aged seventy to eighty in a New England community. Essentially the same patterns emerged in each set of data.

THE PROBLEM AND THE METHOD

Based upon various sets of data such as those illustrated in Table 1, the present investigation proceeded on the assumption that age norms and age expectations operate in this society as a system of social control. For a great variety of behaviors, there is a span of years within which the occurrence of a given behavior is regarded as appropriate. When the behavior occurs outside that span of years, it is regarded as inappropriate and is negatively sanctioned.

The specific questions of this study were these: How do members of the society vary in their perception of the strictures involved in age norms, or in the degree of constraint they perceive with regard to age-appropriate behaviors? To what extent are personal attitudes congruent with the attitudes ascribed to the generalized other? Finally, using this congruence as an index of socialization, can adult socialization to age norms be shown to occur as respondents themselves increase in age?

The instrument.—A questionnaire was constructed in which the respondent was asked on each of a series of items which of three ages he would regard as appropriate or inappropriate, or which he would approve or disapprove. As seen in the illustrations below, the age spans being proposed were intended to be psychologically rather than chronologically equal in the sense that for some events a broad age span is appropriate, for others, a narrow one.

A woman who feels it's all right at her age to wear a two-piece bathing suit to the beach:

> When she's 45 (approve or disapprove)
> When she's 30 (approve or disapprove)
> When she's 18 (approve or disapprove).

Other illustrative items were:

A woman who decides to have another child (when she's 45, 37, 30).

A man who's willing to move his family from one town to another to get ahead in his company (when he's 45, 35, 25).

A couple who like to do the "Twist" (when they're 55, 30, 20).

A man who still prefers living with his parents rather than getting his own apartment (when he's 30, 25, 21).

A couple who move across country so they can live near their married children (when they're 40, 55, 70).

The thirty-nine items finally selected after careful pretesting are divided equally into three types: those that relate to occupational career; those that relate to the family cycle; and a broader grouping that refer to recreation, appearance, and consumption behaviors. In addition, the items were varied systematically with regard to their applicability to three periods: young adulthood, middle age, and old age.

In general, then, the questionnaire presents the respondent with a relatively balanced selection of adult behaviors which were known from pretesting to be successful in evoking age discriminations. A means of scoring was devised whereby the score reflects the degree of refinement with which the respondent makes age discriminations. For instance, the respondent who approves of a couple dancing the "Twist" if they are twenty, but who disapproves if they are thirty, is placing relative age constraint

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turn, the one selected by the investigators to produce the most accurate reflection of the consensus that existed in the data.

The way in which degree of consensus was calculated can be illustrated on "Best age for a man to marry." Individuals usually responded to this item in terms of specific years, such as "20" or "22," or in terms of narrow ranges, such as "from 20 to 23." These responses were counted as consensus within the five-year age range shown in Table 1, on the grounds that the respondents were concurring that the best age was somewhere between twenty and twenty-five. A response such as "18 to 20" or "any time in the 20's" was outside the range regarded as consensus and was therefore excluded.

upon this item of behavior as compared to another respondent who approves the "Twist" both at age twenty and at age thirty, but not at age fifty-five. The higher the score, the more the respondent regards age as a salient dimension across a wide variety of behaviors and the more constraint he accepts in the operation of age norms.[7]

The sample.—A quota sample of middle-class respondents was obtained in which level of education, occupation, and area of residence were used to determine social class. The sample is divided into six age-sex cells: fifty men and fifty women aged twenty to thirty, one hundred men and one hundred women aged thirty to fifty-five, and fifty men and fifty women aged sixty-five and over. Of the four hundred respondents, all but a few in the older group were or had been married. The great majority were parents of one or more children.

The only known bias in the sample occurs in the older group (median age for men is sixty-nine; for women seventy-two) where most individuals were members of Senior Citizens clubs and where, as a result, the subsample is biased in the direction of better health and greater community involvement than can be expected for the universe of persons in this age range. While Senior Citizens is a highly age-conscious and highly age-graded association from the perspective of the wider society, there is no evidence that the seventy-year-old who joins is any more or any less aware of age discriminations than is the seventy-year-old who does not join.[8] The older group was no more or less homogeneous with regard to religious affiliation, ethnic background, or indexes of social class than were the other two age groups in this sample.

Administration.—To investigate the similarity between personal attitudes and attitudes ascribed to the generalized other, the questionnaire was first administered with instructions to give "your personal opinions" about each of the items; then the respondent was given a second copy of the questionnaire and asked to respond in the way he believed "most people" would respond.[9]

[7] For each item of behavior, one of the ages being proposed is scored as the "appropriate" age; another, the "marginal"; and the third, the "inappropriate" (the age at which the behavior is usually proscribed on the basis of its transgression of an age norm). A response which expresses disapproval of only the "inappropriate" age is scored 1, while a response which expresses disapproval of not only the "inappropriate" but also the "marginal" age receives a score of 3. The total possible score is 117, a score that could result only if the respondent were perceiving maximum age constraint with regard to every one of the thirty-nine items. A response which expresses approval or disapproval of all three ages for a given behavior is scored zero, since for that respondent the item is not age-related, at least not within the age range being proposed.

The "appropriate" age for each item had previously been designated by the investigators on the basis of previous findings such as those illustrated on Table 1 of this report. That the designations were generally accurate was corroborated by the fact that when the present instrument was administered to the four hundred respondents described here, more than 90 per cent of respondents on successive test items checked "approve" for the "appropriate" one of the three proposed ages.

[8] On the other hand, members of Senior Citizens are more likely to be activists and to regard themselves as younger in outlook than persons who do not join such groups. If this is true, the age differences to be described in the following sections of this paper might be expected to be even more marked in future studies in which samples are more representative.

[9] The problem being studied here relates to problems of conformity, deviation, and personal versus public attitudes. As is true of other empirical research in these areas, the terms used here are not altogether satisfactory, in part because of the lack of uniform terminology in this field. For example, while age norms are in some respects related to "attitudinal" and "doctrinal" conformity as posed by Robert K. Merton ("Social Conformity, Deviation, and Opportunity Structures: A Comment on the Contributions of Dubin and Cloward," *American Sociological Review*, XXIV [1959], 177–189), these data do not fit that analytical framework because age norms are less clear-cut than the norms Merton discusses, and the realms of attitudinal and doctrinal conformity are less prescribed.

Similarly, the projection of personal attitudes upon the generalized other has been studied by

In about half the cases, both forms of the instrument were administered consecutively in personal interviews. In the remainder of the cases, responses on the first form were gathered in group sessions (in one instance, a parents' meeting in a school), and the second form was completed later and returned by mail to the investigator.

The two types of administration were utilized about evenly within each age-sex group. No significant differences in responses were found to be due to this difference in procedure of data-gathering.

FINDINGS

The findings of this study can be read from Figure 1. The figure shows a striking convergence with age between the two sets of attitudes.

1. Age trends within each set of data are opposite in direction. With regard to personal opinions, there is a highly significant increase in scores with age—that is, an increase in the extent to which respondents ascribe importance to age norms and place constraints upon adult behavior in terms of age appropriateness.

2. With regard to "most people's opinions" there is a significant decrease in scores with age—that is, a decrease in the extent to which age constraints are perceived in the society and attributed to a generalized other.

3. Sex differences are minimal with the exception that young women stand somewhat outside the general trend on "personal opinions," with scores that differentiate them from young men but not from middle-aged women.

DISCUSSION

The difference shown in these data between personal attitudes and attitudes at-

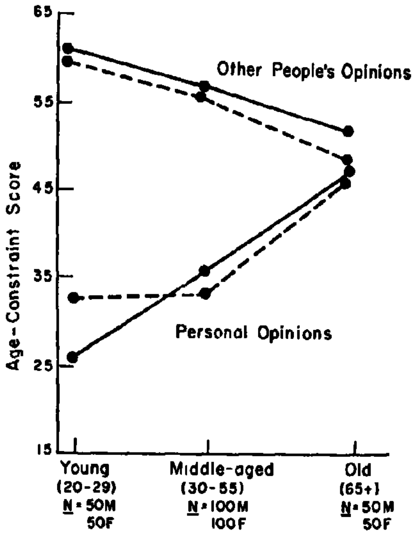


FIG. 1.—Perception of age constraints in adulthood, by age and sex. An analysis of variance for the data on "personal opinions" showed that age was a highly significant variable (*F* is statistically reliable beyond the .001 level); and the interaction between age and sex was significant (*F* is reliable at the .05 level). For the data on "other people's opinions," age alone is a significant variable (*F* is reliable beyond the .001 level). Dotted line, women; solid line, men.

tributed to the generalized other (a finding that holds true for all but the oldest respondents) implies that age norms operate like other types of norms insofar as there is some lack of congruence between that which is acknowledged to be operating in the society and that which is personally accepted as valid. It is noteworthy on the one hand, that age norms are uniformly acknowledged to exist in the minds of "most people." While the data are not shown

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Jacob W. Getzels and J. J. Walsh ("The Method of Paired Direct and Projective Questionnaires in the Study of Attitude Structure and Socialization," *Psychological Monographs*, Vol. LXXVII [Whole No. 454, 1958]); but their theoretical model is not altogether applicable because in the present research the phenomenon of projection cannot be demonstrated. The same lack of fit exists with the concepts used by Milton Rokeach, *The Open and Closed Mind* (New York: Basic Books, 1960); and with the concepts of social norms, norms of common consent, and personal norms as used by Elizabeth Bott, *Family and Social Network* (London: Tavistock, 1957). The *self, generalized other* terminology is therefore regarded as the most appropriate for describing the present data.

here, on each one of the thirty-nine behavioral items some 80 per cent or more of all respondents made age discriminations when asked for "most people's opinions." In other words, general consensus exists that behaviors described in the test instrument are age-related. On the other hand, respondents uniformly attributed greater stricture to age norms in the minds of other people than in their own minds. This difference was reflected in the scores for every respondent as well as in the mean scores.

These findings indicate that there is an overriding norm of "liberal-mindedness" regarding age, whereby men and women consistently maintain that they hold more liberal views than do others. In many ways this situation is reminiscent of the phenomenon of pluralistic ignorance, in which no respondent's personal view of the attitudes of others is altogether correct.[10] In other ways, however, this may be a situation in which respondents tend to exaggerate, rather than to misconstrue, the opinions of others. A young person who says, in effect, "I am not strict about age norms, but other people are," is indeed correct that other people are stricter than he is (as shown in these data on "personal opinions"); but he exaggerates, for other people are not so strict as he thinks. Similarly, when an old person says, in effect, "I think this is the norm, and other people think so, too," he is also partly correct that other old people agree with him, but he ignores what *young* people think.

These partial misconceptions have at least two implications: first, when a person's own opinions differ from the norms he encounters, he may exaggerate the differences and place the norms even further away from his own opinions than is warranted. Second, it may be that in considering age norms, the individual gives undue weight to the opinions of persons who are older or stricter than himself and ignores the opinions of others who are younger or less strict. In both instances, the norm image is not the average of all opinions encountered but the image of the "ideal" norm. In the case of age norms, the "ideal" norms may well be those held by older persons.

The findings of this study are also of interest when viewed within the context of adult socialization. Cross-sectional data of this type must be interpreted with caution since the differences between age groups may reflect historical changes in values and attitudes as much as changes that accompany increased age itself. Still, the findings seem congruent with a theory of adult socialization: that personal belief in the relevance and validity of social norms increases through the adult life span and that, in this instance, as the individual ages he becomes increasingly aware of age discriminations in adult behavior and of the system of social sanctions that operate with regard to age appropriateness. The middle-aged and the old seem to have learned that age is a reasonable criterion by which to evaluate behavior, that to be "off-time" with regard to life events or to show other age-deviant behavior brings with it social and psychological sequelae that cannot be disregarded. In the young, especially the young male, this view is only partially accepted; and there seems to be a certain denial of age as a valid dimension by which to judge behavior.

This age-related difference in point of view is perhaps well illustrated by the response of a twenty-year-old who, when asked what he thought of marriage between seventeen-year-olds, said, "I suppose it would be all right if the boy got a good job, and if they loved each other. Why not? It isn't age that's the important thing." A forty-five-year-old, by contrast, said, "At that age, they'd be foolish. Neither one of them is settled enough. A boy on his own, at seventeen, couldn't support a wife, and he certainly couldn't support children. Kids who marry that young will suffer for it later."

Along with increased personal conviction

[10] Floyd H. Allport, *Social Psychology* (Boston: Houghton Mifflin Co., 1924).

regarding the validity of age norms goes a decreased tendency to perceive the generalized other as restrictive. The over-all convergence in the data, a convergence which we have interpreted in terms of adult socialization, may reflect status and deference relationships between age groups in American society, where high status is afforded the middle-aged and where social enforcement of norms may generally be said to be vested in the mature rather than the young. The young person, having only recently graduated from the age-segregated world of adolescents, and incompletely socialized to adult values, seems to perceive a psychological distance between himself and "most people" and to feel only partially identified with the adult world. This is evidenced by the fact that when asked, "Whom do you have in mind when you think of 'most people'?" young adults tended to answer, "Older people."

Only for old people is there a high degree of congruence between personal opinions and the opinions ascribed to others. This may reflect not only the accumulated effects of adult socialization and the internalization of age norms, but also a certain crystallization of attitudes in the aged. Older respondents volunteered the most vehement and the most opinionated comments as they moved from item to item, as if to underscore the fact that their attitudes with regard to age and age-related behaviors are highly charged emotionally. Under these circumstances, there is likely to be a blurring of distinctions between what the respondent himself regards as right and what he thinks other people would "naturally" regard as right.

With regard to sex differences, the fact that young women perceive greater constraints regarding age-appropriate behavior than do young men is generally congruent with other evidence of differences in socialization for women and men in our society. Young women are probably more highly sensitized to the imperatives of age norms than are young men, given the relatively more stringent expectations regarding age at marriage for women.

It should be recalled that the present study is based upon quota samples of middle-class respondents and that accordingly the findings cannot be readily generalized to other samples. Nevertheless, the findings support the interpretation that age norms are salient over a wide variety of adult behaviors and support the view that adult socialization produces increasingly clear perception of these norms as well as an increasing awareness that the norms provide constraints upon adult behavior.

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On Reporting Rates of Intermarriage

After reviewing all of the available literature on mate selection, particularly on intermarriage, and after conducting a lengthy secondary analysis of survey data from the University of Michigan's Detroit Area Study, the writer believes that several points should be put into writing in regard to the reporting and interpreting of rates of intermarriage. They are, in brief: (1) Let rates based on marriages always be distinguished from rates based on individuals. (2) Let group size be acknowledged as operating through mathematical necessity when it is found to be inversely related to intermarriage rates. (3) One should recognize inevitable differences between ethnic and religious intermarriage rates in evaluating the "triple melting pot" hypothesis. (4) When possible, let the ratio of a group's actual rate of intermarriage to its "expected" intermarriage rate be reported.

THE BASIS FOR COMPUTING RATES

The first point can be disposed of rather quickly. Although most published rates of intermarriage are based on the total number of a group's marriages, it sometimes happens that these are misinterpreted as though they were based on the number of individuals marrying.[1] For example, in a combined sample of 1,470 marriages of whites in the Detroit area, the writer found that interfaith marriages constituted 36 per cent of all marriages involving Protestants and 41 per cent of all marriages involving Catholics.[2] It would be false to conclude from this that 36 per cent of married Protestants and 41 per cent of married Catholics are in mixed marriages. Actually, the rates based on married *individuals* show that 22 per cent of married Protestants and 26 per cent of married Catholics in the Detroit area have intermarried. To understand this, one need only recognize that the 599 *unmixed* Protestant marriages in our combined sample involve twice that number of Protestants, whereas the 336 *mixed* marriages of Protestants involve just 336 Protestants. Similarly, the 343 Catholics in *mixed* marriages (compared to the Catholics in the 495 *unmixed* marriages) comprise just 26 per cent of all married Catholics—considerably lower than 41 per cent mixed-marriage rate for white Catholics in the Detroit area. In the former percentage we are talking about individuals; in the latter, about marriages. Researchers could forestall some misinterpretations by others if they chose to report both types of percentages.

SIZE OF GROUP AS A FACTOR

The second point concerns the size of a group as a factor in its rate of intermarriage. Barron long ago made the observation that the larger the group, the lower its rate."[3] Thomas specified the size of the group as one of the factors influencing Catholic intermarriage rates. He showed by many illustrations from the *Official Catholic Directory* that the highest rates occur in dioceses where Catholics have the smallest representation in the population.[4] Others have observed the same. However, the generalization has been tested most extensively in Canada, where religious affiliation is recorded in the national census and in marriage license applications. A study by Locke *et al.* showed a perfect negative rank-order correlation in 1951 between rates of Catholic intermarriage and the Catholic percentages of the local population in the ten provinces of Canada. The relationship for Anglicans in Canada was said to be "much less striking."[5]

It seems to the present writer that we should recognize this generalization based on the size of the group to be not so much a sociological as a mathematical explanation of the differences in rates of inter-

[1] Two instances of this error are criticized in the following: Loren E. Chancellor and Thomas P. Monahan, "Religious Preference and Interreligious Mixtures in Marriages and Divorces in Iowa," *American Journal of Sociology*, LXI (1955), 233–39; Paul H. Besanceney, "Unbroken Protestant-Catholic Marriages among Whites in the Detroit Area," *American Catholic Sociological Review*, XXIII (1962), 3–20.

[2] The chances are 95 out of 100 that each of these percentages is accurate for the population within four points. See Paul H. Besanceney, "Factors Associated with Protestant-Catholic Marriages in the Detroit Area: A Problem in Social Control" (unpublished Ph.D. dissertation, Michigan State University, 1963), p. 214.

[3] Milton L. Barron, *People Who Intermarry: Intermarriage in a New England Industrial Community* (Syracuse, N.Y.: Syracuse University Press, 1946), p. 191.

[4] John L. Thomas, "The Factor of Religion in the Selection of Marriage Mates," *American Sociological Review*, XVI (1951), 489. See also Lee G. Burchinal and Loren E. Chancellor, "Catholics, Urbanism, and Mixed-Catholic Marriage Rates," *Social Problems*, IX (1962), 363.

[5] Harvey J. Locke, George Sabagh, and Mary M. Thomas, "Interfaith Marriages," *Social Problems*, IV (1957), 331. For rural-urban differences in this matter see Victor J. Traynor, "Urban and Rural Mixed Marriages," *Social Order*, VI (1956), 155.

marriage. To be specific on this point, the smaller the group relative to the total population, the faster its rate goes up with each intermarriage; it can quickly reach a *real* upper limit of a 100 per cent intermarriage rate. A majority group, on the other hand, will find not only that its intermarriage rate goes up more slowly, but that it can never reach a *real* limit of 100 per cent simply because there are not enough mates available outside its group. Therefore, to compare marriage rates without this fact in mind is misleading.[6]

THE "TRIPLE MELTING POT" HYPOTHESIS

We may profitably apply this observation to the "triple melting pot" hypothesis proposed by Ruby Jo Reeves Kennedy some years ago.[7] Based on data covering many decades in New Haven, Connecticut, the hypothesis proposed that ethnic lines were being crossed repeatedly in the United States while people continued to marry within their own religious groups, that is, within the three major religious groups in our country. The point made just above is pertinent here. Since a religious group is larger than any of the ethnic groups that constitute it, we must expect that these ethnic intermarriage rates will be larger than the corresponding religious intermarriage rates—from mathematical necessity.

Furthermore, the fact that identification with a religious denomination is at least in some degree a voluntary association makes it inappropriate to compare rates of religious intermarriage with those of ethnic intermarriage, since the latter are based on an ascribed status. No matter how one is asked to identify his ethnic origins (e.g., by the question, "What is the original nationality of your family on your father's side,") the answer is determined by birth and not by personal choice. On the other hand, we know that husband and wife frequently state that they have the same religious preference even though one of them has converted. Lenski discovered in his sample that "one fifth of the now homogeneous marriages had been contracted by persons raised in different faiths."[8] Similarly, the present writer found in a somewhat larger sample that *three out of five* marriages involving partners raised in different faiths became homogeneous marriages through the conversion of one or the other.[9] When rates of religious intermarriage are based on *present* religious preferences, these facts are hidden and the rates of intermarriage appear to be small.

At least these two facts, therefore, indicate to this writer that the "triple melting pot" hypothesis is not a good substitute for Zangwill's original image of a "single melting pot." Granted that rates of ethnic intermarriage are usually reported to be larger than those of religious intermarriage, a closer examination of the bases for such computations will frequently show that these two types of rates are not truly comparable.

[6] Apart from the problem of sociological analysis, there is great practical importance in this matter. A small group may be faced with extinction through intermarriage with other members of the general population in a given locality. There seems to be a gravitation to the patterns of behavior of the majority in any community, with the result that the children of an intermarriage (other things being equal) will probably be drawn to affiliation with the majority. Hence, in a practical sense, a high rate of intermarriage for a small group is a threat to survival.

[7] Ruby Jo Reeves Kennedy, "Single or Triple Melting Pot? Intermarriage Trends in New Haven, 1870–1940," *American Journal of Sociology,* XLIX (1944), 331–39. See also her "Single or Triple Melting Pot? Intermarriage in New Haven, 1870–1950," *American Journal of Sociology,* LVIII (1952), 56–59.

[8] Gerhard Lenski, *The Religious Factor: A Sociological Study of Religion's Impact on Politics, Economics, and Family Life* (Garden City, N.Y.: Doubleday & Co., Inc., 1961), p. 49.

[9] Besancency, *op. cit.,* p. 218.

RATIO OF ACTUAL TO "EXPECTED" RATES

To return to the problem of the size of a group as a factor in its rate of intermarriage, we believe that, in a national report for 1957, Glick used an effective way of avoiding misconceptions.[10] By also computing the "expected" percentage of intermarriage if the members of each group had married randomly, he arrived at the *ratio* of actual to expected mixed marriages for each religious group. Table 1 presents the findings of our Detroit study after the manner Glick used. We noted above the actual intermarriage rates for Protestants (36 per cent) and Catholics (41 per cent).[11] Let us now observe that the "expected" intermarriage rate would be 68 per cent for Protestants and 73 per cent for Catholics, if they had chosen their marriage partners randomly among whites in the Detroit area. To compute the "expected" rate of religious intermarriage, one must know the frequency distribution of the relevant religious groups in the population. A sample of the Detroit area from the Detroit Area Study, 1954–59, included 3,789 Protestants, 3,307 Catholics, and 642 others, in addition to the Negro denominations.[12] One then uses these totals as marginals in a χ^2 table and makes the calculations shown below Table 1 in order to arrive at the expected rate of intermarriage. The writer believes that he has replicated Glick's statistic reported in the article already cited, although the latter did not give the details of his procedure.[13]

The Catholic and Protestant populations are more nearly equal in the Detroit area than they are in the country as a whole. Nevertheless, the Catholic population is smaller. Computing the ratio of actual to

TABLE 1

ACTUAL INTERMARRIAGE RATES (PER 100 MARRIAGES) AND INTERMARRIAGE RATES "EXPECTED" IF HUSBANDS AND WIVES WERE DISTRIBUTED AT RANDOM, FOR MARRIAGES OF PROTESTANTS AND CATHOLICS IN THE DETROIT AREA, 1955, 1958, AND 1959

EARLY RELIGION OF HUSBAND AND WIFE	MARRIED COUPLES		"EXPECTED" PER CENT IF RANDOM INTERMARRIAGE	RATIO OF ACTUAL TO "EXPECTED" MIXED MARRIAGES
	Actual No.	Actual Per Cent		
One or both Protestant........	935	100	100	
Protestant:				
Protestant....	599	64	32	
Catholic or other......	336	36	68*	.53
One or both Catholic......	838	100	100	
Catholic:				
Catholic......	495	59	27	
Protestant or other......	343	41	73	.56

* As an example for the reader's guidance, the following are the calculations used to determine the expected intermarriage rate for white Protestants in the Detroit area:

RELIGION OF WIVES	RELIGION OF HUSBANDS		
	Protestant	Other	All
Protestant......	928	967	1,895
Other..........	967	1,008	1,975
All........	1,895	1,975	3,870

1,895 × 1,895 ÷ 3,870 = 928

Marriages of Protestants:

928
967
967
2,862

928 ÷ 2,862 = .32

1.00 − .32 = .68

[10] Paul C. Glick, "Intermarriage and Fertility Patterns among Persons in Major Religious Groups," *Eugenics Quarterly*, VII (1960), 31–38.

[11] Permission to use the data on which these rates are based was obtained from Ronald Freedman, then chairman of the Executive Committee of the Detroit Area Study, University of Michigan, and from at least one of the faculty participants responsible for each of the annual studies used. The present writer accepts full responsibility, of course, for interpretations in this paper.

[12] Albert J. Mayer and Harry Sharp, "Religious Preference and Worldly Success," *American Sociological Review*, XXVII (1962), 221.

[13] After this paper had been written, Paul Glick confirmed in private correspondence that the writer had correctly interpreted the methodology he had used for computing this ratio.

expected mixed marriages for the two groups has the effect of narrowing the differences between the measures of their intermarriage behavior. The five-point difference in the actual percentages is reduced to a three-point difference in the ratios. This is the adjustment for the "size of group" factor which the writer believes is needed before comparing intermarriage rates for different groups and different localities.

The ratio of actual to expected mixed marriages reported by Glick for Protestants was .19; for Catholics it was .26. Our ratios are .53 and .56, respectively. Why so much larger? The reason is that our ratios are based on *early* religious preferences and therefore include many marriages that are *now* religiously homogeneous because of the conversion of one partner or the other. Glick's ratios are based on marriages which are *now* mixed, but do not include as "mixed" those marriage partners who were brought up in different religions. If we consider only the *present* religious preference of the respondents and their spouses in our data, as Glick did for the national data, then the ratio computed by the method described above becomes the same for both white Protestants and white Catholics in Detroit: .22. This is certainly comparable to the ratios that Glick computed from the national data.

CONCLUSIONS

We here recommend that in reporting rates of intermarriage researchers (1) indicate rates based both on marriages and on individuals; (2) acknowledge group size as a factor that operates at least partly through mathematical necessity; (3) avoid the "triple melting pot" hypothesis because ethnic and religious intermarriage rates are basically not comparable in such fashion; (4) adjust rates, if possible, to a group's "expected" intermarriage rate.

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LETTERS TO THE EDITOR

A Comment on Conceptual Discontinuity

December 15, 1964

To the Editor:

I would like to call attention to a rather serious discontinuity in conceptual usage in an article by Robert W. Hodge, Paul M. Siegel, and Peter H. Rossi in the November, 1964, issue of this *Journal* ("Occupational Prestige in the United States, 1925–63," pp. 286–302).

The concept referred to is "occupational situs," discussed on pages 300–302 of the article.

Toward the end of an otherwise stimulating and important analysis of occupational prestige, the authors present an analysis of changes in prestige for occupations grouped according to what they call "occupational situses" (Table 5, p. 301). The problem with this occupational classification is that it has little relationship to the meaning that has come to be associated with the concept of situs.

The term "situs" was first coined by Émile Benoit-Smullyan ("Status, Status Types, and Status Interrelations," *American Sociological Review,* IX [April, 1944], 151–61), who used it to refer to a category of individuals or positions placed on a level with other categories, all of which are given the same evaluation. Benoit-Smullyan did not discuss occupational situses but illustrated the concept with reference to clan and sex differentiation.

Paul K. Hatt developed the concept further in his analysis of the North-Hatt occupational-prestige data ("Occupation and Social Stratification," *American Journal of Sociology,* XLIV [May, 1950], 533–43). Using Guttman scaling techniques, Hatt derived eight occupational situses which he called (1) political, (2) professional, (3) business, (4) recreation and aesthetics, (5) agriculture, (6) manual work, (7) military, and (8) service.

Richard T. Morris and Raymond J. Murphy have suggested ten occupational situses which they feel meet Benoit-Smullyan's criterion of equal valuation more adequately than do those suggested by Hatt ("The Situs Dimension in Occupational Structure," *American Sociological Review,* XXIV [April, 1959], 231–39). They define occupational situses as "categories of work which are differentiated in some way but are not invidiously compared" (p. 233). The basis of differentiation is the degree to which occupations contribute to the fulfilment of a particular societal function. The occupational situses proposed by Morris and Murphy are (1) legal authority, (2) finance and records, (3) manufacturing, (4) transportation, (5) extraction, (6) building and maintenance, (7) commerce, (8) aesthetics and entertainment, (9) education and research, and (10) health and welfare.[1]

The occupational "situs" categories used by Hodge, Siegel, and Rossi are (1) free professionals, (2) cultural communication-oriented professions, (3) scientific professions, (4) political/government occupations, (5) big businessmen, (6) customer-oriented occupation, (7) artisans, (8) outdoor-oriented occupations, (9) dead-end occupations, (10) all farm, and (11)

[1] For recent research using this concept see Harry K. Schwarzweller, "Values and Occupational Choice" (*Social Forces,* XXXIX [December, 1960], 126–35), and Donald G. McTavish, "The Differential Prestige of Situs Categories" (*Social Forces,* XLI [May, 1963], 363–68).

other. Clearly, these categories represent a rather gross departure from both the Hatt situses and the more recent Morris-Murphy situses. No reference is made to the earlier work of either Hatt or Morris and Murphy on situs. There is no indication of the logic of criteria involved in the construction of the "situs" categories. It is unclear just why Hodge, Siegel, and Rossi chose to call their categories "situses" and to disregard the conventional meaning of that term. Agreement on basic conceptual terminology seems essential to any kind of progress in the analysis of occupational structure.

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Kavolis' "Economic Correlates of Artistic Creativity"

January 29, 1965

To the Editor:

The stimulating article by Kavolis, "Economic Correlates of Artistic Creativity," which appeared in the November, 1964, issue of this *Journal,* brings to my mind Durkheim's famous thesis about the social integrative role of the notion of Deity. The difference between them is that Kavolis uses an elitist theory of social integration, while Durkheim was a "populist." Kavolis probably follows Sorokin in his concept of logico-meaningful integration that affects only some categories of culture but not the total culture. Simultaneously, a similar emphasis upon the logico-meaningful integration as differentiated from "technical norms" was recently stressed by Gertrude Jaeger and Philip Selznick in their "A Normative Theory of Culture" (*American Sociological Review,* XXIX [October, 1964], 653–69). The question arises, What happens to residual categories of culture called "congeries" by Sorokin?

As far as arts are concerned, there is one more problem. Arts, or some kinds of arts, have usually been consumed by elites only. Provided that we have several elites in a large society, could we relate them to several different types of arts? How is one to explain that particular arts sometimes experienced major efflorescence consecutively and not simultaneously, as Wolfflin, Ligetti, and others have sought to demonstrate? Is this an index of the differentiated elite structure? I would be inclined to think of integration as a more structured phenomenon. For example, in my research on images persons have about the "whole" of a community I have found that different categories such as "men or women," "upper-class and lower-class," and so on possess different imagery of the total community ("The Image of the Whole Community," in *La Sociologia y las Sociedades en desarollo* [Institut International de Sociologie, 1963], III, 113–23). These are, of course, questions that Kavolis did not ask, and consequently did not answer. By writing this note, I would like to point out what rich opportunities open themselves here to students of the core sociological concept, that is, the concept of integration.

JIRI KOLAJA

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BOOK REVIEWS

Sociology on Trial. Edited by MAURICE STEIN and ARTHUR VIDICH. ("Spectrum Books.") Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1963. Pp. 182. $2.25 (paper).

When only six years ago, in a highly favorable review of C. Wright Mills's *The Sociological Imagination,* Dennis Wrong referred to the "Humanistic Underground" of sociology, he could hardly have known that a part of that militant, clandestine group would shortly rise up into the bright glare of day to produce, of all things, an anthology. *Sociology on Trial* is a collection of essays critical of the dominant figures and schools of thought in sociology and is, almost on this ground alone, a welcome sight. But the editors work hard in their brief Introduction at wearing out that welcome quickly by adopting a holier-than-thou conception of themselves and their authors as the "conscience" of sociology, despised and rejected by the careerists in the discipline who have been "unable to resist society's affluent blandishments." If the editors are right about the kind of tight guild control exercised by those who dominate the American Sociological Association, then no mere collection of essays, more than half of them published before, will loosen that grip. But they are very likely wrong; the intellectual stance that these essays represent (a stance, by the way, which has never been adequately characterized by "humanistic," "impressionistic," or, as the editors term it, "critical") is an increasingly important influence in contemporary sociology. The melodramatic manner in which the editors conceive their alleged embattlement, therefore, seems to me to be without justification. In any case it is a small irony, as well as a tribute to the subtle influence of the academic life, that this moralist minority, these promoters of a *sociologie engagée,* should be conducting their intellectual struggles through something that looks suspiciously like a textbook of "readings"—that most academic of literary artifacts.

Because some of the essays are very good while others are very bad, it is difficult to say anything generally evaluative that characterizes the entire book, and it is exactly this that one wants to do with a volume so self-consciously and dramatically titled, wishing so much to stand *for* something collectively. At first glance, it seems fair to say that on the whole the book stands for an ethically relevant sociology and ethically involved sociologists, for attention to the large questions that command that involvement, and for the proposition that objectivity does not require ethical neutrality. Although I share these sentiments, their affirmation does not necessarily make a good book; beyond this minimum common ground the authors go their separate ways with varying degrees of cogency, eloquence, and wit.

The best things in the book are very good indeed. Reprinted from *Social Problems* is Alvin Gouldner's critique of Max Weber, which dares to understand the complex ambivalences behind that tortured figure's influential ideas about the "separation" of facts and values. An essay of permanent value, it is one of those rare examples of sociological writing in which the "feltness" of the prose contributes to the clarity and persuasiveness of the exposition. It is a thoroughly brilliant job. Published here for the first time, John Seeley's essay deals very sensitively with some problems of perspective and indeterminacy in sociology and with the moral consequences of the posture of moral indifference adopted by many sociologists. Although some of the questions that Seeley raises have been adequately answered by Ernest Nagel (see, for example, chap. xiii of *The Structure of Science*), Seeley raises them in such a thoughtful and original way that it is almost as good as if they were genuinely original questions. That infamous chapter v from *The Sociological Imagination,* containing the not-so-subtle attack on that Unnamed Sociologist for which Mills (to whose memory this book is dedicated) was so widely accused of "bad taste,"

is reprinted in full here. I do not find the taste bad. Although Mills's criticism of the sociological "statesman" certainly does *not* apply to Merton, whose reputation is as justly and securely founded as any sociologist's, it is very valuable for its almost perfect characterization of a type every sociologist knows at least one too many of. Lewis Feuer's essay on the history of the concept of alienation is a model of clear-eyed, scholarly thinking about an idea that, abandoned by its parents and having found a foster home in sociology, has comforted far too many less than clear-eyed sociologists.

Unfortunately, not everything in the book is up to the standard set by these essays. Reprinted from *Studies on the Left*, the essay by Hans Gerth and Saul Landau is simply one long peevish grievance against sociology for its alleged neglect of macro-historical concerns. It contains many of the routine complaints one hears about "empirical" sociology but very little understanding of why sociologists do not engage in more historical research of a sociological character. Surely one important reason is that most sociologists (particularly American sociologists) are simply not in possession of the linguistic skills, the historiographic sophistication, and the sheer scholarly patience that it takes to do good historical and comparative sociology. That they are not is no doubt to some extent a matter of the failure of universities and other institutions that contribute to the support of research, but these are matters about which "sociology" can do very little. *Sociologists,* however, can use such influence as they have to see to it, for example, that students are well trained in historical method, that their language examinations do not become empty ritual, and that the appreciation of the historical character of social problems be encouraged. Under the best of circumstances, historical sociology is difficult to do well, but when it is done well "sociology" has been quick to recognize and reward it. If less time were devoted to mourning the absence of Webers in contemporary sociology, there would be more time to create a literature of historical sociology from which students could choose models to emulate.

If Gerth and Landau are peevish about "sociology," Maurice Stein is peevish about "systematic" sociology. Stein affirms clearly enough his preference for "unsystematic" thinkers like Hannah Arendt, Paul Goodman, and Norman Brown over "systematic" thinkers like Merton; less than clear, however, is what Stein means by a "poetic metaphor" and by his suggestion that "drama" and "history," as poetic metaphors in sociology, are suppressed or ignored or relegated to the margins of sociology or out of the discipline altogether by the domination of "system" thinking—which is no less a poetic metaphor than the others. If he means what I think he means, then he is mistaken. The American Sociological Association (whose activities we may use as criteria) has only this year for the first time seen fit to give its MacIver Award to a purely "systematic" thinker. In previous years, Erving Goffman received it for a book self-consciously "dramatistic" and Reinhard Bendix for a book self-consciously historical and comparative, and a study in the sociology of knowledge at that! Others, less eminent and too numerous to mention, have reaped corresponding rewards for works similarly "unsystematic." Moreover, when the ASA can elect *both* Paul Lazarsfeld *and* Herbert Blumer to its presidency, when in successive years it can elect three men of such dissimilar cast of mind as Everett Hughes, George Homans, and Pitirim Sorokin, it means that whatever group "controls" the ASA is far from monolithic; it means also that no sociologist need resign himself to living on the margins of sociology—unless that is where he wants to live. Sociology, to be sure, has its intellectual martinets, but compared with the other social sciences, it is a veritable free market of ideas and intellectual styles in which it is still more than possible for almost any crazy idea to get a sympathetic hearing *so long as an interesting book can be made of it.* And even that isn't necessary if you have access to a ditto machine and well-placed friends to quote your unpublished manuscripts in the learned journals. Many sociologists are so bored with most of the things they have to read that a really interesting book, however heretical of the received wisdom, is likely to be welcomed with at least respectful, if not uniformly good, reviews. And the editors' own *The Eclipse of Community* and *Small Town in Mass Society* are cases in point. This does

not mean that there is not a self-conscious elite in the ASA but only that it is neither monolithic nor powerful enough safely to ignore the superior intellectual achievements of hostile outsiders.

Fortunately, Stein's sense of persecution does not dominate most of the essays. Confidently straightforward and urbane, Robert Nisbet reminds us that the great sociological visions that informed the life work of Durkheim, Weber, and Simmel were not come by "scientifically," but through much the same sort of inspiration that "possesses" artists, and that to forget the art in sociology is to risk trivializing the science or losing it entirely. Reprinted here also is Barrington Moore's well-known article criticizing the major research strategies of mathematical and equilibrium analysis in sociology for their formal, abstract, ahistorical character, for their loss of the critical spirit which informed an earlier sociology, and for bringing contemporary sociology to the point where it has less to say about society then it did a half-century ago. Karl Mannheim is also represented here by his review-essay on Stuart Rice's *Methods in Social Science,* which originally appeared in a 1932 issue of this journal. Mannheim used the occasion to contrast American with German sociology in terms that have since become the standard vocabulary with which partisans of "classic" sociology criticize recent American positivism: it is "methodologically ascetic"; it tends to dry up the sources of scientific inspiration; there is a glaring disproportion between the bulk of the scientific machinery employed and the thinness of the substantive results; and so on. Mannheim conducts his tour of American sociology with a kind of painful politeness that allows him to conclude on a conciliatory note: "there were hardly ever two different styles of study so fit to supplement each other's shortcomings as are the German and American types of sociology." To which one is tempted to say Amen—in lieu of confronting and attempting to explain the disappointing fact that Mannheim's statement is probably as true today as it was a generation ago.

It is instructive to compare Mannheim's exquisite *politesse* on so "sensitive" an issue with the both-barrels polemical style of Daniel Foss writing on the "world view" of Talcott Parsons. I would almost be willing to bet that Foss, whose name I have not seen before, is a ferocious graduate student, the tiger of Brandeis. He stalks Parsons like a big cat, knocks him over with a heavy paw, torments him a little with rough tongue and sharp fang, then eats him. Foss alternates between pointed and relevant criticism of Parsonian theory and bitter, rancorous, contemptuous sarcasm about Parsons the man—and thereby hangs a real problem. For one of the interesting things about sociologists like Foss (and Stein—and others) is the apparently crucial role that the polemical style, the intellectual chip-on-the-shoulder, plays in their work. Foss, for example, seems unsatisfied with merely revealing Parsons' biases, misunderstandings and inconsistencies; his work is suffused with a hatred of Parsons. He must show Parsons to be not simply in error but callously and maliciously and dangerously in error. Now what concerns me here is not primarily the "bad taste," the offense this does to Parsons; he is quite capable of taking care of himself (he is, indeed, never better than when answering his critics). What concerns me is how to regard this kind of intellectual terrorism *when it is also the spirit which informs much that is good in a man's work.* Foss and Stein, like Mills, are at their best when using logic and evidence to slay real or imagined intellectual dragons. Ideas, after all, *are* fairly good weapons, and if one is not only forensically skilled but a battler as well, it is hard not to use what weapons one has.

But academic good manners are very important too, even at the risk of stuffiness, for to neglect them is to endanger the future of scholarly dialogue. Despite the personal vanities and ambitions of scientists, a scholarly dialogue must assume that the discussants have a common interest in arriving at the truth (unlike a political argument, whose participants, interests opposed, aim at winning). Academic good manners support scholarly dialogue by maximizing the probability that intellectual combat will focus on ideas, not personalities; on issues, not egos; on the puzzles of the empirical world, not on the dogmatics of personal doctrine; on the tale, but not on the teller. Academic manners constrain participants in scholarly dialogue (all too human) to yield a point when rea-

son or sense suggests it, while minimizing the "loss of face" involved.

Many sociologists, however, whose spirit of inquiry is sparked primarily by the moral feeling of the involved rather than by the learned curiosity of the detached, find polite debate deadening to the moral imagination. Sociologists like these are quite understandably defending the sources of their own inspiration when, by exercising the polemical style, they try to prevent it from being exiled from sociology. Their very sociological sense tells them (correctly I think) that when a tonelessness without edge is established as the dominant prose style of sociological exposition, it is probably Established values that are being promoted by that tonelessness; hence their continuing interest in revealing the moral and ethical biases and consequences implicit in the "ethical neutrality" of the Established style. But this points up the interesting fact that it is not, then, *any* ethically relevant sociology which *Sociology on Trial* is promoting (Parsons, for example, is still attacked for the "conservative bias" of his functionalism) but, roughly, one relevant to Socialist ethics, and one pursues these, of course, in the classic polemical style of militant socialists, stopping occasionally for an exercise in the sociology of knowledge where this can be used to "unmask" one's opponents. To the extent, then, that Foss's valuable criticism of Parsonian theory depends upon the recharging of his emotional batteries by expressive hostility to Parsons the man, we may have to tolerate the "bad taste" to get the good analysis. On the other hand, those who believe in an ethically relevant sociology must be prepared to tolerate not only the ethical relevance of the work of the heirs of Marx and Mills, but that of Durkheim, Parsons, and their heirs too.

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Investigations of Man as Socius: Their Place in Psychology and the Social Sciences. Edited by SIGMUND KOCH. ("Psychology: A Study of a Science," Vol. VI.) New York: McGraw-Hill Book Co., 1963. Pp. xii+791. $12.50.

This is the penultimate volume of a series organized by the American Psychological Association and edited by Sigmund Koch. Earlier volumes evaluated the major theoretical and substantive materials of psychology and the relations of psychology to the biological sciences. In Volume VI are discussed relations of psychology to the social sciences.

Merely from inspecting the Table of Contents we learn something of how Koch and his advisory committee saw these relations. It shows three chapters covering connections with anthropology and four for economics. Political science and sociology each get one chapter, and there are two chapters on linguistics. Three more concern social psychology.

One must be selective in commenting on so diverse and detailed a book. I focus on the treatments of social psychology and of sociology. It would be a disservice, however, not to note certain chapters of exceptional quality and of likely interest to sociologists. Among them are Lounsbury's on linguistics and Osgood's on psycholinguistics, each an advanced introduction to its subject and each containing an original synthesis and reconstruction of its field. Then there is Herbert Simon's essay on economics. Simon not only portrays what economists have done with psychology but provides as his framework a most instructive sketch of the major tasks of economics—a sketch that then enables him to forecast something of the future of relations between these disciplines.

The three papers on social psychology complement one another but must be read together with those in Volumes III and V of this series to obtain a reasonably accurate view of the field as cultivated in American psychology. The first, by Muzafer Sherif, places us outside the field looking in upon it, calling our attention to the major topics on which research has focused. Sherif's is the informed, exoteric view. So is William Lambert's.

Lambert gives us a content analysis of articles appearing in the *Journal of Abnormal and Social Psychology* in the years 1952 and 1960. As he says, this sample of recent social psychology necessarily omits the larger, systematic works because those are published as books. Nonetheless, his results strike one as

being faithful to the genre and are, at points, devastating. In both of the years studied the majority of investigations employ paper-and-pencil instruments to measure dispositions of individual subjects, those dispositions then being related to one another, to later behaviors, or to conditions (typically known only indirectly) believed to produce those dispositions. When relations between persons are under study, most investigations involve no more than two persons. Social psychology appears in this review of its operations as a study of individuals and dyads. Seventy-one per cent of the papers advance their propositions on these topics as general theories, probably valid across cultures. On the brighter side, Lambert finds some increase in the rigor of procedure from 1952 to 1960 and a growth in the number of studies in which concern for social relations is central to the hypotheses under test rather than a mere operational convenience.

With Donald Campbell's "Social Attitudes and Other Acquired Behavioral Dispositions" we approach social psychology from within, and here, as in Neal Miller's paper in Volume II of the series, we find evidence of a sober and growing confrontation of just those problems that produced what sociologists call symbolic interactionism. Moreover, in Campbell's paper this confrontation is guided explicitly by acquaintance with Mead, Dewey, and Thomas. There is, of course, no virtue in scientific ecumenicity as such. There is value only in the convergence required to explain some otherwise refractory variance. There is exceptional value when the convergence is achieved, not just by adding independent variables, but by integrating the new with the old. This is the direction of Campbell's effort. As a result, sociologists can find here an organization of great ranges of the psychological consideration of learning, perception, cognition, and response, all placed in a framework useful for their concerns. As a further result one can begin to see beyond the realtively schematic assumptions about behavior to which sociologists have clung in the absence of a relevant psychology and one can envision the growth of sophisticated theories of behavior that are relevant for social interaction and that are grounded in solid empirical investigation. Campbell, like James Olds in *The Growth and Structure of Motives*, has taken only a step toward what we need. It is of unusual importance.

Alex Inkeles confines his paper on "Sociology and Psychology" to those uses sociologists make of psychology when the disciplines are conceived of respectively, as the study of social systems and the study of personality. Like many other authors in this symposium, he takes the sensible tack of instructing psychological readers by means of some extended examples. As a result of these two decisions, his paper consists of a classification of studies in which personality and aspects of social organization contribute independently to the explanation of variance in the latter. Some readers will quarrel with his judgment that Freud and the neo-Freudians have given us "a reasonably adequate general theory of personality," and specialists on delinquency and on suicide will accept only with qualifications his extensive appraisals of studies central in their fields. Perhaps Inkeles himself would now boggle at the statement (p. 347) that "the Cargo cult . . . has been reported in a sufficiently large number of cases in widely dispersed culture areas to suggest that there is some general mechanism in human nature which produces this specific type of manifestation." Such specific points aside, he provides his readers with vigorous, comprehensive, and knowing assessments.

Participants in this symposium could not do everything. Some important papers that might have been included have yet to be written. Among them will be an appraisal of contributions from psychology and the social sciences to the study of collective behavior, the growth of the elementary forms of interaction, and the emergence of collective decisions. For explanations of these phenomena, as Campbell hints and Simon suggests, types of theory must be found that relate general processes of behavior and social organization more intimately than can be accomplished by juxtaposing personalities and social systems In these analyses, and Simon has much to say on the point, learning and the higher intellective processes will be of as great if not greater significance than personality. And beyond such appraisals will be efforts to extend what Lounsbury, Simon, Inkeles, and Osgood touch upon: a statement of the general

conditions under which variance in either social relations or personal organization is appropriately explained by variance in the other.

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The Social Democrats in Imperial Germany: A Study in Working-Class Isolation and National Integration. By GUENTHER ROTH. Totowa, N.J.: Bedminster Press, 1963. Pp. 352.

A closer relationship between historical scholarship and sociological analysis is certainly desirable, but it is also very difficult to achieve, and not only for reasons of academic stupidity. For example, the general observation might be offered that the historian trying to open up his data by means of categories and theories of a sociological kind is rather more likely to produce satisfactory results than the sociologist endeavoring to discover new evidence in terms of preconceived theories, even if these are perfectly sound. Despite the claims of the author, it is unlikely that it was his sociology that made L. Goldmann discover, while writing his *Dieu Caché*, evidence of a hitherto unknown line of thought in the Port Royal tradition; on the other hand, R. Bendix's splendid theoretical exposition in his *Work and Authority* tends to fall a little flat as it encounters historical evidence. Roth's study belongs to the category of monographs to which this kind of consideration applies, so that it would seem the first task of the reviewer to find a perspective in terms of which to present the book.

As a piece of historical scholarship, Roth's presentation of Imperial Germany in the early chapters of his study offers little that is new; it is based on a selection of well-known historical accounts. However, as Roth comes to describe German Social Democracy, its emergence and, more particularly, the ideological development of the party and of its followers, he manages to draw widely scattered materials into a brilliant and impressive picture. It is a pleasure rare in sociological studies to read the chapters on "The Role of Marxism in the Social Democratic Subculture," "The Meaning of the Labor Movement to Its Members," and "The Social Democratic Subculture and the Dominant Culture."

One of the reasons for the brilliance of these chapters is that here Roth brings a sociological perspective to bear on important historical data. But he is not content with history in a sociological perspective. There is a theory that Roth wants to propound, cautiously stated at several points and indicated in the subtitle of his book. In brief, this theory states that there are various ways of "integrating" the working classes into industrial societies. England and Russia in the nineteenth century may—according to Roth, here evidently indebted to Bendix, who has also written a Preface to the book—be regarded as two extreme types of this integration: "In the former, integration was accomplished under a parliamentary system of government which acceded step by step to labor's demand for formal political equality. . . . In Russia an authoritarian monarchy insisted on the complete subordination of employers and workers" (p. 305). And: "Imperial Germany constitutes a complex intermediate case between England and Russia" (p. 305), a case which Roth describes, by contrast to "repression" and "democratization," as "isolation" (pp. 83 ff.) So far, of course, this is little more than a descriptive classification; but Roth has a theory in mind, as becomes clear from his statement: "Class cleavage pervaded Imperial Germany because the authoritarian state and the liberal bourgeoisie were unwilling to integrate the labor movement into the national community through compromises resulting in far-reaching democratization" (p. 136).

The question might be raised whether Victorian England is really so very different from Imperial Germany in its treatment of the labor movement and of the whole issue of political equality; but this is a question of historical interpretation. It becomes theoretically interesting if we go one step further and ask ourselves whether there was or is any industrializing country not "pervaded" by "class cleavages." If we take this point of view, the cases of England, Russia, and Imperial Germany become, not three points on a scale of working-class integration, but three examples of the same conflict between

classes about the effective equality of citizenship rights. Owing to the different historical conditions of the three countries, these conflicts took very different forms indeed—forms, however, which might be analyzed in terms of the same general framework. The difference is that, in terms of this framework, patterns of conflict appear more relevant than the problem of "national integration."

I hesitate to advance this suggestion as a criticism of Roth's book or, indeed of its underlying theory. Certainly, the way Roth poses his question is in accordance with the national-social-liberal tradition of Max Weber and of much German historical scholarship. On the other hand, it is conceivable that looking at the history of German Social Democracy in terms of a conflict about equality rather than as a road from isolation to integration might have brought into focus some problems that Roth does not pay much attention to, such as, in his own terms, the "isolation" of the industrial bourgeoisie in Imperial Germany, or indeed the ways in which Social Democratic pressure or resistance has brought about changes in Germany's social constitution.

It might be rewarding to pursue the debate intimated by these remarks, but it seems certain that this debate would merely serve to underline the interest and quality of Roth's study. In places it has some of the more cumbersome properties of a Ph.D. dissertations; elsewhere, one would hope to find a more colorful and comprehensive description of background facts; but in its careful and considered combination of original historical interest and a trained sociological perspective, Roth's study is a welcome addition both to political sociology and to German social history.

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Sociology and History: Theory and Research. Edited by WERNER J. CAHNMAN and ALVIN BOSKOFF. New York: Free Press of Glencoe, 1964. Pp. xix+596. $9.95.

Issuing from the interests expressed in the sessions on "Sociology and History" at the annual meetings of the American Sociological Association, this volume is largely an endeavor to document the historically oriented work of sociologists and the sociologically oriented work of historians. Its forty authors contribute forty-two selections, arranged in three major parts and amounting to 580 pages of text. Twenty-six of its contributors are sociologists, twelve are historians (or specialists in allied fields), one is an anthropologist, and one a rabbi. Readers are not informed about the precise criteria invoked to select the contents (but see pp. viii, ix, 7–8) or how many, if any, of the papers were prepared especially for this book. Eleven papers, two of which were presented earlier at American Sociological Association meetings, are published here for the first time.

Part I on the "Evaluation of Theories" includes six of the new selections. Beginning with Kenneth Bock's "Theories of Progress and Evolution," this section continues with Joseph Maier's "Cyclical Theories," Robert Daniels' "Marxian Theories of Historical Dynamics," Robert Bellah's "Durkheim and History" (originally published in the *American Sociological Review*), Werner Cahnman's "Max Weber and the Methodological Controversy in the Social Sciences" (which admirably supplements Martindale's presentation in *The Nature and Types of Sociological Theory*), Serge Hughes' "The Evaluation of Sociology in Croce's Theory of History," and Alvin Boskoff's "Recent Theories of Social Change."

Part II simply entitled "Recent Research," comprises the bulk of the collection. Only three of the thirty-two selections in this section—Robert Nisbet's "Kinship and Political Power in First-Century Rome," Ernest Manheim's "The Communicator and His Audience: Liberals and Traditionalists in Eighteenth-Century Germany," and Bryce Ryan's "Social Values and Social Change in Ceylon"—appear here for the first time. Obviously, the editors encountered considerable difficulty in subclassifying the contents in five categories: (1) "Feudalism and Power in Various Societies"; (2) "Empire, Nationality, and Religion"; (3) "Enterprise, Labor, and Social Change"; (4) "Political Behavior and Social Structure"; and (5) "The Arts, Literature, and Science." Other arrangements seem to be possible. For instance, articles from all five subdivisions can be extracted to constitute a section on the rise of capitalism and

industrialism (e.g., Norman Jacobs' "Modern Capitalism and Japanese Development," Bryce Ryan's paper mentioned above, Joseph Trachtenberg's "The Attack upon Usury," Norman Birnbaum's "The Zwinglian Reformation in Zurich," William Petersen's "The Demographic Transition in the Netherlands," Reinhard Bendix's "The Ideology of the Masters in Russia," Rupert Vance's "Security and Adjustment: The Return to the Larger Community," William Miller's "The Business Elite in Business Bureaucracies: Careers of Top Executives in the Early Twentieth Century," and Americo Castro's "Hidalguismo: The Sense of Nobility").

Publication of this volume is itself a symptom of welcome historical change in our discipline. This reviewer regrets only that the editors' consideration of sociologists' theoretical and methodological objections to the use of historical data and inquiry was confined to a summary analysis, albeit judicious, balanced, and informed, in the first and last chapters. A more extensive and systematic confrontation would have added to the impact of this work on sociology.

ROSCOE C. HINKLE
Temple University

Problems of Economics and Sociology. By CARL MENGER. Edited by LOUIS SCHNEIDER. Urbana: University of Illinois Press, 1963.

It is extraordinary that Menger's *Untersuchungen über die Methode der Sozialwissenschaften und der Politischen Oekonomie Insbesondere* should not have been translated earlier. Carl Menger is a household name to economists as the founder of the Austrian school; and as he was a pioneer fighter against the historical school then prevailing in the German-speaking academic world his thoughts on methodology are of particular interest. Moreover, as the heroic translator, Francis J. Nock, rightly complains that Menger's German was particularly Germanic. For instance, a paragraph of the original contains two sentences, one of which consists of twenty printed lines, the other of eighteen. Few American or British scholars could have found much pleasure in the original text. They, and future generations of economists and sociologists, must be deeply grateful for this translation.

The volume is divided into four parts: (1) "Economics as a Theoretical Science and Its Relationship to the Historical and Practical Economic Sciences"; (2) "The Historical Point of View in Economic Research"; (3) "The Organic Understanding of Social Phenomena"; and (4) "The Development of the Idea of a Historical Treatment of Political Economy."

It is the third part that will be of most interest to sociologists. Menger is sensible about the organic analogy, so frequently used and abused between a "series of structures of social life" and natural organisms (p. 129). Briefly he considers the analogy valuable "for certain purposes of presentation," though not as a method of research (p. 137). But the most significant point in this book is Menger's spelling out what he considered "perhaps the most noteworthy problem of the social sciences":

> How can it be that institutions which serve the common welfare and are extremely significant for its development come into being without a common will directed towards establishing them? [In italics, p. 146.]

This is certainly not the main problem most sociologists today feel moved to consider. If they think in terms of problems at all, they are interested less in the coming into being of these unwilled institutions than in the defects that result—inequality, immobility, concentration of power, population explosions, urban decay, suburban sprawl, regional poverty—and in the willed plans to change the existing institutions. Nevertheless, it would certainly help in eradicating defects of institutions if there were greater understanding of how the institutions arose.

Menger's chief concern, as the German title indicates, is with methods in economic theory and criticism of the Historical school, however. In the first section of this volume and in several of the nine short appendixes, Menger attacks Baconian empirical induction. His alternative is to ascertain *"the simplest elements* of everything real, elements which must be thought of as strictly typical just because they are the simplest" (p. 60, italics his). In his chapter on "the dogma of self-

interest," (pp. 82–90), perhaps his most illuminating chapter, Menger refers to chemistry and mechanics as based no less than economics on "dogma"—general laws about simple elements, forces, and bodies. But surely these "dogmas" of natural science were originally discovered empirically, while most of the basic assumptions of economics were not. Is not testing these assumptions before too elaborate a superstructure of theory is built upon them an important role of empirical research, in which sociologists, social psychologists, and historians must join with economists?

P. Sargant Florence

University of Birmingham

Shadow and Act. By Ralph Ellison. New York: Random House, 1964. Pp. 317.

Shadow and Act is a collection of essays by Ralph Ellison, the novelist, that makes clear and explicit his position on a variety of social and cultural issues. The volume is replete with material that needs to be called to the attention of the sociological community. Ellison is one of those rare humanists who is at ease, although hardly in agreement, with his sociological colleagues. In particular, the book contains a remarkable autobiographical study and a provocative review written in 1944 but published for the first time of Gunnar Myrdal's *An American Dilemma.*

What events led Ralph Ellison to write a review of Myrdal's study in 1944 is unclear, but in retrospect it hardly seems extraordinary that a sensitive, articulate Negro intellectual should have undertaken such an enterprise. It appears more extraordinary that the *Antioch Review* declined to publish it and that it required twenty years for it to come to public attention. But again, I regret that the specific details are not available. The review is critical of aspects of Myrdal's effort and, in passing, of the University of Chicago "school of sociology," including specifically Robert E. Park, whom Ellison saw as the dominant figure in the development of a sociology of the American Negro.

In my opinion, which on these matters is not worth much and is therefore pretentious to elaborate, Ralph Ellison is the outstanding Negro novelist of this generation. Such a statement is, in addition, a profound insult to this gifted man, because Ellison has established himself first as an extraordinary writer coping with the classic issues that concern novelists, and second as a writer who deals with Negro subject matter. But Ralph Ellison recognizes the genre of sociology that has become a part of everyday language, and he would hardly deny the sociologist a place in that everyday existence.

He does not offer a negotiated peace with the social scientist. "I think that art is a celebration of life even when life extends into death and that the sociological conditions which have made for so much misery in Negro life are not necessarily the only factors which make for the values which I feel should endure and shall endure." Nor does he bother to attack the jargon of social science, but rather, is interested in what we have to say. He seeks to confront our work first in our terms and then to interpret it from his own standpoint. In a scientific age, there is no defensiveness in his intellectual approach. "As for my writer's necessity of cashing in on the pain undergone by my people (and remember I write of the humor as well), writing is my way of confronting often for the hundredth time, that same pain and that same pleasure. It is my way of seeing that it be not in vain."

It is this sheer admixture of incisiveness and powerful language that makes his twenty-years-after-the-fact review of Gunnar Myrdal so startling and avant-garde. Ellison declares that the Negro must, while joining in the chorus of "yeas" which the book has so deservedly evoked, utter a lusty and simultaneous "nay." He proceeds directly to the core of the central theoretical issue—in our language, to Myrdal's frame of reference. In his estimate, the Myrdal contribution hinges on the central notion that "the Negro's entire life and, consequently, also opinions on the Negro problem are, in the main, to be considered as secondary reactions to more primary pressures from the side of the dominant white majority."

Such a perspective is rejected, and I mean rejected by Ellison. He argues, to the contrary,

"But can a people live and develop for over three hundred years simply by reacting? Are American Negroes simply the creation of white men, or have they at least helped to create themselves out of what they found around them?" He adds for good measure. "Men have made a way of life in caves and upon cliffs, why cannot Negroes have made a life among the horns of the white man's dilemma?"

Many sociologists will find this a congenial point of view, if rephrased in our language. Ellison is rejecting a simple one-way deterministic model, a model of rigid environmentalism, whether the environment be defined in terms of technology, social structure, or cultural patterns. While listening with his third ear, Ellison is simply offering a countervailing theme that is found repeatedly in sociological thinking, whether it be the expression of George Herbert Mead's interactionalistic school, W. I. Thomas' concern with personality and social organization, or Talcott Parsons' strain toward voluntarism. Patterns of influence in social and personal systems are two-way influence processes, regardless of the degree of imbalance of power and authority.

From such a standpoint, Ellison plunges into a cultural critique of Myrdal's analysis of social change. He rejects Myrdal's standpoint because the latter concludes, "It is to the advantage of American Negroes as individuals and as a group to become assimilated into American culture to acquire the traits held in esteem by the dominant white Americans." He argues in a most contemporary fashion that there is nothing to be gained by an exchange of pathologies between whites and Negroes. In turn, Ellison is seeking to give a deeper sociological meaning to integration for the Negro than a mere concern with the arithmetic of numbers.

As Ellison develops his critique, there is a trace of a conspiracy theory in accounting for the role of sociology in American society as represented in Myrdal's *An American Dilemma.* Ellison sees Robert E. Park "as the cofounder of the University of Chicago School of Sociology." By inference, I assume Burgess was thought to be the other founder, because Ellison was taught sociology in his Negro college via Park and Burgess by a Negro instructor who used the volume to justify his Uncle Tom attitude. The link from Park, Burgess, and the University of Chicago School of Sociology to the Negro community was through Booker T. Washington and Tuskegee Institute, which served as an important device, in Ellison's opinion, for deflecting Negro energy away from direct political action. In Ellison's view, the support that Park and sociology gave to Tuskegee only served to perpetuate a gradualistic approach on the part of American Negroes. It is important for sociologists to recognize that such a view is held about Park by some Negro leaders of opinion. However, it is unfair when Ellison selects passages from Park's work to show the residues of crude characterizations of the Negro that he labels as "not only uncomfortably close to the preachings of Sumner, but to those of Dr. Goebbels as well."

Ellison mentions in particular the oft-quoted characterization of the Negro: "He is, so to speak, the lady among the races." No doubt this phrase depressed and enraged him when he read it and heard it discussed at college. Park regretted it again and again. But that is of little consequence. What is central is that Park, following on W. I. Thomas, was destroying biological racism and was searching for a new vocabulary of intergroup relations. And it is equally unfair to hold sociologists responsible for the misuse of their concepts by those seeking to maintain the status quo.

Likewise, it is undoubtedly true that there was an element of paternalism in the Chicago school's attitude toward Negroes and Negro intellectuals. Yet, it was the intellectual home of E. Franklin Frazier, who emerged as the pioneer and autonomous Negro sociologist; it trained a cadre of second-generation men, such as E. Franklin Edwards and St. Clair Drake, who are so important in Negro intellectual circles. Fundamentally, what Ellison was pointing out, as has come to be recognized, is that the Park-Burgess school, as a final expression of social Darwinism, did not see politics as an independent factor able to influence the destiny of minority groups. Park and Burgess presented an incomplete picture of the urban society, which the latter-day Chicago school sought to correct.

As could be expected, Ellison makes his criticism without full recognition of what he is arguing against. He assumes that Myrdal had a unified theory. A unified theory would

be incompatible with the richness of the volume and its utility as a benchmark. And, likewise, there was no such thing as a "Chicago School of Sociology." From its very origin and during the period of its most intensive activity, the Chicago Department of Sociology presented a wide range of theoretical and policy positions. Only when the second generation came to believe that there was a Chicago school did their energies dissipate and creativity decline.

At this point, it is appropriate for the sociologist to ask Ellison to identify the Negro reality and the Negro pattern of action that is not primarily a secondary reaction to white society. It is necessary to turn the tables, for the sociologist wants some kind of observable reality, not merely the novelist's aspirations for mankind. The whole corpus of Ellison's writings is a remarkable body of empirical data that supplies some direct answers, as well as numerous ambiguities.

As a humanist, Ellison seeks to give meaning to the voluntaristic component in Negro society by concrete demonstration and by the personal document and the life-history approach. How Negroes act is to be seen in the autobiography of Ellison himself. The introductory essay to the volume, "That Same Pain, That Same Pleasure," highlights his personal development and the social setting out of which he matured. Whether Ellison admits it or not, he has taken hold of the sociological interview and uses it for his own purposes. Thus, this essay is a transcript of an interview between himself and the novelist Richard G. Stern in which the question "How were you able to overcome the parochial limitations of most Negro fiction writers?" is posed and probed.

On the personal level, his is a story of how his family assisted him in breaking out of cultural isolation. His mother was central, and he reports on the most specific resources, including the magazines and opera recordings discarded by a family for whom she worked that were important in linking her son to larger cultural traditions. "I never thought that they were not really for me, simply because I happened to be a Negro." On the level of the social setting, he makes much of the fact that he was raised in Oklahoma City—a new state without a slave tradition, where, despite rigid segregation, there was a more fluid pattern of social contacts between the races. In this all too brief, but most effective essay, Ellison presents a case study in the complex and continuous processes of socialization and the specific Negro reality which they encompassed. He presents a document in the sociological tradition of the natural history—with all its unnatural aspects of personality and culture that produced a literary leader.

But, in addition to making the autobiographical testament, Ellison tries to generalize. First, he declares that some of the action of the Negro community has been negative, a negativism that derives not from exclusion by the whites, but from meaningful rejection of the values of the larger society. Second, Ellison is prepared to define some of the essentials of the American Negro. He is not interested in rewriting the history of the American Negro in order to prove the persistence of a rebellious, defiant, and self-assertive tradition—although it is undoubtedly necessary for social historians to reassess the history of slavery. What emerges is a sharper statement of what he sees as positive human values, and the social scientist will recognize it as such. "I speak of the faith, the patience, the humor, the sense of timing, rugged sense of life and the manner of expressing it which all go to define the American Negro." Third, and most profoundly, he implies that the Negro society existed and persisted under slavery, and that to exist and persist under these circumstances was a positive act. The preconditions and consequences of this achievement can be analyzed only when Negroes move into the mainstream of a pluralistic American society, by the manner in which they transform it.

At the present moment the pressure is for Negroes to enter the mainstream of American society as it is currently constituted. To what extent will the deprivations of the past lead Negro leaders to press for changes in the mainstream? These demands, their extent and their nature, will be a powerful measure not only of what segregation has done to the American Negro, but of what Ellison means when he declares that a social group does not exist by merely reacting.

Morris Janowitz

University of Chicago

The Symbolic Uses of Politics. By MURRAY EDELMAN. Urbana: University of Illinois Press, 1964. Pp. 201.

Forty years ago the principal challenge facing political scientists was to establish the point that the political dimension of social process could be productively studied by using the concepts and modes of empirical inquiry common to the psychological and cultural sciences. It was relevant to show, for instance, that those who played agitational or other identifiable roles in political life were employing the specialized symbols and operations of government to accomplish results relevant to the requirements of their personality systems. The manifest content of constitutional charters, legislative statutes, and political proclamations were demonstrated to perform symbolic functions (catharsis) in addition to whatever impact they might have on particular deeds, on changes in the aggregate shaping and sharing of values, or on institutional perspectives and operations.

Professor Edelman's book is evidence of the degree to which the political scientist's map has changed. It is no longer a question of stressing the features of politics common to personality and society. The issues are more refined. Assuming the universality of mechanisms like "role-playing" or "threat and reassurance," what are the relatively distinctive constellations of the various situations that recur in the political process?

Most of the literature concerned with the symbolic dimension of politics has dealt with electoral campaigns, war scares, or similar crises of collective action. Edelman underlines the significance of the day-to-day interaction of public officials with one another and with private individuals or groups, whether organized or unorganized. He has masterfully summarized the image of administrative action that has emerged from modern research. In part:

> The creation of an administrative agency in a political area signals the emergence of a changed relationship beween the groups labelled as adversaries. The agency, the regulated groups, and the ostensible beneficiaries become necessary instruments for each other while continuing to play the role of rivals and combatants [p. 57].
>
> For the unorganized the administrative activity brings a change from the role of potential victims to the role of the protected: ostensible sharers with the regulated industry in the economic benefits together with a powerful showering of symbols suggesting that the new role is secure [p. 58].
>
> Administrative activity is effective in inducing a measure of wide acceptance of all the objectives symbolized by the agencies only because the mass public that does the acceptance is ambivalent about these objectives [p. 61].

The administrative system is put in the wider context of political leadership, the political "settings" of decision, the language of politics, persistence and change in political goals, and mass response. At every point comments or hypotheses are put forward that will undoubtedly focus future research. For example:

> While elites are more ambivalent about statutory language, the lay public is apparently more ambivalent about the administrative style [p. 144].
>
> The disparity between the connotations of each style for the mass public and for direct participants grows smaller as one moves from the hortatory style to the bargaining style [p. 149].
>
> The intensity of an interest in a particular political objective is lessened in the degree that there is (1) constitutional, statutory, or administrative action dedicating "the state" to achieving the objective, and (2) frequently renewed ritualistic assertion, overt and explicit, that the objective is being achieved [p. 164].

Edelman's penetrating book cannot fail to inspire research planned to make the "symbol-deed" dimension of politics more explicit and intelligible. Since every political act can be described as a symbol-deed ratio, the ratios may vary from act to act and from one decision phase to another. Recent researches do not distinguish clearly enough between "symbolic" and overt ("factual") exposures. Nor do they make clear the value expectations that account for the appearance of relatively overt or relatively symbolic responses.

The present volume helps to remove ambiguities that have stood in the path of researchers, who may at last discover "systematic patterns in departures from administrative rationality" (p. 68). And, one may add, from all forms of political and social rationality.

HAROLD D. LASSWELL

Yale University

The Politics of the Budgetary Process. By AARON WILDAVSKY. Boston: Little, Brown & Co., 1964. Pp. xi+216. $4.00.

This book is a report of the political process in which congressional leaders, appropriations committees, Bureau of the Budget staff, and department or agency budget staff interact and finally produce the national budget. The budget is the mechanism used by government for making choices among alternatives: the author's purpose is to investigate the political processes that are involved in this highly political process.

This book claims and appears to be rather different from other types of scholarly appraisal of the budget. Many are simply criticisms of the methods used by special interest groups (private or government) or particular congressmen to obtain appropriations, while others study the conditions under which national budgets for particular programs expand or contract. This book considers this literature and dismisses much of the more value-loaded criticism and reform suggestions on grounds that too little is yet known of the consequences of traditional practice to risk radical change.

The primary contribution, and it will probably be the reason why many of the actual participants in the budget game will buy and read this book, is the sensitive codification of the preferred strategies and consequences of a wide variety of situations that were reported to the investigator by some 160 participants. Many of the interviews were with perceptive and articulate civil servants and politicians. In drawing together their observations on the politics of budgeting the author performs a useful service. Transcripts of appropriation hearings covering twenty-five different agencies concerned with foreign and domestic policy from 1946–60 were reviewed but seem neglected in the analysis. Data on the number of Bureau of the Budget actions on agency budget are reported.

In common with much good reporting and qualitative evaluation of politics this book reminds us of the richness of human response. Wildavsky has studied a rich and complex subject but he has yet to achieve the conceptual order that would precede qualitative analysis and prediction of specific outcomes. His book should, however, provide a useful base for this work.

PHILLIPS CUTRIGHT

Social Security Administration

Presidential Elections: Strategies of American Electoral Politics. By NELSON W. POLSBY and AARON WILDAVSKY. New York: Charles Scribner's Sons, 1964. Pp. xii+218. $3.95.

As their subtitle implies, the authors have approached their subject with a special concern for the strategic factors that operate in presidential nominations and elections. This provides a useful matrix in which to assemble the principal research findings of the last fifteen years. The minutiae of party rules and the idiosyncrasies of presidential primaries may also seem less dull when viewed in terms of the strategic problems they produce for candidates and other participants.

One-fifth of the book is devoted to analysis and rebuttal of various proposals for reforming the party system. Here the authors purport to be addressing themselves mainly to the 1950 Report of the Committee on Political Parties. For this reviewer, who served on the Committee throughout its life, this part of the book is not entirely persuasive in either its factual assessments or its reasoned conclusions. The authors certainly neglect the impact of the Committee's message on the state politics of such states as Michigan and California.

The most rapid road to genuine party reform in this country would probably involve strengthening the minority party in the South by means similar to those through which the minority party was strengthened in Maine, Vermont, Pennsylvania, Michigan, Oregon, and California. This is not irrelevant to presidential elections or to the customary practices of Congress, but it is not discussed by the authors of the present book.

PAUL T. DAVID

University of Virginia

The Nerves of Government: Models of Political Communication and Control. By KARL W. DEUTSCH. New York: Free Press of Glencoe, 1963. Pp. xviii+316. $6.50.

The State Department and the guidance system of an anti-aircraft missile are both examples of machines that select information from their environment and transmit it over predetermined channels, screen the data for relevance, compare it to data stored in memory circuits, arrive at a decision, transmit instructions for action, and then wait to see whether the action has taken them closer to or farther from their goal. The first example is of an exceedingly complex apparatus that is poorly understood. The second is a simple piece of machinery that we understand very well. What can we learn by comparing the two? *The Nerves of Government* presents some tentative answers, and its author is frankly enthusiastic about the prospects for this approach to political systems.

The first product of the analogy is a broad theoretical viewpoint which stresses knowledge, information, communication, and rational behavior, and which redefines and assigns a minor role to words like "power," "culture," "personality." Deutsch sees this viewpoint as a foundation for a new type of theory that will overcome some of the difficulties inherent in older theories whose roots lie in analogies with mechanical or organic processes. The theory draws most heavily upon Norbert Wiener's *Cybernetics*, but with an input from the mathematical model-builders, particularly game theorists, and from the writings of Parsons.

The fundamental soundness of the approach seems obvious. Communication comes in measurable units, can be easily described and understood, and is fundamental to the decision process. Furthermore, it tends naturally toward analysis of a system in terms of components and subassemblies, and struggles consciously with determining the proper unit of analyis. It immediately generates ideas about comparing countries by content analysis of internal conversation, about looking at feedback circuits, at control over communication channels, or at their capacity.

It is harder to suggest where the boundaries of this approach are, or what its weaknesses will be. One reason for this is that the outline presented here is quite vague. No serious effort is made to analyze any body of data with it, and when the theory touches the hard reality of politics, the result may be anything from a perceptive comment on the importance of colonels in military coups to a seemingly facetious reference to high-pressure salesmen as being obnoxious because they overload one's information circuits. An elaborate flow chart for foreign-policy decision-making, with forty-five channels connecting fifteen components, shows promise, but it is placed in an appendix with little development.

However useful this approach will be in the long run, the short-run impact of this book will be limited by its failure to descend from its high level of abstraction. The reader who is willing to struggle with definitions of will, power, consciousness, feedback, and equilibrium may be less than pleased to find that he must then go on to definitions of humility, pride, lukewarmness, faith, love, grace, and spirit. The reader may also be disappointed by the references to Parsons and game theorists, for their work has been exciting in the abstract and disappointing in the flesh.

Thus is seems likely that most readers will postpone making use of a cybernetics approach to politics until after Deutsch has shown the way by using the model in his own research.

ROBERT L. CRAIN

University of Chicago

Class Structure in the Social Consciousness. By STANISLAW OSSOWSKI. New York: Free Press of Glencoe, 1963. Pp. 202. $5.00.

This book, translated by Sheila Patterson from the Polish original published in 1957, presents a systematic analysis of the different ways of interpreting class structure. Based on a survey of viewpoints ranging from Aristotle to contemporary American and Soviet writers, Ossowski begins by outlining three basic ways of perceiving class structure: the dichotomic scheme, the functional scheme, and the scheme of gradation. The first two focus on relations of (one-sided or mutual) dependence and see classes distinguished by opposite or different attributes. The scheme of gradation refers to an ordering relation based on the degree of possession of the same attribute. In the persistence over thousands of years of these ways of interpreting class structure Ossowski finds enduring social ideals. For instance, the scheme of gradation em-

phasizes the perennial conflict of social reality with the ideal of equality. In a separate chapter devoted to Marxian class analysis its contribution is found in a new mode of perceiving class structure, based on the intersection of three dichotomic divisions. In a subsequent chapter the American and Soviet conceptions of non-egalitarian classlessness are compared.

In the second part of the book Ossowski compares the different meanings of the term "class." He distinguishes its most general usage—class as basic group in the social structure—from several more specific meanings which limit the term to a particular type of such a basic group. The conceptual differences are not merely due to discrepant terminological conventions, but reflect different assumptions about reality, that is, about what distinguishes the basic social groups predominantly. The choice of one particular conceptual scheme also reflects practical interests. Thus, for revolutionary groups the dichotomic scheme can serve an important ideological function, while those who defend an existing social order are inclined to present their society in terms of a functional scheme or one of non-egalitarian classlessness.

Ossowski holds the different ways of perceiving class structure not to be mutually exclusive, but he does not maintain that they are equally applicable in particular historical instances. Thus in our century the classic Marxist-Leninist conception is held to be inadequate for analyzing the social structure both of modern socialist and capitalist countries. The main reason for this is seen in the ability of political authorities to change the class structure, which decreases the independent effect of economic relations and increases the importance of the dimension of political power in class analysis.

This undogmatic, well-informed, and admirably lucid study shows how fruitful an exposure to both Western and Marxist social theory can become, provided their meeting ground is a mind of such superior stature and independence as that of Stanislaw Ossowski.

RENATE MAYNTZ

Free University, Berlin

Der Spätbürger ("The Bourgeois"). By EMIL MAURER. Bern: Francke Verlag, 1963. Pp. 330.

This is a late comer among the books on the crisis of our time. The author has found a culprit who is in the main responsible for the decay of Western civilization, the seemingly inescapable loss of individual freedom and the true personality. It is the *Spätbürger*, the bourgeois of the time between roughly 1860 and 1920 who preferred comfortable living, money, and the power it gives to the life of daring, vision, and responsibility that marked the earlier bourgeois. The *Spätbürger* by now is passé. His heir, but not his kin, is the *Kleiner Mann*, the common man. Neither able nor willing to recoup personal initiative and risk, this type is headed for an anonymous existence in an insect state society. As simple as that.

Mr. Maurer is well read and not without perception, but his book does not really contain anything new or original. It is much less well organized and balanced than earlier books on the topic both here and in Europe. Clichés abound, and the surface is seldom really pierced for a deeper probe of the currents that led to the present critical stage in the development of our civilization. The author's vision and his judgment are clouded by his strong emotional involvement: nostalgic longing for the "good old society" of post-Napoleonic days and a strong contempt for the *Spätbürger* and especially for that despicable upstart, the common man. Modern technology and the problems it creates are also approached rather negatively, though the author himself, as director of the Munich Department of Fairs and Exhibitions, might be expected to develop a different attitude. It is interesting to note that the author, who has witnessed in his lifetime the rise and fall of Mussolini and Hitler, should deal with the former only very briefly, with the latter not at all. Russia and Communism are treated in some detail, as he considers both to be arch enemies of our civilization.

WERNER A. BOHNSTEDT

Michigan State University

Amish Society. By JOHN A. HOSTETLER. Baltimore: Johns Hopkins Press, 1963. Pp. 437. $6.50.

John A. Hostetler's *Amish Society* is an authoritative, well-documented, and highly

readable account of the sect's social history and customs in America.

The Amish migrated to the United States from southern Germany in the eighteenth century. They have prospered in an urbanized and secular America, but have disappeared from their area of origin in Europe. Along with other Anabaptist sects, the Amish provide colorful textbook illustrations for many sociological concepts—*Gemeinschaft*, community and cultural diversity. They swim against the main stream of the twentieth century.

The book describes a culture which is being transmitted within a strange and hostile environment. But it does not ask many theoretical questions. One reads of assimilative trends, but there is no attempt to analyze what differentiates the Amish who transmit their traditions from those who break away. What distinguishes the Amish from other separatist sects in America that have disappeared? Why did the Amish survive in America, whereas in Europe only their gravestones remain? The book was not intended to answer such questions, but it provides data which raise them.

Persecution, when carried on energetically, may wipe out the persecuted group. But when it is inefficient and spasmodic, it has been a stabilizing influence on minority groups. The Amish sacred book, *The Bloody Theater* or *Martyrs' Mirror*, is largely an account of the many Amish who were condemned to death for their steadfast faith. The persecution was so severe that the faithful died, converted, or fled to America. Persecution made the Amish flee Europe. In the United States they prospered in spite of it.

Another factor credited with supporting the survival of the Amish is their primary-group way of life. Church districts are kept small enough for face-to-face interaction. Most of American rural society once lived this way, only to go the way of urbanization. The book fails to deal explicitly with Amish immunity to this trend.

The Amish have common religious principles, but are more than a reliigon. What distinguishes them from other Anabaptist sects is not a matter of fundamental principle but of historical identity. No efforts at conversion by Amish are mentioned. One must be born Amish. They are an ethnic as well as a religious group.

The Amish are "other world" minded in contrast to most Christian churches which want to make the world a better place in which to live. The Amish interpretation of salvation also differs in emphasis from much of modern fundamentalism. Belief in predestination is taboo, as is the idea of an assured salvation. The Amish show little interest in improving their environment. They profess "to be strangers and pilgrims" in the present world.

The Amish stress obedience. Failure to obey is likely to result in *Bann* and *Meidung* (excommunication). This doctrine was intrinsic in the entire Anabaptist movement from the very beginning. Among the Amish today it continues to be a major principle and the basis of many of the splits that have occurred in the sect.

There are other elements, such as mutual aid, the cohesive family, and the closeness of interactions within each area, that may help to explain the remarkable continuity of Amish tradition in spite of competition from the outside. But there are among the Amish indications of breakdown. The book briefly refers to a few of them but unintentionally exaggerates the group's stability by excluding from analysis those persons of Amish origin who left the fold. There are new Amish who are more worldly than the Old Order.

JOSEPH W. EATON

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Our Changing Rural Society: Perspectives and Trends. Edited by JAMES COPP. Ames: Iowa State University Press, 1964. Pp. x+354. $4.95.

This is a symposium of invited papers presented at the 1961 annual meeting of the Rural Sociological Society. Robin Williams opens it with a general essay on American society in transition. Olaf Larson and Everett Rogers give some aspects of the rural side of recent change; Donald Bogue and Calvin Beale give a demographic analysis. Christopher Sower and Paul Miller analyze the recent overt social and political influence of the farmer class; Lee Burchinal writes on the rural family; and Thomas Ford and Willis Sutton discuss the changing community. In

the next part George Beal discusses social action; Joe Bohlen, the adoption and diffusion of new ideas in agriculture; Robert Bealer and Frederick Fliegel, the relation of "social change" to rural sociology; and Irwin Sanders, community development programs. James Copp ends the book with an essay on "whither rural sociology." Does rural sociology have a future in capitalistic agriculture and if so, what? The topic is very important. The book will be studied wherever rural sociology receives academic consideration. The group should be congratulated.

Williams (p. 3) suggests that recently social change has not been a reputable topic for research. This is partly true because of the domination of the field by political academicians, who have perverted the subject away from a simple search for truth. It also explains in considerable part the metaphysical amateurishness strikingly displayed in some chapters in this work. Larson and Rogers recognize only transeunt causation (pp. 39–40). Hence their later analysis makes all their facts "independent" symptoms of maladjustment instead of part and parcel of a larger system of change. Bealer and Fliegel (pp. 288–301) get lost in words and finally come out where they started.

Several chapters are unusual, particularly those by Bogue and Beale and by Sanders. The demographic chapter raises demography from arithmetic to thought. The analysis by Copp on the future gets partly away from the mental constipation which considers rural sociology only a study of farmers and the limited group that are so defined by census limitations. According to this common conception a place like rural New Hampshire, where commercial farmers are practically nonexistent, is not a "reputable topic for research." This, of course, violates the dictum of Pope that the "proper study of mankind is man." The general skew distribution of rural wealth is such the world over that, under this American rule, less than 20 per cent of villagers and peasants could be classified as proper subjects for rural sociology. Even in the United States there are 60 to 70 million rural people only about a sixth of whom meet the occupational classication of farmers.

CARLE C. ZIMMERMAN

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The Black Jews of Harlem. By HOWARD BROTZ. New York: Free Press of Glencoe, 1964. Pp. 144. $4.50.

The Commandment Keepers Congregation of the Living God, with a thousand members, is the largest among a handful of tiny sects known as the Black Jews of Harlem. The subject of a brief, thoughtful book by Brotz of Smith College, the Commandment Keepers believe themselves "the lost sheep of Israel," descended from the Ethiopian Hebrews and linked today to the Falasha Jews of Ethiopia who practice an archaic, syncretized Judaism. Like the Black Muslims these people reject white society with its broken promise of equality of opportunity, as they reject the white man's word for them, "Negro," fastened upon them in slavery and still connoting a people without history or pride in culture. However, where the Muslims have turned to Islam in a rather haphazard and negative sense—Islam means *not* to be white or Christian—the Commandment Keepers are content to express their rejection of the white (and Gentile) world in religious solidarity and ritual practice. Where Muslim nationalism is keyed to militancy, expanding organization, and even the vague dream of attaining counterpower through creation of a separate state, the Commandment Keepers, guided by their own Rabbi, Wentworth A. Mathew, are absorbed in reconstructing their "true" history and in following faithfully, if incompletely, the ritual of the Jews: Friday and Saturday services, the study of Hebrew, obedience to the taboo on unkosher foods, and participation in the Passover *Seder*, a ceremony with obvious double symbolism for black men who are also Jews.

Brotz's account proceeds on two levels, the first very competently written, the second puzzling and disappointing. The first half of this short book describes objectively yet sympathetically the outlook and life of the sect, including the text of the Rabbi's "Anthropology of the Ethiopian Hebrews and Their Relationship to the Fairer Jews." Brotz shows with particular insight that the Black Jews represent not only self-emancipation and dissociation from the white community but also an attempt at separation from the social disorganization of secular Harlem and the erratic holiness cults of spiritual Harlem, both of which seem to the Commandment Keepers to

contribute to the white man's stereotype of the American Negro as lost, drifting, childlike. ("Niggerations" is the Rabbi's term, not the white man's, for letting go in a religious service. And since the core of the Rabbi's congregation is West Indian, there is a hint of contempt for the Negro no longer in the South but still without roots.) The Commandment Keepers accomplish this separation within Harlem by pointing to the sobriety of their ritual, their strict morality, and the absence of juvenile delinquency among them.

My sole complaint about the descriptive section is that Brotz provides not a word on the method of his participant observation of the sect. Granted that sociology often suffers from a far worse sin, a pretentious "methodological schema" to no end, yet here the minimal effort has not been made to define the circle of real, over against casual, informants, nor are there any clues to the relationship between the observed and the observer who is, after all, white and, I imagine, a Jew himself. This omission would certainly not weigh heavily against the virtues of the first section had not the author, in the remaining seventy pages, embarked on a scrutiny of the entire history of Negro leadership, protest, and accommodation. This is a laudable intention. Moreover, it yields useful generalizations and observations—together with some dubious historical reinterpretation, such as a decided overcompensation in criticizing the critics of Booker T. Washington. But in the many comments on Washington, DuBois, Myrdal, and others, both the Black Jews and what I would take to be the essential question—the function of sect religion in the Negro community, Judaic and Christian, in simultaneously limiting and liberating the Negro in the ghetto—seem to sink in a sea of reflections. Characteristic is a random, stray sentence on James Baldwin with no mention of his early and basic commitment to sect religion. Nevertheless, *The Black Jews* remains rewarding for its first half; the sect has found a sociologist of ability and sensibility.

RICHARD ROBBINS

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Dark Strangers: A Sociological Study of the Absorption of a Recent West Indian Migrant Group in Brixton, South London. By SHEILA PATTERSON. Bloomington: Indiana University Press, 1964. Pp. xvi+470. $9.00.

The recent increase in popularity and academic respectability of sociology and social anthropology in Britain has coincided with an influx of colored migrants. Hence much research has been done on Africans, West Indians, and Asians in various locations. Mrs. Patterson, who previously studied the "Cape Coloured" in South Africa and postwar Polish immigrants in Canada, here reports on West Indians living in a deteriorating section of London.

West Indians have come lately to Brixton. There were virtually none until 1948; by 1960 there were 9,000 or 10,000. They have found the same sorts of jobs as other immigrants. Like the others they have been welcomed cautiously in time of labor shortage by employers and regarded with hostility and suspicion by fellow workers, and have slowly gained acceptance and begun to climb the job ladder. They have found accommodations with difficulty, and sifted themselves out into streets of shifty, anonymous roomers and streets of stable, respectable households. Some have moved up and away, although dispersal is especially hard for the colored. Social contacts with whites have been casual and categoric rather than associational or informal, reluctance to enter recurrent or intimate relations being shared by newcomers and hosts.

Within the West Indian group, unstable and matricentral family units outnumber legalized patricentral and equalitarian families; unwanted children present a problem to social agencies. Religious associations have little place, except for a few Pentecostal groups There are not even many gangs centering around illegal activities. The recency of settlement, the viewing of Brixton as a temporary abode, and lower-class norms have prevented the growth of community structure.

Mrs. Patterson believes that the West Indians will in time be assimilated, as Irish migrants have been, and offers suggestions for hastening the process.

A theoretical discussion and a statement about field-work methods precede the description of West Indians in Brixton. The core of the first consists of assertions that the West Indians are better understood in the context of immigration than of race relations and that

only accommodation and not assimilation or integration is occurring. Both assertions are plausible; however, they are not tested as hypotheses. The report on field-work techniques tells why a person trained in anthropology and belonging to a department of social anthropology labels her work sociological: she considers complex literate societies and techniques beyond direct observation and the interview to be taboo for anthropologists. This is unduly restrictive. The characteristics of *Dark Strangers* are more anthropological than sociological: a wealth of detail, at times tedious and occasionally repetitive; greater sureness in handling the internal structure of the West Indian group, particularly its kinship relations, than relations between groups; and limitations to a small area, so that what transcends the area or is hard to localize eludes discussion.

One defect is probably personal rather than vocational: garrulity. Perhaps it is another expression of the vigor which carried the author through two and a half years of field work and two and a half more of observation and writing. If so, one must mingle admiration and gratitude with regret that there was not enough energy left over for a thorough pruning of the report.

JEAN BURNET

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Ethnosoziologie sowjetischer Völker ("Ethno-Sociology of Soviet Peoples"). By ZVI RUDY. Bern: Francke Verlag, 1962. Pp. 244. Swiss fr. 24.50.

Soviet ethnology is what Soviet ethnology does, and Professor Rudy introduces the non-reader of Russian to the subject by summarizing four postwar contributions. Soviet ethnologists have been prolific. They write as much as do their brethren in the land of "publish or perish." So the choice is somewhat arbitrary. Yet by means of his choice, the author succeeds well in illustrating the dominant features of the discipline as it is today.

In each of the four summarized writers one finds a basic devotion to descriptive data. E. M. Schilling's *Kubatschinzyi ich kultura, istoriko-etnografitscheskie etjudy* (1949), for example, includes detailed documentation of various aspects of the material culture of the Kubatshin, especially of their tools, techniques, and achievements of artistic metalwork. The tendency for such description to be better for older than for more recent times is apparent throughout. M. A. Sergejew's *Nekapitalistitscheski put razwitija malych narodow sewera* (1955), a highly useful and authoritative survey of the culture history of twenty-six small ethnic groups in Siberia, is notably more comprehensive for older than for later periods. Ecological limitations which make economic development beyond hunting, fishing, and reindeer-herding extremely difficult also make Siberia an important laboratory for the study of ways of life now rare in other parts of the globe. Interest in this ethnic laboratory is intensified for the Soviet ethnologist, who is concerned to document examples of Marxist-conceived stages of evolutionary progress, including that of primitive communism. Marxists working in Siberia are especially intrigued with co-operative hunting, fishing, and herding practices and not the least with the tradition of sharing game with band members who do not participate in the hunt. Sergejew's research adds much to older reports, although Rudy here summarizes only two of the subjects dealt with, socioeconomic conditions and religious practices. A. F. Anissimow's *Religija Ewenkow* (1958) contributes to the documentation on primitive belief, shamanism, and the bear cult of one of the largest Siberian societies, the Tungus (Evenki).

Soviet ethnological theory is limited to concepts of cultural history or cultural evolution. Marxist in conception, such theory is basically no more than a particular version of nineteenth-century evolutionary thinking that is itself doctrinaire rather than challenging. A unilinear sequence of evolutionary stages is postulated, and the illustration of these stages from ethnographic accounts is the primary object of research. At best, this can be congenial with feasible historical reconstruction. T. A. Trofimowa's *Etnogenes Tatar Povoložjia w swetje dannych antropologii* (1949) describes the craniological characteristics of the Volga-Tatars and compares them to characteristics of the Golden Horde. Using also archeological and linguistic evidence not summarized here, Mrs. Trofimowa concludes reasonably, as have others before her, that race, speech, and nationality vary independently and that the role of folk migrations in racial change should

not be overly stressed. In short, the Mongolian Horde in Europe was not as Mongolian and not as much of a horde as is generally thought.

But orientation to simplistic evolutionary theory can vitiate otherwise promising work. Schilling's excellent treatment of material culture is joined to an analysis of social structure that is disappointing. He describes societies of unmarried men characterized by the use of drums, face-masks, and temporary men's houses. As simple description of a variant on age grades known to the traditional cultures of other parts of Europe, this has considerable interest. Contextual reference, however, is to the supposedly predictable association of men's societies with matriarchy. The prediction is ultimately based upon the old argument that at a certain stage in the evolution of society men form secret societies to counter the overt power of their womenfolk. It has no better support than this, and runs against the observation that tribal societies are far more common than matriarchy. The original proposition, in fact, grew out of a confusion of matriarchy with a quite different phenomenon, matrilineality. This kind of thinking, long discarded in most places, survives here to support a misleading emphasis upon matriarchy among the Kubatshin as reconstructed through specious inferences from a small number of items such as the old custom whereby female fieldhands may capture any stray man and ransom him for his small change. Conversely, other aspects of the social structure are left neglected or unmentioned.

In sum, Soviet ethnology—or ethnosociology as Rudy prefers to call it—offers a large ethnographic inventory with diachronic dimensions. This inventory tends to be very good for material culture, but rather spotty and distorted for social structure.

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Biennial Review of Anthropology 1963. Edited by Bernard J. Siegel. Stanford, Calif.: Stanford University Press, 1963. Pp. 314. $7.50.

The eight authors of the 1963 *Biennial Review* have maintained the distinguished level of the two earlier editions through their synopses of developments in a notoriously pluralistic field: linguistic anthropology (William Bright), psychological anthropology (Robert A. LeVine), biological anthropology (F. S. Hulse and N. P. Lamb), and three reviews of social and cultural anthropology (e.g., "Cultural Change," by Fred Voget). The conspicuous absence of a general treatment of archeological method and theory is more than compensated for by Maria Gimbutas' "European Prehistory: Neolithic to the Iron Age." And as in Easton's superb "Political Anthropology" in the 1959 edition, Norman A. Scotch's "Medical Anthropology" here brings the reader up to date on an important but comparatively specialized subfield. All of the contributions display a catholic awareness of recent research, but they range considerably from sequences of familiar rubrics, with all the references made, to internally integrated discussions that stand on their own as important theoretical summations.

A biennial review naturally suggests the dominant or new directions in a field—toward genetic models in physical anthropology, toward mathematical linguistics, toward large, specialized teams in archeology, and toward a fixation of the disillusionment with even neo-Freudian psychology. Much social anthropology is currently concerned not with primitives but with peasants, as sketched in Ernestine Friedl's "Studies in Peasant Life." Some typical concerns are, for example, the definition of "peasant": what is the role of dyadic contracts and of self-sufficient households in such systems? Or, when inferring the attitudes of peasants, are we to be satisfied with the patterns of conventional rivalries, as reflected in the gossip, or only with the more abstract "qualities of interpersonal relations"? Is peasant behavior to be seen as generated from logical premises that can be rigidly formulated, or does it emerge primarily from the interaction of conflicting and emotion-laden attitudes? How is the analysis and even the classification of "peasants" affected by the demonstrably fruitful distinction between society and culture? Finally, what is the relative value of internal differentiation as against external criteria when it comes to characterizing peasantries and, in particular, distinguishing them from primitives? The article gives intimations of a certain scholasticism in the

peasantologist's quest for adequate and universal definitions, which turn out to be based on one or two cultures, or championed without reference to larger theoretical goals.

The 1963 *Review* also demonstrates that much of the best field work and finely chiseled reasoning in anthropology is still related to social organization. William Davenport's brilliant article summarizes a spate of recent contributions under the headings of "Descent Groups," "Marriage and Affinity," "Kinship," "Politics and Government," "Complex Society," and "Comparative Method." En route he distils with elegance a much larger current of analytical distinctions that have been elaborated during the past decade, emphasizing in a challenging way the increased distance of such anthropological studies from sociology and statistics, and their growing interpenetration with linguistics, logic, and the psychology of cognition. For such reasons the chapter is cordially recommended to the sociologist with conceptual *Wanderlust* and a cultivated sense of laissez faire.

PAUL FRIEDRICH

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Horizons of Anthropology. Edited by SOL TAX. Chicago: Aldine Publishing Co., 1964. Pp. 288. $5.00.

As the title and editorship of this volume might lead some into believing it an updated version of *Anthropology Today,* we must explain that it is instead a series of papers which were initially prepared for delivery in a forum broadcast on the Voice of America. Acting in his role as the Diaghilev of anthropology, Tax assembled a team of nineteen youthful scholars and, then, in publishing their lectures, invoked the title "horizons," because of their age and the focus of the forum on "current American thought."

That a collection of half-hour talks addressed to a lay audience should also prove valuable to an audience of colleagues is expecting a great deal. Here, it is significant that the poorer articles suffer, not from oversimplification or academic condescension, but from too strong an orientation toward that which is "current" and "American." A few speakers even seem to imply that their subdiscipline was a recent invention within the United States and had no scholarly ancestors elsewhere. Conversely, those scholars who break out of the constraint of the "current American" mold and just write about recent anthropological research produce by far the more informative and original articles. (As chief architect of programs of co-operation among the anthropologists of the world, Tax seems uncomfortably aware of the bias of some of the essays; in any case, he devotes his own introductory essay to the international character of the early development of anthropology. Also, he seems aware that using "American" as an adjectival form of "United States" is not only inexact but inappropriate to international communication, although some of the contributors appear less sensible of the geographic imperialism of this usage.)

Our guess is that the book will have its chief use as an auxiliary to instructors or graduate students who wish a convenient compendium of recent anthropological findings and formulations in order to supplement standard texts. Of the essays, we found almost half to be excellent and many warrant a discussion for which we have no space here. Steven Polgar presents a schematic outline of how the illnesses that plague mankind have evolved with human culture and technology. Frank B. Livingstone restates the problem of the genetic variability of human populations using an ecological framework. Irven DeVore summarizes his and Washburn's observations of baboon colonies in African game parks. Manning Nash lucidly outlines a scheme of dimensions for comparing economic systems. Paul J. Bohannon assails the errors of applying the concepts of traditional jurisprudence outside the Western states and proposes an alternative system for a legal anthropology. Marshall D. Sahlins plays the *enfant terrible* as he attacks both popular and scholarly opinions about cultures, their integration, and their relationship to their social and natural environments. An ecological theme is likewise present in the essays of Robert M. Adams and Eric R. Wolf, so that it might well be said that the horizon of this volume, or of many of its better essays, is a new vision of culture and environment. Of the remaining contributors, we have space merely to note that they do not show here at their best.

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